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THE

AMERICAN

# JOURNAL OF OBSTETRICS

AND

DISEASES OF WOMEN AND CHILDREN.

EDITED BY

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ORIGINAL COMMUNICATIONS.

---

ON COMPRESSION OF THE FETAL HEAD BY THE FORCEPS  
AND CEPHALOTRIBE.

---

By HUGH L. HODGE, M.D., Philadelphia.

---

PHILADELPHIA, *November 5, 1874.*

GENTLEMEN OF THE PHILADELPHIA OBSTETRICAL SOCIETY:

In a letter addressed to this Society, dated November 6, 1872, my father promised to prepare a paper upon the compressibility of the foetal head by the forceps, and upon the great value of cephalotripsy and that form of cephalotribe which would render it most useful as a compressor, and also as a tractor.

He at once began the preparation of this paper, and continued to work upon it until within two days of his death, in February, 1873. Among his final directions, in the last hour of his life, he told me to take charge of it and complete it. This I have endeavored to do in his own words, by extracts from what he had already written and published on the subject. The article, prepared in this manner, is, I think, of more value than it could have been made in any other way, because it contains nothing that is not his own thought expressed in his own words. But it is only a fragment of what it would have been if he had lived.

Respectfully yours,

H. LENOX HODGE.

"One the most dangerous errors relative to the forceps that a student could take up, would be the opinion that the forceps is, in its very design, a compressive instrument. It is not so; the forceps is not a pincers, it is an extractor—it is a real tire-tête; and I think it ought to be established as a principle in obstetrics, that where there is not space enough for the descent of the head without the forceps there cannot be produced a due proportion by merely squeezing the head down to the required dimensions by such an instrument." (Meigs' Treatise on Obstetrics, 4th edition, p. 528.)

This was the earnest teaching, both written and oral, of my excellent friend, the late Chas. D. Meigs, M.D., Professor of Obstetrics in Jefferson College. It is not his alone, but seems to be the universal opinion of British practitioners since the time of Smellie, if we can form an opinion from the size and dimensions of the various forceps which have been, and still are, in use in the British domains. Very slight compression can be made upon the head even when the handles are brought in contact. Obstetricians are afraid to trust the novitiate, or even themselves, in the use of an instrument capable of diminishing any of the diameters of the head. Many of the practitioners of our own country practically adopted the same opinion by employing the English forceps with some slight modifications.

The fear of undue compression influenced the mind of Dr. Foster, who, in 1781, recommended "a stop, or kind of nail, to pass from one handle to the other at their extremities," for the "excellent purpose of preventing too much pressure upon the head of the child," etc. ("Principles and Practice of Midwifery," by Edward Foster, M.D. Lond., 1781). It is singular with what pertinacity this idea has been maintained among the English-speaking people, for we find that the late Prof. Elliott, of New York, recommends "a sliding pivot" on his modified forceps, not being willing to trust his own prudence and judgment in the use of his own instrument.

This opinion as to the impropriety of compression, sanctioned by some of the best obstetric authorities, is, I believe, fundamentally erroneous, and has greatly restricted the practical use of the most invaluable of all obstetric instruments, the forceps. It is not surprising, therefore, that Prof. Barnes, as late as 1870, affirms that the English forceps "has declined" since the time of Denman and Smellie, and that the highest compressing power



of this instrument has not been developed. "Indeed the present forceps are not much better than Chamberlen's."

My observations upon this important subject will be best arranged under distinct propositions.

FIRST.—*The vault of the fetal cranium is so constructed as to render it capable of compression.*

This is well known and acknowledged. It depends not merely on the interosseous spaces termed fontanelles, but also, upon the arrangement of the commissures of the cranium, improperly called sutures. The edges of the several bones are not serrated, but are simply in contact, and are connected by a fibro-cartilaginous tissue possessed of some elasticity, and at the same time very strong. The bi-parietal commissure is extended forwards between the two portions of the os frontis to the nose, and is usually termed bi-frontal. There is often, also, to be observed, a transverse fissure across the base of the occiput, behind the foramen magnum, and may be termed the bi-occipital commissure. It is evident, therefore, that a very large proportion of the head—indeed, all that part above a line drawn from the base of the occiput over the external meatus auditorius to the orbital ridges—is capable of great diminution in size, simply from its anatomical arrangements.

SECOND.—*Compression does actually occur in Labor.*

This also is acknowledged by the most sceptical. It is observed in every case of labor to a greater or less degree where the head meets with any resistance in its transit, whether from the rigidity of the muscular and other tissues or the disproportion between the head and pelvic canal. The alteration of the form of the head varies according to the presentation and other circumstances. In the usual vertex presentation the cranium is diminished in its lateral direction, and increased in its length. The first is evidently owing, as observation proves, to the riding of one parietal bone over the other, and also of one portion of the os frontis over its fellow. The elongation of the occipito-mental diameter, which, Dr. Barnes affirms, may be to the extent of one inch or more, is not so easily explained. Some slight motion may possibly exist at the connections of the os frontis, and the malar and nasal bones may allow of some recession of

the os frontis. Still more may be gained by the backward projection of the occipital bone, permitted by the bi-occipital commissure. These, however, do not appear to be sufficient to explain the elongation of the occipito-mental diameter from five to six and six and a-half inches, as has been alleged. I suspect, however, that the elongation of the head is more apparent than real, owing to the tumefaction of the scalp from pressure, effusions, etc. As to the lateral diminution of the head, it is usually stated from three to six lines. Bandelocque's well-known experiments upon heads of stillborn children would limit it to three or four lines; but, as well-developed fœtuses have been born alive, where the conjugate diameter of the brim has not measured more than three inches, we have a right to infer that the bi-parietal diameter would be lessened to six or eight lines. Much depends upon the degree of ossification of the head, and the peculiar circumstances of the labor.

THIRD.—*Compression of a fœtal head at term can be effected to a great extent with safety to the child.*

It is impossible to determine *à priori* what are the safe limits. Much depends on the compressibility of the cranium. Much upon the degree of pressure applied, much upon the intermittent character of the pressure, and much on the time which is expended. Hence, children are often born alive under apparently the most unfavorable circumstances, and sometimes they are stillborn, when great hope is entertained of their safety. The experience of the profession has indicated that children at term may be born with safety by the natural efforts of the mother, when the antero-posterior diameter of the superior strait measures three inches, under which circumstances, efforts should always be made for its preservation. If the diameter be less than three inches, no reasonable hope can be entertained, unless the head be imperfectly ossified, as in premature children.

FOURTH.—*The death of the fœtus during labor from pressure, results from two causes—1st, the diminution or suspension of the functions of placenta and funis, and 2d, from the cessation of the circulation generally.*

This last is often a sequel of the first, but as it is accompanied with different phenomena, should be considered separately.

It is usually asserted that the death of the infant is owing to pressure upon the brain, *per se*, as the primary and essential cause. This cannot be admitted, for it is well known that the life of the fœtus in utero is almost entirely organic or vegetable. The brain, so all-important as the centre of the nervous system after birth, is really of no value to the fœtus. Its organic actions, its growth and nutrition can be carried on as well without as with the brain. This is fully demonstrated in the case of an anencephalus or an acephalus fœtus. There is in the Obstetric Museum of the University of Pennsylvania, a fœtus almost twice the size of its twin, which was perfect, and yet there were no head and no upper extremities developed. In addition, experience proves that, no matter how much pressure is made upon the cerebral mass during labor, even for a long time, the child may survive, provided there be no solution of continuity of the tissue and no internal extravasation.

The essential cause of the death of the fœtus is a suspension of its respiratory function. This, physiologists have established, resides not in the lungs, but in the placenta, while the funis or cord is the conduit by which impure and pure blood is carried to and from the placenta. Experience also clearly demonstrates that the interruption of this important function is as dangerous to the fœtus as a cessation of breathing to the adult. The child perishes in a few minutes if its connection with the parent be interrupted. Many restrict the time to two or three minutes. Dr. Barnes and other good authorities affirm that five minutes is the extreme limit.

It is the general declaration that pressure upon the cord for any length of time is fatal. This is the truth, but only part of the truth. Pressure upon the placenta, or the separation of this organ from the uterus, is equally the cause of death. In pelvic deliveries, and also in cases of prolapse of the funis, accoucheurs direct various measures to protect or relieve the cord from pressure, which directions are often valuable. But in cranial deliveries we have no reason to suppose that the cord is often subjected to pressure, and yet in tedious and severe labors the child often perishes. The explanation of this circumstance is, that after the membranes have been ruptured, the whole placenta is compressed between the body of the child and uterus. A flat body of some eight or nine inches in

diameter is thus exposed to pressure from the powerful contraction of the uterus, augmented by spasmodic contractions of the abdominal muscles. All are familiar with the great force of these powers, sufficient almost to paralyze the hand of the accoucheur when within the cavity of this organ, or even, in some instances, to cause a rupture of its walls. We have a right, therefore, to presume that in most cases during the second stage of labor, there is at least a partial interruption to the circulation of blood, and therefore to the respiratory functions of the placenta. That death does not ordinarily ensue is owing to the admirable provision of nature that the expulsive efforts of the parturient woman are intermittent, not continuous. If, however, the pains should be severe and persistent, and the labor tedious, death does result from the interruption of the functions of the after-birth. This affords us an explanation why the *secale cornutum*, which excites such permanent and violent contractions of the uterus, has proved so detrimental to the fœtus.

There is still another circumstance why, in some cases, the infant perishes, which has been strangely overlooked by even the best authorities in the profession. It is the separation of the placenta from the uterus before the head is delivered. This may be occasionally noticed in cases of vertex presentation, where from a blow or fall the placenta has been detached with or without hemorrhage; also in cases of placenta prævia. But it is chiefly in cases of pelvic deliveries where the danger to the fœtus from this cause is most imminent. Practitioners, even the most distinguished, such as Professors Simpson and Barnes, appear to be satisfied that the child will be comparatively safe if the cord can be removed to the side of the infant's face, or tucked away in the notch on each side of the sacral promontory, forgetting that when the head is fairly engaged in the superior strait, the uterus is as completely emptied as it is when the child is born. It is contracted to a small size above the head of the child, and the placenta very universally separated from its walls. This seems to me by far the most frequent reason why children so often perish in pelvic deliveries, and why so little confidence should be placed in attempting to remove the cord from pressure.

The changes which take place in the child from the suspen-



sion of this respiratory process are very analogous, perhaps perfectly identical with those of asphyxia in the adult. In all cases of tedious labor, the surface of the child, and especially its face, is of an unusual blue color. In proportion to the interruption of the placental functions, the blue tinge deepens, and in confirmed cases, the whole surface is purple, the eyes are injected, the external tissues tumefied, which tumefaction often extends to the neck and the chest, while the hands and feet are often pallid and cold. The tongue and lips are also cold and blue, the pulsations of the heart are feeble, and that of the cord partially or completely suspended. This has been termed the apoplectic condition of the new-born infant, but is in reality a case of asphyxia or venous engorgement from suspended placental functions. All the phenomena will rapidly disappear if the pulmonic respiration be re-established. In bad cases, more permanent and serious evils may result from effusions of serum or pure blood, not only into the subcutaneous tissues, but also into the cavities of the cranium and thorax; hence the functions of the brain, heart and lungs cannot be established, or else are so imperfectly excited that the child remains blue, with an imperfect circulation and oppressed respiration, with great dullness, and with feebleness of its muscular powers, usually perishing after a few hours or days. It is in this way that children so often die during labor or immediately afterwards.

A second cause of death to the foetus is the suspension of the circulation. The child is born showing few, if any, marks of vitality, with no pulsation in the cord, and seldom more than a feeble thrill of the heart. The limbs are motionless, the face and whole surface of the body pallid, the features sunk, and little or no impression can be made upon its sensibility. It has been termed the anæmic condition of new-born children, but I have always regarded it as the sequela of prolonged asphyxia. It arises from the same causes, and it is possible that it may be, in a great degree, dependent upon such continued pressure from the uterine powers, that the circulation in the placenta and child is completely arrested.

A practical deduction from these facts is all-important. The practitioner, in all his operations, should never forget that all pressure on the child's head or body should be intermittent;

that the placental and circulatory functions should not be suspended; and, secondly, that no hope but that arising from speedy delivery can be entertained for the safety of the child when the placenta is detached from the uterine surface.

FIFTH.—*Compression upon the head of the fœtus can often be made by the forceps, not only with safety, but with great advantage during labor.*

The safety of delivery by the forceps is a point settled beyond controversy. It is established by universal experience. It seems almost equally incontrovertible, that in every forceps delivery more or less pressure is made by the instruments very generally employed in Europe and America. For in them the fenestræ are so small, or of a shape so peculiar, that the parietal protuberances cannot project through them, hence the thickness of each blade must be superadded to the measurement of the transverse diameter of the head, and necessitates, therefore, sufficient pressure on the head for their accommodation.

Again, after the instrument is applied, the handles must be approximated, in order to have a secure hold, as otherwise the blades would vacillate, or even slip upon the scalp of the child. When Dr. Meigs directs that only one blade should be acted upon at a time, carrying it from one side to another, so as to pry out the head, it is manifest that the head is compressed between that blade and its fellow, or pelvis upon the opposite side. And when Prof. Barnes affirms that no pressure should be made by the forceps, that they should be used simply as tractors, it is still evident, that as the head is drawn from a larger to a smaller space, the compression upon the sides of the pelvis, which necessarily results, is enhanced, not only by the thickness of the blades, but in proportion to the tractive power exercised.

Compression being thus always more or less effected by the forceps, the important question arises to what extent it can be carried with safety to the infant. It has been already said that this cannot be possibly determined, as the limit must vary in different cases.<sup>1</sup> Experience, however, shows clearly that

<sup>1</sup> Dr. Barnes would limit the use of forceps to a conjugate diameter of three and a half inches or more.

Baudelocque restricts the degree of compression of the head to one-fourth

children may often be saved by means of the forceps, when the passages are contracted and the head arrested. By this valuable instrument I have often delivered children with safety in women who in previous labors had lost their infants, and in some cases when there were but three inches in the conjugate diameter of the brim. The experience of many other practitioners coincides with my own.

In modern works, therefore, it is a settled principle that a perforator should not be employed until a previous attempt at delivery has been made by the forceps or by version. Of course, when the contraction is so great, the prognosis must always be doubtful. Many children perish and many are preserved. No statistics have established the proportion of safe deliveries under these untoward circumstances, as the condition of the patient and child, the time of operating, the skill of the operator, the instrument employed, and other conditions vary exceedingly.

Perhaps no one circumstance is of more importance than the construction of the forceps. Even the best in general use are lamentably deficient in some one or more points. Most of the English forceps are too short. Their long forceps, so called, have fenestræ so small that they occupy space—that is, the thickness of the blades must be superadded to the diameter of the cranium, while they are incapable, even when the handles are closed, of making any effective compression. They are, therefore, totally inadequate to accomplish delivery when the short diameter of the strait measures but three inches, as the measurement from one blade to the other is, very generally, nearly three inches. I do not wonder, therefore, that the experienced and judicious Dr. Burns (*"Burns' Obstetrics,"* by Dr. James, Phila., pp. 429, 30) observes, that it might not be possible, in a greatly contracted pelvis, to withdraw the forceps if applied. It may be added that all such attempts must be exceedingly dangerous to the mother's tissues during the passage of the head. No wonder, then, we read of such horrible laceration of the uterus and vagina, accidents which

inch; Velpeau, Oslander, and Siebold to one-half inch. Joulin and Chasagny made many experiments, proving that the head can be moulded more by the forceps than is commonly believed. Delore thinks compression of the head by the forceps may be harmless, if exerted over a large surface.

ought *never* to occur with a well-constructed instrument. The forceps of Dr. Haighton and those of Prof. Davis differ from the generality of British forceps in having large fenestræ, so that when properly applied they do not occupy space. The former instrument was, on this account, strongly recommended by Prof. James, of the University of Pennsylvania, and the latter by Prof. Meigs, of Jefferson College, but both instruments are confessedly inadequate for deliverance at the brim of the pelvis. Dr. Davis and Dr. Meigs would both restrict their use to cases where the head is *within* the pelvis, and the former invented a second instrument of a peculiar structure for the head retained at the brim.

The forceps generally used on the Continent of Europe are capable of making compression, and hence, if we may judge from the reports given, are far more efficient than the British. When the head is at the superior strait, Baudelocque's instrument, especially as modified in this country by Dr. Dewees, is certainly one of the most powerful and valuable devised,<sup>1</sup> and by it many children have, doubtless, been saved who otherwise would have perished. It is suitable to all obstet-

<sup>1</sup> The forceps used in Great Britain and often in this country, have short handles compared with the long handles of those used in France. It is evident that the longer the handle, the greater power as a lever, hence, its efficiency is greatly augmented and in cases of great difficulty much force can be exerted by very little muscular effort. The short handle, on the contrary, under similar circumstances demands much muscular effort, which not only induces fatigue, but renders it impossible to be very accurate or delicate in the application of the necessary force, and so we read of, not only the exhaustion produced in the operator, but of the slipping of the forceps, endangering the tissues of the mother and child and imperilling the equilibrium of the accoucheur. The power of this valuable instrument as a lever has been dreaded as much as its compressing power, forgetting the fundamental maxim *arte non vi*, and that the practitioner should be governed by his science and judgment, and not by the artificial restraints imposed by the instrument.

The latest account we have seen showing the inefficiency of the English forceps, as to their powers as levers and compressors, is reported in the Edinburgh Med. Jour., Feb., 1873, as detailed in the Obstet. Soc., Dec. 11th, 1872. The forceps had been applied three times in a case of contracted pelvis; there were two gentlemen in attendance. The one being exhausted by his efforts yielded the instrument to his companion, who, after great exertion, advanced the head very little. Perforation, hooks, etc., were resorted to, and the fifth effort was made with the forceps, by which, with much difficulty, the head was extracted. The operation was regarded by the members of the Society as being very well executed, especially as the woman recovered.



ric emergencies. By the advice of Prof. Dewees, I used it advantageously for many years, but found that it had its defects, three being very important ones. The first was, that when the blades were passed high up in the pelvis, the orifice of the vagina was unduly stretched by the divergency of the blades near the lock, and hence there was danger of injuring the perineum, especially in primiparous patients. The second and more important defect was the small size of the fenestræ, so that the parietal protuberances could not project through them; hence, owing to the thickness of the blades, undue encroachment was made upon the cavity of the pelvis and undue compression upon the head. A third defect that it had, was the flatness of the internal surface of the blade, so that it did not conform to the convexity of the cranium, and the internal edge of the fenestra was very thick compared with the external margin. Hence, when much pressure was made by the forceps, the edges of the fenestræ left their mark, often bruising and sometimes actually cutting the foetal scalp, giving rise to inflammation and even abscesses. I determined, therefore, to modify this otherwise excellent instrument, by accepting some of the peculiarities of Haighton's and Davis' forceps. The chief of these modifications consisted in rendering the internal surfaces of the blades concave, so that they would correspond exactly to



the surfaces of the head; also in enlarging the fenestræ that the parietal protuberances could project through these openings, and thus no encroachment would be made upon the diameters of either the pelvis or the head. They may be said to occupy no space. Again, I adopted the plan of Dr. Davis in having shanks intervening between the lock and the blades, and running parallel to each other, one anterior and the other posterior, and not laterally, as in Davis' instrument, by which arrangement there is no stretching the vagina, no danger of injuring the perineum, however high in the pelvis they may be introduced.

One great improvement in these forceps is the form of the space between the blades: it is ovoid, the small extremity of which is towards the points of the blades, and the large extremity towards the shanks. Hence the greatest transverse diameter is about two-thirds the distance from the ends, and the whole space is in exact conformity to the ovoid shape of the fœtal cranium. When traction, therefore, is made, the force is applied, not upon one portion of the head, but upon the whole anterior surface, extending from the parietal protuberance to the face. This arrangement, if the instrument be made of well-tempered steel, is an effectual preventive of its slipping; the greater the force applied, the greater will be the compression of the head, and a retraction of the whole more within its grasp. If the head should be small the points of the blades may reach beyond the chin, and of course be firmly secured.

This modified forceps I presented to the class at the University during the session of 1837-38, but an account of it was not published until 1843, when my friend, Prof. Huston, of the Jefferson College, politely requested a drawing and a description of it for his edition of Churchill's *Obstetrics* (p. 335). I have used it very frequently in almost every variety of forceps cases where the head was in the superior or inferior strait, and even when there were great pelvic contractions to the extent of even three inches in the conjugate diameter of the brim. In all instances it proved most efficient and safe. I do not know that any injury has ever resulted from it either to the mother or child, not even an abrasion of the scalp. It has been much used, not only in this city, but, perhaps more extensively than any other, throughout the United States. I have heard of no criticism as to its safety and efficiency.

For the successful use of forceps as compressors, many circumstances must be taken into consideration. No unnecessary delay should be permitted. As soon as it is ascertained that the natural powers are inadequate, artificial means should be employed. It is unwise to wait until the patient's strength is exhausted and the fœtal life jeopardized by the severe and repeated uterine efforts. Sir Jas. Y. Simpson has abundantly demonstrated, by an array of facts, that the more prolonged the labor the greater, *ceteris paribus*, is the danger to the mother and child. In contracted pelves the difficulty can be ascertained at

the beginning as well as at the end of labor, hence, the forceps should be applied as soon as the os uteri is dilated or at least easy of dilatation. There is no objection, and, to the experienced practitioner, no difficulty, in passing a suitable instrument even into the cavity of the uterus when the first stage of labor is completely over, and before the head is driven down or wedged in the contracted aperture. The blades being properly adjusted there is no necessity for immediate action. The forceps for a time may lie passively in the hands of the physician until the head is considerably depressed. Assistance should then be gradually rendered, when the compression, traction, and moderate leverage may be used with great efficiency in causing transit of the foetal head. The natural *vis a tergo*, conjoined with the artificial *vis a fronte*, as Dr. Barnes expresses it, will thus insure as little delay as possible at that particular juncture when alone danger is to be apprehended; the rest of the delivery in ordinary cases is easy.

All action with the forceps should be intermittent, and in unison with the uterine efforts. Constant traction would keep up constant uterine action, which, interfering with the respiratory process of the placenta, would be dangerous.

Another important rule is that the blades should be as nearly coincident as possible with the *occipito-mental* diameter of the head, as compression will then be made in the most favorable direction. If the head should be arrested with the occiput towards the pubis, this rule can be readily fulfilled, but this is a rare circumstance. The head, far more frequently, is found directly transverse; these direct applications over the parietal protuberances will be, in contracted pelvis, so dangerous to the tissues of the mother, that it should not be attempted; in such cases, the forceps should be applied obliquely over the side of the occiput and the side of the os frontis, one blade being directed towards the sacro-iliac junction and its fellow towards the opposite ramus of the pubis. Compression in the lateral direction can thus be very satisfactorily made. The blades acting in this way over the opposite extremities of the head would have a tendency to facilitate rotation, which would be a desirable circumstance. Hence the importance of learning precisely the exact position, by means of the commissures and fontanelles, of the head in the strait. This is in contravention of the German

practice, followed by many English practitioners, even by Dr. Barnes in his recent excellent work, of applying the blades of the forceps to the sides of the pelvis irrespective of the position of the head, a practice which I must regard as unscientific and often very detrimental. By following this rule the occiput and face would often be embraced, and all compression in this direction would tend to increase the transverse diameters of the head already locked between the pubis and sacrum, augmenting the difficulty of transmission and enhancing the dangers to the mother's tissues, especially the bladder and urethra. Such an application of the instrument can only be justified in rare and extreme cases.

Another rule for the successful application of the forceps, whenever they are employed, especially where the pelvis is contracted, is that flexion of the head must be previously induced<sup>1</sup> either by the natural bearing down efforts, or by the accoucheur. If the head be not flexed, the blades cannot be passed in the direction of the long diameter of the head, but will glide in the direction of the vertical diameter, for the top of the head, as represented by the anterior fontanelle, would be the lowest in the pelvis, and the base would be the highest. Traction thus made would be comparatively inefficient, and in the contracted pelvis perhaps wholly ineffectual, as the long diameter of the head would correspond to the lateral diameters of the pelvis; the occipito-frontal or mental instead of the vertical or cervico-bregmatic. This mode of operating is far too frequent, especially since M. Nægelè has so greatly ignored the deviated positions of the vertex, and introduced a practice at variance with the true mechanism of labor. Another serious objection to this direction of the blades, is that their points will necessarily project beyond the base of the cranium, endangering the tissues of the neck, or, if the cord should happen to be there, even the life of the child, by the pressure or laceration of the funis.

The almost universal direction given by our brethren across the water, of passing the blades over the ears of the child irrespective of the position of the fontanelles, has a tendency to

<sup>1</sup> Dr. Burns, who wrote his valuable work before the opinions of Nægelè had indoctrinated the profession, gave the same advice of pushing up the face and bringing down the occiput, by the vectis, before applying the forceps.



confirm this serious error. I have no doubt that under the guidance of these and similar rules, the application of the forceps, in moderately contracted pelvis, will be far more successful than it has usually been represented, particularly by British obstetricians, and that the powers of this invaluable instrument will be still further developed.

SIXTH.—*Delivery in moderately contracted pelves can be effected with greater safety by the forceps than by podalic version.*

Of late years this question has been warmly debated by the profession, and is still *sub-judice*. Prior to the introduction of the forceps, version was the only resource in cases of difficulty. It was, however, generally abandoned, notwithstanding the high authority of Madame La Chapelle, preference being given to the long forceps. A revulsion in favor of version has taken place since the year 1845, owing to the great talents and the laborious efforts of Sir Jas. Y. Simpson, whose authority, supported by that of Dr. Radford, has induced many of the best accoucheurs, not excepting M. Cazeaux, of Paris, and Prof. Barnes, of London, to adopt his ideas and practice. The question is, of course, a very difficult one, and at present it is impossible to come to any positive determination, simply from clinical experience, so far as facts can be collected.

I do not now propose an examination of this point, having in my work on the Principles of Obstetrics, on p. 404 et seq., presented my objections both to the theory and practice of Dr. Simpson. I shall, therefore, now merely give the conclusions therein reached. The idea of Dr. Simpson that the top of the head of the child is flattened when driven down in a moderately contracted pelvis, and therefore that the transverse diameter is elongated, is hypothetical. No evidence is adduced, and it is difficult to conceive how it can be substantiated. Authors do not allude to it, and the ease with which the parietal bones overlap each other, as so constantly observed, is opposed to this assertion.

In speaking of the form of the head as being conical, it is a great error to say the base of the cone is at the top of the head, and the truncated apex at the lower part, and represented by the bi-mastoid diameter, measuring two and a half inches. That

in some unusual cases the base of the cranium presents, especially when traction has been improperly directed, there can be no doubt, but this is *contra naturam*. It is very universally true when the process of labor is not interfered with, that either the chin or vertex presents. Hence, the proper representation of the form of the obstetric head is that of a double cone, the apex of one being at the occiput and the other at the chin, their common base being represented by the cervico-bregmatic circumference, and of course by the bi-parietal and cervico-bregmatic diameters. The head is, therefore, ovoid, and by a wise provision of nature the large extremity of the ovoid usually, 96 per cent., descends first. Any presentation of the top of the head as represented by the anterior fontanelle, or of the base of the cranium, is irregular and comparatively very rare. Moreover, it would be complicated by having the occipito-frontal, or long diameter of the cranium, substituted for the short or vertical, diameter. Dr. Simpson dwells upon the importance of having the bi-mastoid diameter present first in preference to the vertical, owing to the wedge-like form of the side of the head facilitating the descent, and that this portion is more compressible than the broad expanse at the summit; but as already remarked it is not the top of the head, but the occiput, that presents. It is with this portion, therefore, that the comparison should be made. The compressibility of the side of the head is very trifling, the mastoid portion of the temporal bone cannot be contracted, and this occupies one-half of the distance to the parietal protuberance, while a large proportion of the space between the occipital protuberances and the parietal protuberances is compressible, as is abundantly demonstrated in protracted labors, where the head becomes elongated and diminished in its lateral direction. The presentation of the vertex, therefore, is more favorable for delivery than the presentation of the base of the cranium. The length and compressibility of the occipital extremity being greater than that of the sides, no argument, therefore, for version by the feet can be drawn from the supposed advantage of the wedge form of the side of the head.

Numerous objections exist against podalic version. True, it is an easy and a safe operation when the uterus is distended with the liquor amnii, when it is in a relaxed state, and the

child in a mobile condition, and when there is no special impediment to delivery, and where the chin is approximated to the breast. But even in such cases, statistics demonstrate that the average death of children is thirty-three per cent.; but in difficult cases, M. Capuron, who is endorsed by M. Cazeaux, affirms that the fatality amounts to seventy or seventy-five per cent. We have no statistics at present as to any advantages which can be positively claimed for the forceps in contracted pelvis. Neither can we expect any favorable report from those who employ the British forceps, for reasons already mentioned. The Continental physicians have generally trusted to this instrument rather than to version; and even the high authority of Dr. Simpson, supported by many of his English compeers, by Madame Lachapelle and M. Cazeaux in France, does not appear to have made many converts. Dr. Lusk, in a résumé of German practice, published in Elliot's "Obstetric Clinic," covering many thousand cases, states that after making every allowance for doubtful cases, the mortality of children in deformed pelvis was thirty per cent. in vertex cases, and thirty-six per cent. in version cases. Dr. Lusk concludes from his extensive review, that the experience in Germany is decidedly in opposition to Simpson's views. Even M. Cazeaux, who gives a preference to version, has known children to be delivered naturally when the short diameter of the pelvis has measured three inches, or even less, and advises, that if turning should be impracticable, an attempt should be made with the forceps before perforation, as with their assistance children have often been born alive. Dr. McClintock, of Dublin, and Dr. Martin, of Berlin, are both in favor of occipital rather than pelvic deliveries in contracted pelvis. In the former case, time can be allowed for the gradual moulding of the head, while in the latter, the delivery must be speedily effected with strong traction.

Theoretically, the strongest argument in favor of version is the assertion by Drs. Simpson and Barnes, that it is not the bi-parietal diameter which is immediately involved, but that the head is nipped in the bi-frontal diameter, between the pubis and sacrum. They support this opinion with the fact that an indent or depression is often found on the side of the os frontis where it had been pressed by the promontory of the sacrum,

and not at the projection of the parietal bone. Hence, it is the bi-frontal, measuring three inches, which is involved, and that, as a necessary consequence, the whole head posterior to the bi-frontal commissures, descends upon the right or left side of the pelvis, thus rendering, they say, deliveries by the base more easy than deliveries by the vertex. The truth of this assertion is not by any means clear, for if the side of the os frontis presses against the sacrum posteriorly, it would imply that the head was in an oblique position, and hence it would not be the os frontis that would be opposed to the symphysis pubis, but the parietal bone behind the ear, which would give, not the bi-frontal, but the fronto-parietal diameter as corresponding to the conjugate diameter of the strait, which would measure fully as much as the bi-parietal diameter. Moreover, it presupposes that the transverse or oblique diameters of the strait are not contracted, which they usually are in a deformed pelvis. Even if it were true that the head could thus be brought through, presenting its bi-frontal diameter, this constitutes no argument in favor of pelvic deliveries, inasmuch as there must be the same accommodation on one side of the pelvis for a larger part of the head where the vertex presents, and hence the bi-frontal diameter would be concerned in the one case as much as it is in the other. The simple truth is, that there is a body of a peculiar size and shape which is to pass through the kidney-shaped opening, and this body being an ovoid, it is, mechanically speaking, of no importance whether the smaller or greater extremity of the ovoid projects first through the opening.

I cannot, therefore, avoid the conclusion, in view of all the facts and opinions, that, in cases of contracted pelvis where there are at least three inches in the conjugate diameter, the delivery by suitable forceps is far more safe for the mother, and that the mortality for the child would be less. All practitioners acknowledge the great difficulties and dangers incident to podalic version when the passages are contracted, enhanced immeasurably when the liquor amnii has been evacuated and the uterus firmly contracted on the child. Often in these cases it is not merely dangerous, but actually impracticable. Details are unnecessary. If the forceps be employed, comparatively no additional danger to the mother is incurred; the child de-



scends with that presentation which is always regarded as the most favorable. The blades of the forceps occupy no space. All the compression that is requisite, is made by the forceps rather than by the walls of the pelvis, and the greater the compression of the head, the less pressure there is upon the pelvic tissues. The extracting powers of the instrument can be regulated with the greatest facility, and be employed or intermitted at pleasure ; time being given for the gradual moulding of the head, as well as for the extractive efforts, inasmuch as the circulation of the funis and placenta are comparatively little disturbed. In my own experience the tissues of the mother have not been injured, or her life endangered. The advantage then for the mother greatly preponderates.

In version, even in natural labors, the child frequently perishes, often in the proportion of one to three, but in deformed pelvis the deaths are more than double. The reasons have been already stated. The placental functions are necessarily interrupted, so that the whole process of delivery must be accomplished in four or five minutes. No time is allowed for the gradual compression and moulding of the cranium. Powerful traction must be made by the muscles superadded, according to some, by the weight of the accoucheur, all of which power is directed through the medium of the child's neck, injuriously to its tissues, and not unfrequently there is a complete laceration of it, so that the practitioner sometimes finds himself prostrated, holding the decapitated trunk in his hands. Certainly this is not in accordance with science based on natural laws. The danger to the child from the above causes is aggravated by the neglect of the leading advocates of this practice to secure flexion of the head in unison with nature's mode of delivery, securing the synclitism of the cervico-bregmatic circumference of the head, instead of the occipito-frontal, as must be the case when the bi-mastoid descends first.

I may add that version or the forceps, in cases of contracted pelvis, should be regarded, in the present condition of our knowledge, as an elective operation. The practitioner, recognizing the deformity, should determine as soon as possible which mode of delivery to adopt. His decision should then be irrevocable. This must be the case if version be employed, the forceps must be abandoned ; it ought also to be the case when the forceps

operation is determined on. The blades being once applied should not be removed till delivery be accomplished, for, if the instrument be well constructed, the child can be delivered, if there be three inches in the conjugate diameter, sometimes, though not always, with safety. Nevertheless, we continually read in the journals of the forceps being applied once or twice and then abandoned with the declaration that after much effort the head would not descend. This certainly indicates the bad character of the instrument. What is still more surprising is, that after such failure version must be resorted to, and even with safety to the child. It has been said that success justifies the means, but how a practitioner can be justified, in a protracted case of delivery, where the waters have been long evacuated and the body of the uterus firmly contracted upon the body of the child and placenta, and when a portion, if not the whole, of the presenting part has passed the circle of the os uteri, in attempting version, is inexplicable. *A priori* it would seem to be impossible. The uterus is firmly contracted to a comparatively small size, there is no room for the return of the presenting part, and every attempt to push up the head and to introduce the hand, must be of the most imminent danger to the integrity of the vagina and uterus, and we know that the operation is often impracticable and fatal. No latent hope that the child might possibly be saved under these circumstances can compensate for the immense risk to the mother.

If the considerations now presented be hereafter confirmed, the utility of this invaluable obstetric instrument, the forceps, will be greatly enhanced. Its value, however, is not to be restricted even to the cases mentioned, for it may be affirmed—

SEVENTH, *The obstetric forceps, when well constructed, is the best extractor in cases of dead children, and also in cases of craniotomy.*

Of course, if the child be dead, there can be no objection, even when there is no disproportion between the head and the pelvis, to make powerful compression not only by the hands, but by a fillet wound tightly around the extremities of the handles. Some little time may be allowed for the yielding of the bones of the head before traction be made, then delivery can be effected with comparative ease, as all the diminution

made in the cranium not only facilitates its descent, but diminishes the pressure on the sensitive tissues of the pelvis.

If, however, there be any mechanical impediment, the head should be perforated, either before or after applying the forceps, and the brain and meninges should be well torn up, and then compression be made in the manner just mentioned. By the Hodge forceps the transverse diameter can be thus diminished to two inches, and for reasons already mentioned, the head will glide more and more into its grasp as the parietal bones yield, so that slipping will be an impossibility, and all requisite power is at command for delivery. So far from injuring the mother's tissues, they are comparatively safe in proportion to the diminution of the size of the head. Compare this with all the extractive instruments hitherto employed in craniotomy, as the crotchet, single or double, the craniotomy forceps of every variety, the cranioclast, tire-tête, etc. The direct influence of all these instruments is that of mere tractors. The head, it is true, is indirectly diminished, but as Dr. Meigs well expresses it, it is upon the principle of wire-drawing; the body is drawn from a larger to a smaller opening by mere force. The diminution of the size is effected by the resistance of the walls of the aperture. In labor, therefore, in a deformed pelvis, the opening is formed of bone covered by vital tissues; all the compressing force, thus, which is often immense, must be at the risk of the integrity of these tissues. The horrible results not unfrequently met with in the form of contusions and lacerations of the neck of the uterus, of the vagina, bladder, rectum, etc., are too well known to need repeating. Another terrible source of mischief is the slipping or tearing away of the hooks and crotchets, by which even the hand of the practitioner, as well as the parts above mentioned, are lacerated; or by the dragging away of portions of the cranium with their sharp serrated edges, with consequences almost equally severe, when extraction is to be repeated over and over again. No wonder craniotomy is reported by a late writer as the most terrible of obstetric operations, or that M. Baudelocque, Jr., should declare that fifty per cent. of women in these bad cases perish! How tedious and painful also is the convalescence of those that escape! How often are they left with fistulæ of the vagina, bladder and rectum! How fre-

quently do they suffer from dangerous thrombi, pelvic abscesses, vaginitis, metritis, adhesions and contractions of the vagina and neck of the uterus, and complete atresia!

I feel confident that nearly all these sufferings can be avoided by abandoning this wretched "wire-drawing" process and adopting an entirely new principle; that diminution of the head may be accomplished, not through the intervention of the mother's tissues, but by forceps, strong as they need be to insure sufficient compression for an easy delivery. This principle I have adopted and inculcated for more than thirty years, and have no reason to doubt its comparative safety, having observed no ill consequences even in very protracted deliveries, or when the transverse diameter was reduced to three inches. My first experience was in the case of a young primipara who had been five days in labor, under the care of a midwife, and afterwards of several physicians, who, *inter alia*, had given large doses of ergot. The patient was still in good condition, but the uterine efforts had subsided. Tumefaction of the head was so great that the presentation could not be determined with accuracy. I applied a strong pair of Bandelocque's forceps, but soon found the head was immovable. Perforation was therefore necessary, but as the head was firmly held, I determined that the forceps ought not to be removed. I gradually approximated the handles with a strong fillet, and was gratified to find that the head soon became movable and was delivered without any real difficulty. It was found that the arrest was owing to a presentation of the side of the head at the superior strait, and that the blades had been passed over the occiput and os frontis, and thus the long diameter of the cranium was lessened sufficiently for the transit of the head through the pelvis, and also through the inferior strait and vulva, and this without laceration of any of the tissues, not even the perineum. The patient readily recovered.

My next experience was equally remarkable. My excellent friend, Dr. Warrington, called me in consultation where there was an arrest of the head at the brim, the conjugate diameter being about three inches. Dr. W. had used the forceps, and repeated the operation after my arrival, but the head not descending he requested me to operate. I had no hesitation to make strong compression as well as traction, and for some time there

was no apparent effect, but during my efforts we were startled by a loud noise evidently within the abdomen. After which all difficulty vanished and delivery was accomplished. An examination showed that the noise was caused by a sudden collapse of the left parietal bone, which became, as it were, inverted, concave externally. The patient recovered without any bad symptoms.

Such facts—and many might be adduced—prove that the forceps may be so constructed as to be capable of making compression, and on suitable occasions they should be used as compressors, and as a substitute for the various tire-têtes heretofore employed.

*EIGHTH.—On the principles above inculcated, the introduction of cephalotripsy into obstetric practice by M. Baudelocque, Jr., should be regarded as the greatest improvement in operative midwifery since the seventeenth century.*

The cephalotribe, or brise-tête, indicating a crushing instrument, I have always thought an unfortunate name. True, the head is broken or crushed, but this is only one effect. It fulfils all the duties of a pair of forceps. Baudelocque's instrument is, indeed, nothing more than an enormous pair of strong forceps furnished with a screw by which the handles can be approximated, and thus the head in the grasp of the blades would be lessened and delivered through a contracted passage. Like the forceps, therefore, they act as *compressors*, *tractors* and *levers*. This idea should be borne in mind, as in many modifications since made, it seems to have been forgotten, some regarding it simply as a brise-tête, and others more as a double crotchet.

We have already indicated the great value of the principles involved; first, that the head is to be diminished, not upon the principle of wire-drawing, by the tissues of the mother, often to their irreparable injury, but by means of the blades. All the compression thus made lessens the pressure upon the mother's organs, perhaps in a direct ratio. This result is generally so effectual that, unless the deformity be very great, no injury is sustained by the parent. This, at any rate, is my experience. The mother's tissues were unhurt, and her convalescence unretarded. This affords a great contrast with all the reported



crochet deliveries, as they are usually termed, so tedious, so painful, so dangerous, not only to the pelvic organs, but also to the life of the mother. Well might Chailly exclaim, "Grâce au Ciel! that all the hooks and pincers have been driven from practice by the introduction of the cephalotribe." Thus he wrote in France thirty years ago; and I cannot but regret that the advance of improvement has been so slow that hooks and craniotomy forceps are still the common resort of the profession in all parts of the civilized world.

The original cephalotribe of M. Baudelocque, Neven, was first described by him in a paper before the French Institute, in 1829. A more elaborate account was published in 1832; and in 1833, a gold medal, valued at two thousand francs, was accorded him as a testimonial of the high estimation of the value of his instrument.

This cephalotribe was very heavy as well as long. It resembled, in its general conformation, the French forceps, the blades being without fenestræ and very concave on their internal surface, while at the extremities of the handles a strong screw was fixed, attached to a crank mandible, by which the handles could be forcibly approximated and the head crushed. Its weight was seven pounds. Soon after, in 1841, Busch reduced the weight to four pounds, and it became generally employed in France. A short history of the cephalotribe is given by Dr. Lusk, of New York, in a paper before the Academy of Medicine, and published in the *Medical and Surgical Reporter* for June 8, 1857, from which it would appear that, in Germany, the cephalotribe was first modified by Prof. Braun, of Vienna, afterwards by Scanzoni, of Wurzburg, by Busch, of Berlin, and Seifert, of Prague. Dr. Paul Grenser, of Dresden, states that there are four varieties in general use in Germany. The character of these German instruments differs from the French, in being much shorter and lighter, with the ends of the blades turned inwards, and their internal surfaces furnished with ridges, so that they act much upon the principle of the double crochet.

Many years elapsed before the accoucheurs of Britain adopted the cephalotribe. They did not approve even of the long French forceps, and dreaded the stronger compressors of Baudelocque, Jr.

“The theoretical objections to the brise-tête of Baudelocque<sup>1</sup> seem to be very great, *à priori*. First, its weight, amounting, according to the specimen in the author's possession, to nearly five pounds; second, its unnecessary length; third, the great power of the screw, and the large size of the handle or crank—all of which circumstances demand great care and attention in its employment, and sometimes may render its use difficult. The main theoretical objection, however, is the effect which such a powerful compression must have upon the bones of the head, and the dangers which may result to the mother. It might readily be supposed that the bones of the head being fractured, their sharp edges would be driven through the scalp of the child into the tissues of the mother; all attempts to extract, under such circumstances, would be exceedingly dangerous, if not impracticable.

“Experiment and observation have, however, not confirmed these theoretical objections. The author made a number of experiments upon the heads of children that died at term, and found that when the brise-tête was applied upon the sides of the head, the transverse diameter would be readily diminished to two inches, while the occipito-mental diameter, and also the cervico-bregmatic, were lengthened. The bones were turned inward upon the cavity of the cranium, and although fractured, in no case did they penetrate the scalp. . . . The top of the head projected during the process of compression to a considerable degree beyond the edges of the blades, but this of course would afford no impediment to delivery. Being satisfied of these important points, the author, not willing to employ the heavy instrument of Baudelocque, induced Mr. Rohrer, who had assisted him so materially in the construction of his forceps, to make, in 1843, some modifications in the French instrument. These modifications chiefly embrace the size and weight of the instrument, the change in the cephalic curve, and some alteration in the mechanical arrangement of the screw in the handle. . . . The instrument thus modified is far more manageable than the cephalotribe, and even than the common forceps, as the blades are narrower. The name ‘compressor cranii’ has been adopted as indicative

<sup>1</sup> Principles and Practice of Obstetrics, by Hugh H. Hodge, M.D. Philadelphia, Blanchard and Lea, 1864, p. 273, et seq.



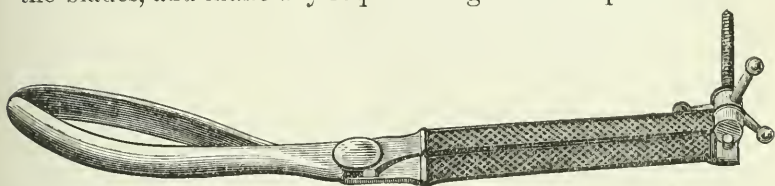
of its mode of operation. The whole weight of the instrument is three and three-quarter pounds. The proper *blades*, or cephalic portions, are without fenestræ, as in the brise-tête, and are six inches and five lines long, and one inch and five lines broad; the extremities are rounded, and the exterior convex, and well polished. The pelvic curve is lessened, and a perpendicular line, drawn from the upper edges of the points of the blades to a horizontal surface, upon which the instrument is placed, measures three inches; the cephalic curve is similar to that of the author's forceps, so that when the handles are closed, the points are in contact, and the greatest breadth, measured from their outer convex surfaces, is one inch and nine lines, and is nearer to the shanks than to the points, the former distance being two inches and three lines, and the latter four inches and three lines. Hence, the blades, when closed, can be passed through an orifice measuring two inches by one inch and five lines, and include an oval space. The inner surface of the blade is smooth, but quite concave from one edge to the other, contributing, by the projection of the margin of the blades, to prevent any slipping of the instrument. The danger of slipping, however, is chiefly counteracted by the peculiar form of the cephalic curve, as above described, causing the whole head, where compression is made, to glide more and more within the grasp of the forceps, till even the points project, in some instances, beyond the chin.

"The *shanks* are thick and strong, and diverging from the joint, terminate at the distance of three inches and five lines, in the cephalic portions. The lock is formed of a pivot, half an inch in length, strongly riveted into the left branch, and surmounted with a very broad flat button, an inch and a quarter in diameter. The right branch is furnished with a mortise for the reception of the pivot, as in the English forceps. The flat surfaces of each blade, at the joint, measure about two and a half inches, by one inch in width. These surfaces, in conjunction with the broad button of the pivot, prevent any disposition of the blades to twist when compression is made, and thus maintain the parallelism of the blades.

"The handles are nine inches and six lines in length, each one being flat and slightly tapering from the joint toward its extremity, where they measure seven to eight lines in width,

but toward the joint, about eight or nine lines, and three lines in their thickness. These flat surfaces are roughened.

“At the lower extremity of the left handle is attached a screw, four and a half inches long, fixed by a pivot, and is made to pass through a large opening or foramen at the extremity of the right handle. It may be convenient to have a slit in this foramen, so that the screw may be turned in or out at pleasure. A movable nut with female screw, and furnished with three projecting handles, an inch and three quarters in length, is adapted to the male screw, so that when the screw has been passed through the opening of the right branch the nut will act as a powerful screw and lever to close the blades, and make any requisite degree of compression.



“The *Compressor Cranii* being made of strong steel, well tempered, is unyielding, and capable, under the influence of the screw, to compress any foetal head at term, so that its transverse diameter should measure but two inches. It is as effectual, therefore, as the heavy brise-tête of Baudelocque, while being lighter and of smaller size, it can be handled with much more ease and precision. It can, indeed, owing to the narrowness of the blade, be applied with more facility than even the forceps. In operating with the compressor cranii, in a case, for example, of deformed pelvis, the blades are to be introduced separately wherever there is most room, and as near as practicable to the sides of the child’s head, but, of course, never directly toward the pelvis or sacrum. The male blade must be introduced first, and then the female blade on the opposite side; this can generally be done with facility. The practitioner should take care, if possible, to insert them so deeply that the points of the instrument should reach the face of the infant, in order that the whole head should be subjected to pressure, otherwise, after compressing the cranium, it may possibly be requisite, if the deformity be great, to loosen the instrument and reapply it so as to embrace the facial extremity

of the head. Being carefully applied, pressure on the handles will be sufficient to determine whether the instrument is properly located, and whether the tissues of the mother are not within its grasp. The screw, then, may be employed, and very slowly tightened as the vault of the cranium yields, and as the top of the head becomes more and more projected below the blades into the pelvis. When the compression is completed it will be advisable, especially in those cases where the blades have been applied to the sides of the pelvis before making any traction effort, gradually to twist the forceps, so as to approximate the blades to the sacrum and the pubis. The effect of this manœuvre will be to bring that diameter of the head which has been diminished to two inches in the direction of the shortest diameter of the superior strait; for it is manifest, that if the blades of the compressor be applied to the sides of the pelvis they will have a tendency rather to increase the size of that portion of the head intervening between the pubis and the sacrum, while by turning the instrument, and with it the head, the smallest diameter of the cranium will correspond to that of the superior strait. This being accomplished, the instrument may now be used as a tractor and lever, slowly and carefully, in causing the descent of the head, precisely as delivery is effected by the forceps.

“Great care should be taken as the head passes over the perineum that this tissue is not injured by undue pressure from the head, or from the extremities of the blades, which may, when the compression is great, be found projecting beyond the chin.

“When the deformity is great, especially at the lower part of the pelvis, after the compression of the head has been completed, and after it has descended to the floor of the pelvis, it may be advantageous, by means of the craniotomy scissors, to remove the upper portion of the head which projects below the margin of the blades, taking the convex edge as a guide for the course of the scissors; thus, much less space will be occupied by the head, and less pressure made upon the vagina, the rectum and perineum. This may be the more necessary, as the cervico-bregmatic diameter, which corresponds to the coccy-pubal, is augmented during the compression.

. . . . .

“But whatever modifications the instrument may require, the great fundamental principle now urged upon the profession, that the diminution of the size of the child’s head should be accomplished by instruments acting as compressors, and not by the bones and soft tissues of the pelvis—as must be the case where tractors of any kind are employed—is not to be forgotten in any case where cephalotomy has to be performed. This principle being acknowledged, the operation of craniotomy will be divested of half its terrors; perforation and compression, each of which can be very easily and safely performed, embrace all the important peculiarities of the operation, traction being securely effected, as in the common operation of the forceps. The patient thus escapes all those varied dangers arising from slipping of the crotchet, the breaking up of the bones of the cranium, and the removal of sharp broken fragments of the bones by means of hooks, osteotomist, cranioclast, etc.; and especially the terrible contusion and laceration of the tissues and viscera of the pelvis, which must result, to a greater or less degree, by dragging the child’s head through the contracted passages.

“If there be three inches in the antero-posterior diameter of the superior strait, craniotomy should not be resorted to, if the child be living, until at least every other measure, especially by means of the forceps, has entirely failed. And it is in such cases, where the deformity varies from that of a standard pelvis to that where the short diameter is three inches, that the forceps, in modern times, has been so effectual in preserving the lives of many infants, which would otherwise have perished from neglect on the one hand, or from craniotomy on the other.

“If, however, the deformity be below three inches in the conjugate diameter, and the patient arrived at the full period of gestation, craniotomy becomes justifiable for the sake of the mother; for a living child at term cannot pass through a pelvis so contracted.

“If, however, the contraction of the short diameter of the pelvis be two inches, or under, then, the Cæsarean operation is to be preferred, as affording a better prospect for the mother, while it has the strong recommendation of affording a good prospect of safety for the child.”

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## THE MUCOUS MEMBRANE OF THE UTERUS, WITH ESPECIAL REFERENCE TO THE DEVELOPMENT AND STRUCTURE OF THE DECIDUA.

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*Introduction.*

IN the fall of 1871, while pursuing my microscopic studies in the pathological laboratory of the General Hospital of Vienna, I was requested by my honored friend, Prof. Späth, Director of the Second Obstetrical Clinic, to examine an aborted mass, probably an ovum of the sixth week, in order to determine the structure and condition of the various tissues.

The outer wall of this ovum was formed by a thick, resistant membrane, which presented a most striking and interesting microscopic appearance; the tissue consisting in some places of large, well-marked epithelial cells, whereas in others it presented rather the appearance of young connective tissue; throughout its entire extent it was traversed by large, irregular sinuses, between which and the openings of apparently well-defined ducts, which appeared upon the smooth inner surface of the membrane, no direct connection could be traced; the capillaries were large, and distended with blood.

The specimens were examined by many, even by the veteran Rokitansky himself, who turned away with a word of thanks, but vouchsafed no explanation. The structure, it was evident, did not appertain to the ovum, but to the womb, it could be nothing else but the decidua; was it normal? was it pathological?—these questions no authority could answer, and Dr. Kundrat, first assistant to Prof. Rokitansky, appreciating equally with myself their high scientific and practical interest, we determined to seek for the answer, to study this still little-known structure, and give to the profession the microscopic anatomy of the decidua, of the mucous membrane of the womb during all its phases and physiological changes.

The pathological laboratory of the Vienna Hospital, within



whose walls the post-mortem examinations of the immense hospital, as well as the legal inquests of the gay capital, are held, was perhaps the only place where these researches could be undertaken, as none other could have yielded us the extent and variety of the rare material necessary.

In the course of the winter we were enabled to examine a large number of uteri in all possible conditions; more material was afforded by the post-mortems of the Charité and the obstetric clinic of Berlin during the summer of 1872, but above all by the extensive collection of ova in the anatomical museum of Berlin. These latter specimens I was enabled to study most thoroughly, as the unequalled liberality of my honored teacher, Prof. Reichert, had placed them entirely at my disposal.

The result of these researches was announced in a paper which was read by Dr. Kundrat before the "Gesellschaft der Aerzte" of Vienna on the 25th of October, 1872, and appeared in Stricker's "Medizinische Jahrbücher" for 1873.

The simultaneous publication in this country, which had been decided upon, was delayed, and other questions engrossing my attention, I intended to lay this matter aside for the present, knowing that such of the profession as were more especially interested in the results obtained, would refer to the German publication, and hoping at some future time to present a paper which would more thoroughly exhaust the subject, as the wealth of material accumulated would enable me to do.

To this resolution I have not been able to adhere; I deem it important that certain of the views expressed in our previous publication, which I cannot endorse, and which were developed after my departure from Vienna, should be corrected; other circumstances also, together with the request of friends, force me to what I might almost call a republication of our paper of 1873; the illustrations are all copied from this. I will not confine my remarks to the decidua, the mucous membrane of the womb during pregnancy, its period of highest vitality and greatest physiological importance, but will briefly discuss the structure of the uterine mucosa in its various changes, from its first appearance in the fœtus, throughout its period of development in the child and its long season of maturity and functional activity, to the time of involution and inactivity. This I deem



necessary in order that the picture presented may be a more perfect one, and that the result of our investigation may be more fully understood, as the macroscopic, and more especially the microscopic changes, which the mucous membrane of the womb undergoes during the various phases of female life, have never been thoroughly studied and defined.

I shall endeavor to present an accurate delineation of the membrane in all its various conditions *as characterized by the specimens examined*; confining myself strictly to these, I shall avoid a discussion of the numerous conflicting theories, calling attention merely to the more important particulars in which I deviate from current and accepted doctrines.

My conclusions are based upon the examination of a large number of uteri in various conditions, and of ova as well as uteri during all the periods of gestation. Seventeen uteri were examined containing normal, healthy ova in all stages of pregnancy, from the second week after conception to full term; of two hundred others, some were virgin wombs, some exhibited the menstrual condition, some that after abortion, and others that from the first day to the sixth week after delivery; in addition to these a large number were examined before the establishment of functional activity.

Twenty-nine of the ova were products of less than a month's gestation, a still larger number examined belong to the later period.

## I. THE MUCOUS MEMBRANE OF THE WOMB IN ITS DEVELOPMENT UP TO THE TIME OF PUBERTY.

The mucous membrane of the womb before it has attained functional maturity, as in the child, consists simply of round or polygonal cells with round nuclei imbedded in a very fine network of connective tissue. It possesses this same formation in uteri of incomplete or retarded development, which macroscopically also resemble that of the child in the flat hour-glass shape and in the prominence of the cervix. Spindle-shaped cells are found only around the vessels and near the surface.

All these elements, as well as the ciliated epithelium which lines the surface, are more delicate than in the fully developed uterus; the essential difference between this and the infantile womb, however, is in the glandular structure.

No glands are to be found in the even, hard, uterine mucous membrane of the foetus; nor do we meet with any trace of them during the first years of life, during which the membrane increases but little in thickness, averaging about 0.0078 inch (0.2 mm.), whereas in the seven months foetus it measures between 0.0035 and 0.006 inch (0.09–0.15 mm.).

The small depressions existing in the mucosa near the lateral angles of the uterine cavity are not indicative of beginning glandular development, being nothing more than radiations of the *palmæ plicatæ*, which, in children, are continued beyond the cervix.

A change takes place in the third or fourth year, at which time the membrane has increased in thickness to 0.0118–0.0196 inch (0.3–0.5 mm.), and the first traces of the developing glands appear in the shape of small round fossæ, often arranged in groups of three, clustered about a common opening; a delicate epithelium lines these sinuses, for glands we cannot yet call them.

From this time onward very few changes take place until after the tenth year, when the womb develops more rapidly and approximates in shape to that of the mature organ at puberty. The mucous membrane has now attained a thickness of 0.0275–0.0315 inch (0.7–0.8 mm.), and the glands are more numerous and more completely developed, forming no longer shallow crypts, but straight ducts 0.00098 inch (0.025 mm.) in diameter, extending to a greater depth than before, sometimes throughout the entire thickness of the mucosa and even into the muscular stratum.

This process of development once inaugurated now steadily progresses, and in girls of twelve or thirteen (in the climate of central Austria, lat. 48° N.) we find the changes still more marked.

Though the mucous membrane is not yet more than 0.0275–0.0315 inch (0.7–0.8 mm.) in thickness, it already contains well-defined glands, still rectilinear in their upper third toward the free surface of the membrane, but slightly curved or serpentine at their fundus.

The great difference of race among the inhabitants of Vienna, and the fact that many of the uteri examined were from girls who had long suffered with tuberculosis, caries,

coxitis, and other chronic and wasting diseases which may have retarded development, preclude any generally applicable statement as to the precise time of life at which the full development of the membrane takes place. This same intermingling of races and nationalities takes place in a much greater degree in our own country, which, even in itself, presents all varieties of climate.

Notwithstanding continuous attention to the subject, I have been unable, in our own city, to make any investigations bearing upon the development of the membrane, interesting as it would be to ascertain to what degree a change of latitude, climate, and habits would influence this process; the example of the Jews, whose Oriental characteristic of early puberty has adhered to them for ages throughout all climates, leads me to believe that the effect of such a change would not be a very marked one. In short, I must content myself with stating that the development of the uterine glands begins, in the first years of life, by the growth of the surface epithelium from without inward, in the form of small crypts, few at first; these increase, especially in the seventh and eighth year, in number and depth, forming narrow ducts; this development now progresses slowly, but steadily, and is completed by more rapid strides immediately before puberty.

## II. THE MUCOUS MEMBRANE OF THE WOMB DURING ITS PERIOD OF MATURITY AND FUNCTIONAL ACTIVITY, FROM THE TIME OF PUBERTY TO THE CHANGE OF LIFE.

### A. THE FULLY DEVELOPED MEMBRANE DURING ITS PERIOD OF REST.

The mucous membrane of the womb is characterized by the absence of even the slightest trace of submucous areolar tissue, and by its peculiar substratum of connective tissue abounding in cells. It appears in the virgin, non-menstruating womb, after removal of the superjacent transparent mucus, as a delicate, soft, pale-gray or grayish-red layer, which, owing to the absence of the submucosa, is closely and inseparably attached to the muscularis.

The thickness of the membrane is a little over 0.04 inch

(1.0 mm.) at the fundus and the anterior and posterior walls, diminishing toward the sides, the cervix, and the tubar ostia.

Transverse sections exhibit a system of delicate white striæ, indicative of the glands which traverse the membrane and open at the surface.

These glands are tubular, often bifurcated in their lower third, and round or oval on transverse section; they may be said to run parallel to each other, perpendicular to the surface of the membrane upon which they open; yet, strictly speaking, this is only true of the course of the glands in their upper half, and even here this course is not straight, but wavy, like the duct of a sweat gland. In their lower half they deviate much more, being more irregular and tortuous, the fundus curved, sometimes even so much as to run parallel to the muscular layer.

The fundus of a gland is very rarely found extending beyond the mucosa between the muscular fasciculi.

The glands are not equidistant throughout the entire depth of the mucous membrane; they are less wavy near the surface, and, moreover, narrow somewhat toward the mouth, so that they do not approximate as closely as in the deeper strata, where they bifurcate and become more sinuous.

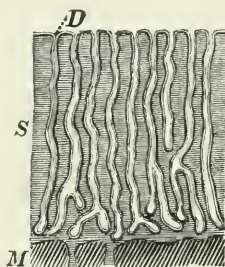


FIG. 1.—Vertical section of the mucous membrane of the normal virgin womb.  
Magnif. 40 diam.  
S. Mucous membrane.  
D. Uterine glands.  
M. Muscular stratum.

The uterine glands, unlike most others, possess no basement membrane, but appear simply as epithelial tubes, composed of cylindrical ciliated cells, and are directly imbedded in the substratum of connective tissue; neither the most careful preparation, nor the best of lenses, have enabled me to detect a membrana propria.

The shape of the cells is polyhedral when seen in perspective, but in horizontal preparation (in profile) it appears somewhat conical, tapering from the base toward the free end. In fresh sections and in specimens prepared by teasing, the body of the cell and the nucleus are seen to be granulated.

The ciliae can only be seen in very fresh specimens, and at favorable periods, though the remnants of the ciliary lining are recognizable in less recent and even in hardened specimens.

The substratum in which the glands are imbedded has been compared by Henle to the interglobular tissue; in the virgin womb it consists almost exclusively of round and spindle-shaped cells, of so delicate a character that in hardened specimens the faint outlines of the cell-body are mostly invisible, and the whole tissue presents the appearance of a structureless homogeneous mass with imbedded nuclei.

Near the surface of the membrane the round cells are most abundant, while the spindle-shaped cells predominate in the deeper layers, especially in proximity to glands and blood-vessels, where they assume a more fibrillar appearance, and accompany the course of those structures in parallel bundles.

A reticulum of delicate fibres can be traced between the cells—best visible in fresh, teased specimens; those fibres, like the spindle-shaped cells, are also collected in more compact longitudinal bundles in the deeper layers around glands and blood-vessels, while in the upper (inner) layers they divide irregularly, forming a loose mesh-work.

These fibrillar bundles of connective tissue, in the deeper layers of the membrane, radiate outward, and are lost in the interfibrillar septa of the muscularis, thus forming a firm connecting link between the two structures. Muscular fibres I have never been able to demonstrate, even in the deepest strata of the mucosa.

As the connective tissue of the mucosa is continuous with that of the muscularis, so the glandular cells are directly continuous with the ciliated epithelium lining the inner surface of the mucous membrane.

This is the normal appearance of the inactive uterine mucosa from the time of its complete development to its involution, and we will now consider it during its condition of functional physiological activity.



## B. THE UTERINE MUCOSA DURING THE MENSTRUAL PERIOD.

In any case in which the ovary reveals a recently ruptured Graafian vesicle, or even a fully developed corpus luteum, while the uterus and its appendages are tumefied and congested, the mucous membrane will be found in an essentially different condition from that just described, even when the hymen is intact, *i.e.*, when the state is unquestionably that of menstruation, and not of conception.

The membrane is swollen 0.118–0.236 inch (3.–6. mm.) in thickness, of an almost pulpy consistency. Its surface is puffy, wavy, and in places reveals the delicately injected capillaries, after removal of its coating of whitish, opaque mucus, occasionally tinged with blood. In some cases the injection is more marked in certain parts of the membrane, while in others, especially in those of sudden death, not due to hemorrhage, it is intense throughout, giving it a uniformly red appearance.

The greater thickness of the mucosa in the fundus and the central portion of the anterior and posterior walls of the uterus, and its decrease toward the tubes, the lateral angles, and the cervix, is much more marked in this tumefied menstrual uterus than when that organ is at rest.

After removing the mucus, the openings of the glands, apparently enlarged, become much more distinctly visible, especially after the specimen has been hardened in bichromate of potassa, and in vertical sections, even of very recent specimens, the glands themselves are distinctly seen by the naked eye in their entire length as white striae.

The microscopic changes which take place are equally marked, and prove the increased thickness of the mucosa to be due to a tumefaction of its superficial layers; its upper half, in which the stroma appears less compact, is rich in embryonic cells, and has grown far above the original gland openings, circumvallating them, and thus causing the funnel-shaped depressions, those small pits, which make the ostia seem enlarged.

The tumefaction is owing to a proliferation of the round cells of the stroma and an enlargement of the individual cells of all kinds, including those of the glands themselves, as well as an increase of the succulent, homogeneous, intercellular substance.



The enlargement and the increase of round cells is marked only in the upper layers, becoming less prominent in the deeper strata; it is not found at all in the tissue surrounding the fundi of the glands, which remains unchanged.

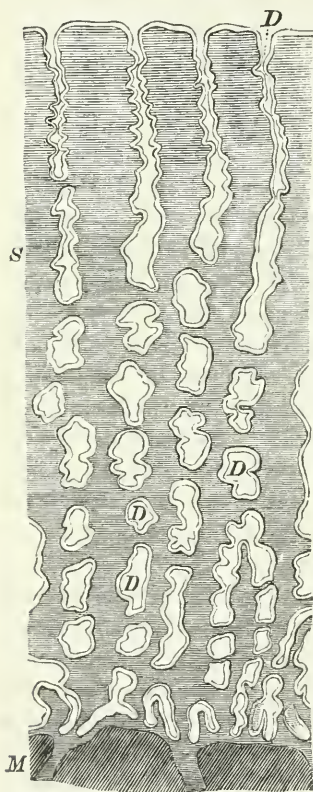


FIG. 2.—Section through the mucous membrane of the menstrual virgin womb. 40  $\times$ .  
S. Mucous membrane.  
D. Uterine glands, with funnel-shaped ostia.  
M. Muscular stratum.

The glands themselves, throughout their greatest extent, are very much enlarged, often two and four fold; the fundi, however, like the stroma surrounding them, remain unaltered, the ostia, with the upper portion of the ducts, are also of normal width, sometimes even narrowed by the pressure of the more forcibly expanding stroma.

Vertical sections show that the increase in the length of the glands, if we measure their length throughout their tortuous course, is even greater than the increase in the thickness of the mucous membrane, which is accounted for by their numerous shallow sinuosities in the upper layers and the more marked undulations in the deeper strata.

The glands are hence seen in such sections, especially in their central layers, as round, irregular spaces, partially lined with cylindrical epithelium, superimposed upon each other, and separated by very narrow portions of the substratum (Fig. 2, D D); nearer the surface we

can trace them in their continuity. The vessels are enlarged and gorged with blood, especially in such places as show an injection or a reddish tinge to the naked eye.

The process of segmentation, which I have observed in the cells of the stroma and of the glands, differs in no respect from

the well-known features of cell proliferation as observed in other tissues and under other circumstances.

A new formation of blood-vessels I have not been able to discover.

The changes just described are unquestionably due to menstruation, as I have observed them in virgin wombs concomitant with very recent corpora lutea, which we know to be indicative of the escape of ova.

Anatomical experience, as I will show, does not bear out the assumption, which theoretically appears well founded, that the menstrual changes of the mucous membrane just described come and go as rapidly as the menstrual period itself.

In two of our cases, girls who died suddenly a few days before the catamenial period, the precise time of which could unfortunately not be ascertained, and in a number of others in which we had no record of the time of menstruation, the mucous membrane already presented the less compact, tumefied appearance characteristic of the menstrual condition, while no signs of ovulation, no recently ruptured follicles, could be found in the ovaries.

In other cases in which the catamenial discharge was said to have ceased several days before death, and in which well-developed corpora lutea were found, the mucous membrane was still more or less tumefied, though not to the same degree as during the continuance of the hemorrhage.

This is the condition I have always found existing soon after cessation of the catamenia; in not one of the many uteri examined at such periods was the mucous membrane, or even its superficial layer, found wanting, a fact which I should have deemed wholly unnecessary to mention, were it not that theories, such as Pouchet's, that the mucous membrane is shed at each catamenial period, seem still to be accepted by some, and quite lately it has again been revived by Williams (On the Structure of the Mucous Membrane of the Uterus and its Periodical Changes, *Obstetrical Journal of Great Britain and Ireland*, February and March, 1875), who would have a new mucous membrane formed in each intermenstrual period, only to pass away as "sanguineous debris" with the next catamenial flow. He tells us that "when hemorrhage has taken place into the membrane, it undergoes rapid disintegration and be-

comes entirely removed." After having established this physiological improbability, to which we can find nothing analogous in the human system, he endeavors to explain the extremely rapid restoration of the membrane by a histological incongruity. As he has left nothing of the mucous membrane from which its regeneration may take place, he allows it to spring from the muscular tissue. To use his own words: "This membrane is produced by proliferation of the elements of the muscular wall of the organ, the muscular fibres producing the fusiform cells, the connective tissue the round cells, and the groups of round cells in the meshes formed by the muscular bundles the glandular epithelium." By such fallacies alone can theories like the above be explained.

The facts gathered warrant the conclusion that the mucous membrane of the womb begins to increase in thickness and succulence as the time of menstruation approaches, that this tumefaction is most marked during the period itself, and gradually decreases after the cessation of the catamenial discharge.

We rarely find a completely normal, inactive, uterine mucosa which seems to indicate that the actual period of rest for that membrane is much shorter than is generally assumed.

During the period of menstrual hemorrhage, and shortly after, until the restoration of the membrane to its normal condition, the cells of its tissue are more opaque and granular.

Although there is a more or less marked accumulation of fat granules within the cells of the upper layers of the membrane, during and immediately after the hemorrhage, it cannot be said that the fatty degeneration is very intense or extensive; as far as it goes this metamorphosis involves not only the cells of the interglandular tissue, but also the blood-vessels and the glandular and surface epithelia.

That these structural changes take place in the tissue coincident with the menstrual discharge, is unquestionably proved; but the relation they bear to the hemorrhage has not as yet been definitely ascertained, and anatomical appearances afford us no solution of this problem.

To my mind the relation is one of cause and effect; not only do I assert that the changes found to exist in the tissues are developed independently of the hemorrhage, but that they are, in fact, the cause of that hemorrhage.

A brief statement of certain facts proved in the course of our investigation will fully justify me in the position assumed:

1. *The congestion of the organ cannot alone cause the hemorrhage, as we find a more marked hyperæmia, a greater turgor of the vessels in the pregnant uterus and its mucosa, and yet no hemorrhage follows.*

2. *There are other physiological changes of the uterine mucosa in which a fatty degeneration of the tissues takes place, as in the gravid womb at term, or at any time previous when the ovum has perished, and the tissues are thus prepared for its separation and expulsion, as in premature delivery and abortion.*

3. *The hemorrhage in the menstrual womb is always confined to the surface of the lining membrane, and the fatty degeneration is likewise more marked in its upper layer. I have never found extravasations of blood in the substance of the mucosa.*

Thus it appears to me that after a certain degree of tumefaction has been reached by the menstrual membrane, this change in its elements, this process of disintegration, is inaugurated, and rapidly developing, especially upon the surface, leads to the rupture of the distended vessels, and hemorrhage ensues.

This retrograde metamorphosis of the tissue may be due to deficient nutrition, owing to an insufficiency of the vascular system, no new formation of blood-vessels being observable.

The destruction and detachment of a large part of the more exposed elements, of the surface, and even of the glandular epithelium, accompanies this process—in proof of which I will say that numerous epithelial cells, in a state of beginning fatty degeneration, are found imbedded in the whitish, somewhat bloody, mucus which fills the uterine cavity. I cannot, however, agree to the assertion that the *entire* epithelium of the surface is lost; until the beginning of the retrograde metamorphosis it certainly remains entire and intact.

Closely allied to the question last discussed is that of the temporal relation of the menstrual hemorrhage to ovulation.

The evidence we have of the simultaneous occurrence of these



processes appears to me conclusive, and it seems but natural that the high degree of congestion existing in the organs of generation should cause at the same time in the uterine mucosa tumefaction and menstrual hemorrhage, and in the ovary ripening and rupture of the Graafian vesicle.

I do not propose to say that the escape of the ovum takes place invariably upon one and the same day after the hemorrhage has set in, but the specimens examined by me do go far toward proving that the rupture of the Graafian follicle generally occurs toward the close of the catamenial period.

I need only refer to cases already cited, two especially characteristic, known to have died shortly before the menstrual period, in which the mucous membrane already displayed the menstrual tumefaction, and no signs of retrograde metamorphosis; in these uteri menstrual hemorrhage had not yet taken place, but was evidently soon to be expected, and no sign of a recently ruptured follicle was to be found in the ovaries.

In the ovaries of those whose death had taken place during the continuance of the hemorrhage clotted blood was found filling the cavity of the follicle, indicative of very recent rupture.

Cases that had died shortly after cessation of the catamenia showed the still tumefied, somewhat disintegrated membrane, and well-marked corpora lutea in the ovary.

These observations are moreover fully corroborated by the cases cited by Dalton in his prize essay, "On the Corpus Luteum of Menstruation and Pregnancy" (*Transactions of the Am. Med. Association*, 1851, p. 549), two of whose cases especially tend to prove the position here taken, and I will cite both, as they cover a time of the catamenial period not observed by me: In the first (p. 577), in which death occurred during the menstrual period, an enlarged Graafian vesicle, not yet ruptured, was found in the ovary; in the second (p. 578), death had taken place at the termination of the period, and the ovary revealed a Graafian vesicle, prominent, and on the point of bursting. Equally convincing is the case related by Dr. Michel (*Am. Journal of Med. Sciences*, July, 1848, p. 261) of a woman executed on the second day of the catamenial flow, in whom the follicle was found filled with clotted blood, its rupture having but very recently taken place, probably hastened by the circumstances of the case.

I have felt it incumbent upon me to explain my views in this matter at greater length, because in our joint publication in 1873, the manuscript for which was completed by Dr. Kundrat during my absence in England, a theory is advanced with regard to the temporal relations existing between menstruation and ovulation, which I deem erroneous and wholly untenable. My honored friend and co-laborer is led to second the views of a clinical observer, Loewenhardt (*Archiv für Gynæcologie*, 1872, iii., p. 456), who attempts to prove that the ovum impregnated has not escaped at the menstrual period last preceding conception, but that it is coeval with the menstrual period following, at which only tumefaction of the uterine mucosa is found, but no discharge takes place.

By this theory the duration of pregnancy is made to be three-fourths of a menstrual period, or lunar month, less than is generally accepted, and, as the author claims that impregnation cannot take place in the four or five days immediately preceding menstruation, he must consider the rupture of the Graafian follicle to occur much earlier than the menstrual hemorrhage. In view of this, I do not think that my friend, Dr. Kundrat, will still maintain that Loewenhardt's clinical observations are borne out by our anatomical facts.

Kundrat very truly says that a comparison of the mucous membrane at a time immediately preceding the menstrual discharge with that of a uterus soon after conception, containing an ovum, proves the identity of structure, the only difference being in the greater development of tissue in the membrane of the fecundated womb, a difference merely in quantity, and not in quality.

I think that he errs, however, in the inference he draws that the menstrual growth and tumefaction of the uterine mucosa is to be regarded as simply a preparatory state for the reception of the impregnated ovum.

The tumefaction of the membrane probably favors conception in cases in which the cavity of the womb has become enlarged in consequence of previous deliveries, or a pathological condition of the tissues; in these uteri the increased volume of the mucosa certainly favors retention of the ovum or semen, by bringing the walls into closer proximity, a circumstance we need scarcely take into account in the normal condition



of the organ, in which they are almost in contact with each other.

Enlargement of the uterine cavity and deficient development of the mucous membrane during menstruation are enumerated by Bischoff among the causes of sterility.

Of the menstrual abnormalities of the uterine mucosa, none has attracted more attention than that known as dysmenorrhœa membranacea, in which the normal physiological process is increased to morbid intensity. This condition, found in women who have borne children, as well as in virgins, in which large shreds of the membrane are expelled, illustrates more forcibly than the normal state how deeply penetrating the retrograde metamorphosis after menstruation may become. Instead of a mere gradual disintegration of the elements of the tumefied portion of the mucous membrane, and their rapid absorption, this entire upper stratum is detached and expelled, *in toto*, as decidua menstrualis. This is a morbid process, independent of fecundation, or the congestive excitement of sexual intercourse, as it is present in two of our cases, which are undoubtedly virginal; in one, the post-mortem examination revealed the deciduous membrane in the vagina; in the other it was still partially adherent, the greater mass, however, being lodged in the cervical canal. A comparison of the expelled shreds with the remnants of the mucosa lining the uterine walls showed that an unusual and morbid development of the membrane had existed. The degree of retrograde metamorphosis could not well be estimated, on account of the extravasation of blood throughout the membrane and the post-mortem changes which had already taken place.

#### C. THE MUCOUS MEMBRANE OF THE UTERUS DURING PREGNANCY— THE DECIDUA.

##### 1. *Normal development of the decidua.*

We have now studied the condition of the mucous membrane of the womb at the time of menstruation and ovulation.

All the examinations I have made of menstrual uteri have proved that the openings of the Fallopian tubes, as well as the internal os of the uterus are, even in that condition of the membrane, completely pervious, that the mucous membrane

does not unite or even agglutinate at these places. At whatever period conception may take place, which it is not for me here to discuss, whether in the tumefied menstrual condition or not, and in whatever part of the female genital tract, the ovum, impregnated or non-impregnated, cannot produce a reduplication of the uterine mucosa in seeking its passage from the tube into the cavity of the womb, by pushing the mucous membrane before it, as was once supposed. The theory of the origin of the reflexa based upon this view is refuted by the facts stated.

The ostia are not only pervious during the passage of the ovum, but continue so throughout pregnancy, unless they are closed by the growth of the ovum, or its adhesion in the immediate vicinity of the tubar ostium or the os-internum; the latter is always to a certain extent closed by the gelatinous secretion of the cervix, but never by the membrane.

The ovum is in most cases found lodged in the upper portion of the uterine cavity, upon the posterior wall, near the opening of one of the tubes.

Hence it is plausible to suppose that the mucous membrane being tumefied and completely occupying the uterine cavity, the impregnated ovum, as it leaves the tube, can descend no farther, and lodges in the fundus, generally in the posterior wall of the succulent tissue (on account of the recumbent position), whether this tumefaction is the result of a congestive condition and increased vitality due to conception or of the periodical menstrual swelling.

In accordance with this theory we could readily explain the more frequent occurrence of placenta prævia in multiparæ in whom, as above mentioned, the cavity of the womb is larger, and the thickness of the mucous membrane often less than in the vigorous virgin organ, so that the ovum would not be so readily arrested, and would be more liable to sink down toward the os.

I must say that I have not been enabled to examine any authentic specimens of impregnated human uteri earlier than the second week after conception, so that, in order to supply this missing link, I must connect the appearance of the tumefied organ preparatory to the menstrual hemorrhage with that of a fortnight's fecundation.

From the time at which the ovum enters the uterine cavity and begins to develop in consequence of impregnation, the uni-

formity of structure previously found in the mucosa ceases, and we can no longer treat of the membrane as a whole, but must distinguish three separate parts in accordance with the relations they bear to the ovum :

1. *The larger portion of the membrane lining the cavity of the womb, with the exception of that part to which the ovum is adherent : the decidua vera.*

2. *That portion to which the ovum is attached, serving to unite it with the maternal system : the decidua serotina.*

3. *The portion enveloping and enclosing the greater surface of the ovum : the decidua reflexa.*

It is best to study these several parts, constituting the maternal membranes of the ovum, at the various periods characteristic of their development, as they change greatly in the relations they bear to each other and to the ovum during the progress of gestation.

### *The Deciduae in the First Month.*

My observations upon the maternal membranes enveloping the ovum during the first month of pregnancy are mainly based upon the examination of three healthy uteri containing well-developed, perfect ova, the result of two and three weeks' fecundation ; these I obtained by the kindness of Prof. Reichert, of Berlin. (Prof. Reichert has published a most interesting paper upon the condition of the embryo in one of these uteri, which he judges from the known time of menstruation to have been only twelve or thirteen days old : *Beschreibung einer frühzeitigen menschlichen Frucht im bläschenförmigen Bildungszustande*, etc. *Monatsbericht der Akad. der Wissenschaft zu Berlin*, February, 1873, No. 1, p. 108.) Equally serviceable for the examination of the decidua vera were two uteri received in Vienna, in which conception was ascertained to have taken place two or three weeks before death ; in both of these the ovum had, however, been expelled—in one, which, with other of our finest specimens, we owe to the kindly interest of Prof. Rokitsky, the expulsion was caused by a traumatic peritonitis, resulting from a pistol wound in the abdomen ; in the other the expulsion was owing to the intense vascular excitement of a pneumonia combined with pericarditis.

In addition to these we had at our disposal a very instructive ovum, of the size of a pigeon's egg, which had been expelled with the entire mucous membrane of the posterior wall and fundus of the womb. The Berlin uteri had been preserved in alcohol, whereas the Vienna specimens came under our observation in the post-mortem room, and were examined in a fresh condition as well as after hardening.

In order to be precise, as well as brief, I will confine myself to the description of one of these specimens which is characteristic of the condition of the organ in the third week of pregnancy.

The uterus was enlarged; it had lost its flattened pyriform appearance, and was full, rounded at the fundus and sides, and highly congested. The thickness of its walls was increased to 0.6 inch (1.5 cm.), the body was 2.75 inches (7.0 cm.) in length, and the cervix 1.38 inch (3.5 cm.); the latter measurements varying in the other specimens from 2.0 to 3.15 inches (5.5 to 8.0 cm.), and from 1.18 to 1.38 inch (3.0 to 3.5 cm.) respectively.

The cervical canal was already obstructed by a plug of tenacious glassy mucus, and the walls of the cervix were also thickened, in consequence of which the *palmae plicatae* had become less prominent.

The mucous membrane of the uterine cavity is greatly hypertrophied, as much as 0.4 inch (1.0 cm.) in thickness, and very vascular, the intense injection of even the finest vessels giving it a uniform reddish color; its texture appears still more loose and puffy than in the menstrual condition, and its development more rapid than that of the surrounding muscular coat; the membrane forms wavy projections, and even convolutions and folds of 0.078–0.118 inch (2.0 to 3.0 mm.) in width, and up to 0.04 (1.0 mm.) in height; the surface presents a cribriform appearance.

The tumefaction abruptly ceases 0.06 inch (1.5 mm.) above the internal os, forming an overlapping fold.

In the fundus, near the left tubar opening, I found a round tumor (more oblong in other cases), enveloped by the mucosa and projecting into the uterine cavity, above the level of the membrane, with about two-thirds of its circumference. The character of the mucous membrane is somewhat changed in the portion enclosing this prominence; it is more dense, quite

smooth, and exhibits glandular ostia only at the base, which are distorted, though very distinct. By incision of the covering membrane the shaggy ovum was disclosed, its chorion covered with delicate villi uniformly developed over the entire surface.

After the ovum had been detached from the enclosing tissue, which was accomplished with the very slightest traction (unless the greatest care is taken, the simple handling of the organ will liberate it), a saucer-like depression was found in the mucous membrane immediately beneath the ovum, where its thickness was 0.04–0.06 inch (1.0 to 1.5 mm.) less than in other parts, though no other changes were visible on vertical section; upon its surface the glandular ostia were less distinct; and the irregular indentations existing gave it a rugged, uneven appearance.

From the border of this saucer-like depression in the mucosa grew the fold of the membrane which served to enclose the ovum, some 0.0787 inch (2.0 mm.) in thickness at its base, it formed but a very thin layer while upon the height of the prominence. Such were the naked eye appearances; corresponding structural changes were revealed by microscopic examination.

The mucous membrane in those parts of the womb where the ovum is not adherent—in the decidua vera—presents the same features which we have already seen in its menstrual condition, more fully developed.

The proliferation of interglandular tissue extends to a greater depth, and in the upper layers is still more marked than in the menstrual membrane; the individual cells are more enlarged, almost throughout; and in many the nuclei are found in the various stages of segmentation, indicative of active cell proliferation. The cells of the superficial layers resemble in character those of embryonic connective tissue; they are roundish or spindle-shaped, and frequently multiform from numerous processes sent out.

The finely granular protoplasm of the cell body contains a round or oval, coarsely granular nucleus.

The fibrous structure is almost lost, no new formation has taken place, and its delicate meshes are widely distended by the active cell proliferation which now extends throughout the entire membrane, even to the fundus of the glands.



The loosening of the tissue is now more marked in the lower strata, on account of the vascular distension and the glandular sinuosities; the crowding, closely-packed cells of the superficial layers give these greater density. Fig. 3. The openings of

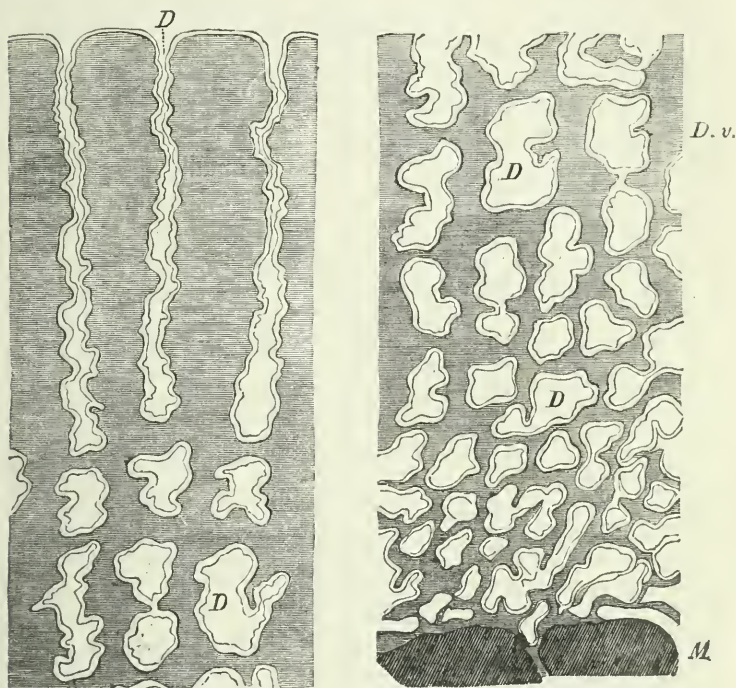


FIG. 3. Vertical section of the decidua vera in the second or third week after conception.  
40 ×.

D. v. Decidua vera.

D. Uterine glands, more rectilinear in the upper half, and serpentine in the lower.

M. Muscular layer.

the glands become more widely separated from each other, and are often narrowed by the increase of a more compact interglandular tissue in the superficial layers, but the funnel-shaped depressions, which remain in the thickened membrane, render them even more distinct. Between the upper portion and the almost normal fundus of the glands, throughout the greater part of their course, they are wavy and very sinuous, frequently with a diameter of 0.006–0.01 inch (0.15 to 0.25 mm.).

The fundus of a gland is occasionally found amid the loosen-

ed, slightly infiltrated cellular tissue between the muscular fibres, which is probably explained by the fact that fibres of the rapidly developing muscularis have grown upward into the mucosa.

The cells of the glandular epithelium are enlarged, increasing in size as well as number; those lining the duct near its mouth have grown more in breadth than in height, and are flattened, compressed, often very irregular or detached. The cells in the fundus alone seem to retain more of their normal appearance. The surface of the mucous membrane remains as in its previous condition, lined with ciliated cylindrical epithelium.

The decidua vera of the first week of pregnancy is, in short, characterized by a prolific development of the mucosa, especially of the interglandular tissue of its upper layers, and by an increase in length and volume of the glands themselves.

The mucous membrane at the point where the ovum is in contact with the uterine wall is developed in the same manner and undergoes the same changes as the rest of the membrane, the decidua vera. This part of the mucosa then, the decidua serotina, which at a later period underlies the placenta, is neither a new formation nor a plastic exudation. Though identical with the decidua vera and continuous with it in the first days after fecundation and homologous in structure, certain features peculiar to the serotina are already developed in the second and third week. Most apparent is its diminished thickness and the saucer-like depression above described.

The disparity in the size of the ovum and that of the glandular openings has long since demonstrated the fallacy of the theory which saw the impregnated ovum safely ensconced in one of the glandular tubes.

The examination of our earliest specimens leads me to concur in the views now generally accepted, that the ovum lodges in one of the depressions between the folds of the tumefied membrane, and that after impregnation has taken place, whether in the tubes or in the uterine cavity, the developing ovum is imbedded, pressed, as it were, into the soft and yielding membrane, by its own increased weight and volume as well as the resistance of the opposite wall; so that the convolutions of the decidua now surround the ovum, and the succulent tissue,

like water over a sinking body, closes over it more and more until its folds almost envelop the ovum; then the approaching borders of this developing prolific structure soon unite and entirely cover the germ, thus completing the formation of the decidua reflexa.

This explanation was adopted after that theory had been abandoned by which the reflexa was considered a reduplication of the mucous membrane at the tubar ostium, pushed forward by the descending ovum, and after it had become apparent that the ovum could not well enter a glandular duct; its correctness

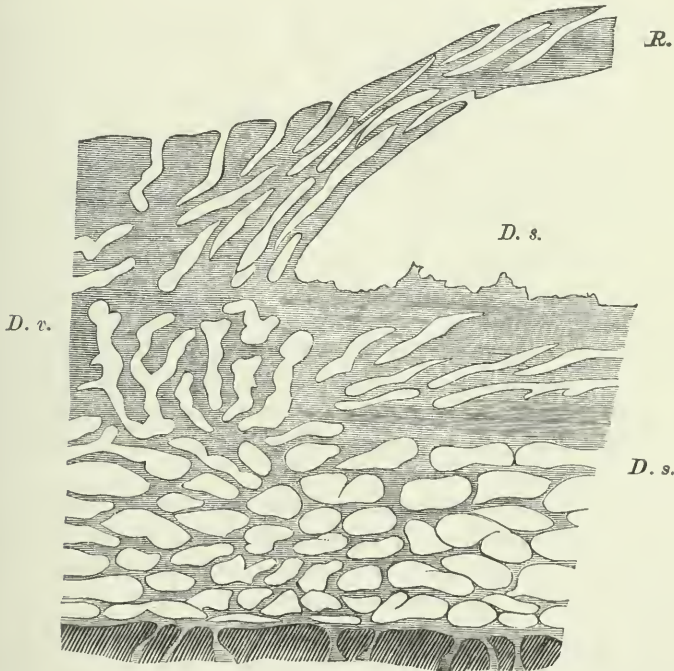


FIG. 4. Section through the maternal membranes in the second month of pregnancy.  $20\times$ .

D. v. Decidua vera.

D. s. Decidua serotina.

R. Decidua reflexa.

The ovum has been removed from its point of fixation between R. and D. s.

is conclusively proven by an examination of the decidua reflexa and the decidua serotina. In describing the macroscopic appearance of the membranes which envelop the ovum, we

have already referred to the glandular ostia on the smooth outer surface of the reflexa. They are distinct and numerous only in that part of the membrane where it is continuous with the decidua vera, but are here visible both upon the outer surface of the reflexa and upon its more uneven, slightly roughened inner face which is turned toward the ovum.

Microscopic examination teaches us not only that these distorted fissures are glandular openings, but also that the respective ducts, to which these ostia correspond, do not penetrate the reflexa diagonally, but pass longitudinally, parallel with its surface, to the depth of the mucous membrane from which it has sprung. But even where glandular openings are no longer visible to the naked eye, in the thinner, most prominent portions of the reflexa, over the projecting pole of the ovum, the microscope reveals at intervals very long, narrow ducts which still retain their epithelial lining. The appearance of the openings of glands on both surfaces of the reflexa and of the longitudinal course of their ducts within its tissue proves the correctness of the assertion that this membrane is developed from folds of the mucosa whose approaching borders unite with each other.

I cannot state from personal observation at what period of impregnation and in what way these edges of the developing reflexa grow together, as I have no specimens of so early a time; the earliest examined by me, at the end of the second week after impregnation, all disclosed a fully formed reflexa with no trace of its previous condition or manner of union.

The structure of the reflexa is analogous to that of the superficial layers of the vera; its tissue abounds in the same large cells, and even the glandular ducts found in the reflexa all contain a more or less well preserved epithelial lining.

The development and structure of the decidua serotina furnish farther proof of the above-described origin of the reflexa; its development advances uniformly with that of the decidua vera at the commencement of pregnancy, before the pressure of the ovum becomes greater, and before the formation of the placenta is begun, whereas at a subsequent period the interglandular tissue of the upper layers attains its characteristic density more rapidly.

The interglandular tissue in the upper and lower layers is



the same as that of the vera, and the glands, moreover, open directly upon the surface like those of the vera, their ostia being merely somewhat distorted by the pressure of the superimposed ovum.

The surface of the serotina, however, differs somewhat in appearance from that of the vera, being more uneven and irregular. With the aid of a magnifying-glass, sometimes even with the naked eye, we can see that the small protuberances upon its surface are not due to adherent remnants of the villi of a forcibly detached chorion, and microscopic examination proves them to be pointed projections of the tissue of the serotina itself with sinuous indentations (D. s., Fig. 4). Similar shallow depressions are found upon the rougher inner surface of the decidua reflexa.

It was not possible for me to obtain a gravid womb of the first month for the study of the vexed question of the connection of the chorion with the decidua serotina and reflexa. The Berlin uteri I could not well use for an investigation so detrimental to the appearance of those unique specimens; as far as macroscopic examination can enable me to judge, I must say that I could find no connection beyond a slight superficial agglutination here and there; and Prof. Reichert, after careful examination of the ovum in the earliest of the uteri, twelve or thirteen days after impregnation, says, in the paper already mentioned, that no connection existed between the ovum and the serotina; the villi did not enter the glands, whereas the reflexa was in some places already agglutinated to the chorion, individual villi being imbedded in its tissue, as he erroneously thinks, entering the glands.

The first uterus of an early stage of gestation available for this investigation, and which was at once examined in the very best state of preservation, was one obtained from a woman in the second month of pregnancy, who had died suddenly, crushed by a falling embankment.

The union of the chorion with the uterine membranes enveloping the ovum was in this case extremely superficial; the slightest traction sufficed to completely detach nearly all of the villi of the chorion, but very few of the delicate terminal branches remaining adherent to the uterine envelop. This adhesion I found to be caused by a union of a two-fold charac-



ter: it was in part owing to an agglutination of the villi to the depressions of the serotina and to the sides of its protuberances, the parts being simply superimposed and cemented by a connecting medium of tenacious mucus and detached epithelia; the continuity of the lining epithelium had been destroyed, and thus a more favorable surface was presented for an agglutination of the approximating elements; this was due, in part, to the fact that a few of the villi were already found imbedded in the developing projections of the serotina itself, this active proliferating tissue having enveloped the delicate branches in its growth. These villi alone, thus grasped, remained adherent to the serotina and reflexa when the ovum was removed.

Some of the villi were of course, before detachment of the ovum, found agglutinated to the surface of the serotina over the glandular ostia; but in a very few isolated instances only did microscopic examination prove a villus with its terminal branch to enter the mouth of a gland, and then it was never found to penetrate far into the duct which traverses the serotina obliquely in its superficial strata. The connection existing between the chorion and the decidua reflexa is the same in character, but the adhesions are still more scanty and delicate.

It was formerly maintained that the villousities of the chorion penetrate the glandular ostia and enter the ducts of the glands themselves, thus forming a comparatively firm connection between the ovum and the surrounding uterine membranes. This is still universally assumed, and is upheld, I am sorry to say, by some most eminent and well-known observers. It is a very plausible theory, which is demonstrated by most striking and conclusive illustrations, duly copied in all, especially obstetrical, text-books; but no macroscopic and microscopic preparations, proving the same, are in the hands of any of the authors who advocate its truth, and can, indeed, not be found.

My examinations and still preserved specimens prove that no such relation exists between the ovum and the uterine membranes, that the villi do not enter the gland tubes, but that the adhesions existing are owing to an agglutination of the parts and to the growth of the serotinal tissue around the villi.

This is the case in the human female, and I have found a similar condition in some of the higher mammalia, but have never seen the villi of the chorion regularly penetrating the

glandular tubules. With regard to the surface epithelium I can only say that I have still found it in places upon the decidua vera, so that it has not as yet entirely disappeared; but I can make no definite statement as to the extent of its preservation on account of the difficulty of preserving that delicate layer at this period of development.

The blood-vessels of the entire mucous membrane are enlarged, as is already apparent in the macroscopic specimen, they have increased in length, and extend their ramifications into the most superficial layers, and in the serotina even into its projecting villousities. In the serotina we have, moreover, found vessels which strongly suggest a new formation by their branching processes and the irregularity of their lumina.

The reflexa at this period is still vascular, showing blood-vessels accompanying the longitudinal course of the glands.

#### *The Decidua in the Second and Third Month.*

Our knowledge of the condition of the membranes during this period is gained from the examination of the uterus in the second month of pregnancy, already referred to, which contained two ova, each with its own chorion, in a single sack of the reflexa, a uterus in the latter part of the third month, and numerous ova expelled with the maternal membranes.

The uteri were larger than those of the previous month, showing an increase both in the thickness of the walls and in the size of the cavity; the ovum, however, had not correspondingly developed, and did not as yet completely occupy the entire uterine cavity. In both the ovum was attached, as is usually the case, at the posterior wall and fundus, and the space left in the cavity beneath was filled with an opaque, viscid, mucous fluid.

The decidua vera was up to 0.236 inch (6.0 mm.) and over in thickness, whereas the serotina was less than 0.157 inch (4.0 mm.), still puffy and convoluted with the cribriform appearance of its surface. The reflexa, which was only in partial apposition to the vera, presented a thickness of 0.0787 inch (2.0 mm.) at its periphery, near its line of continuity with vera and serotina, but not over 0.039 inch (1.0 mm.) in its central portions; its external surface was smooth, and in the uterus of

the third month also its inner face.

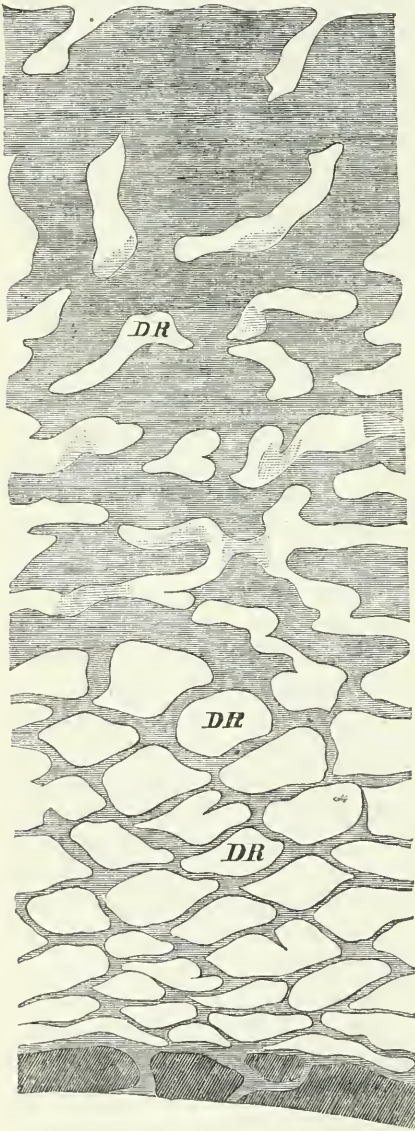


FIG. 5. Vertical section of the decidua vera in the third month of pregnancy.  $25\times$ .

D R. Glandular space, already beginning to assume the appearance of meshes in the lower stratum; narrower in the denser upper layer.

In the latter specimen the distorted glandular ostia were only to be found in the peripheric portions of the reflexa, and that membrane was already more firmly united to the chorion; chorion and amnion were in close apposition throughout, but as yet easily separated. The surface of the serotina in the third month has become more irregular and villous than in the second, and is more intimately connected with the villi of the chorion, which have also developed in that part of the ovum, having increased in length and become more densely ramified, forming shaggy tufts. Upon the rest of the chorion, over the projecting portion of the ovum, often designated as chorion laeve, the villi have apparently diminished; no further development having taken place either in size or numbers, the existing ones being more widely separated by the expansion of the growing ovum, seem smaller and less numerous.

The multiform cells in the interglandular tissue of the decidua vera are

very much enlarged, and that tissue itself is more dense and homogeneous in its upper layers, more delicate and minutely porous in its deeper strata.

The enlargement of the glands in the lower third [0.0098–0.0118 inch (0.25 to 0.3 mm.) in diameter] almost to their fundus, makes transverse sections of the membrane appear like a meshwork with larger meshes and finer threads [0.00196–0.00394 inch (0.05–0.1 mm.) in thickness] in the lower strata, the spaces become smaller, and their walls heavier [0.0098 inch (0.25 mm.)] while nearer the surface. The fundi of the glands, which are now almost parallel to the muscularis, are not enlarged, and near their ostia, which are still characterized by the funnel-shaped depressions, they are often even narrowed.

In some places the epithelia are well preserved, in others they are wanting, and a finely granulous mass is found instead. The occurrence of blood in the glandular cavities is probably pathological. Of the surface epithelium not a trace was visible upon any of the numerous sections examined. I am, however, unable to say whether this was not merely due to an imperfect conservation of the subject.

The projections of the serotina were more distinctly developed, and in their growth had imbedded individual tufts of the chorion within their proliferating tissue, above the former surface of the uterine membrane; yet it might appear to the casual observer that these tufts, which may be said to be the first indication of the future cotyledons, had penetrated the serotina. Still comparatively few of the villi had become attached in this manner, and it was evident, too, that they did not penetrate the glandular ducts, though some were agglutinated upon the funnel-shaped depressions which form their ostia. The terminal branches of the villi rarely approach the serotina in a direction vertical to its surface, but more often obliquely, and even run parallel with it.

The glands of the serotina presented an appearance almost precisely like those of the decidua vera, wide and very sinuous in their course, but narrow at their ostia and fundi; the epithelium was likewise only partially preserved; in part it was detached, or, its outlines having become obliterated, it remained as a finely granular mass within the glands. Similar granular masses were also found adherent to the terminal villi of the



chorion, probably the remnants of the surface epithelium, which serve, as above stated, together with the scanty mucus, to agglutinate the parts.

The cells of which the reflexa is mainly composed are very large, corresponding to those of the interglandular tissue in the upper layers of the vera; the glandular ducts are very much compressed and contracted, often visible only as narrow fissures; the blood-vessels of the reflexa, with the exception of those in its periphery, are in process of obliteration.

I have already referred to the close connection existing between the chorion and the decidua reflexa; in the preparation of microscopic specimens, chorion and amnion, whose smooth surfaces approximate, are readily separated, but chorion and reflexa adhere more firmly to each other, presenting irregular, rough surfaces of contact; the villi of the chorion are forced, as it were, into the tissue of the reflexa, and are enveloped by it.

The diminished thickness of the reflexa seems to indicate that the growth of the ovum is more rapid than that of the membrane enclosing it; this is indeed the case, but the development of the reflexa has not entirely ceased, as even a macroscopic comparison of the specimens will show, and the condition of its cells and the segmentation of their nuclei is a still more conclusive proof of its activity and growth.

#### *From the Fourth Month to the Termination of Pregnancy.*

Our observations as to the condition of the mucous membrane of the uterus during this period of pregnancy are based upon the examination of nine uteri, all of which were obtained in a most excellent state of preservation; every one of the latter months of pregnancy, from the fourth month onward, is represented by one, some even by two, pregnant uteri.

At about the fourth month the size of the ovum has become such as to completely fill the uterine cavity. During the three first months the growth of the uterus is more rapid than that of the ovum; the uterus is enlarged, its walls have become thicker, and its cavity, increased in size, is but partially filled by the slowly growing ovum. After the fourth month the germ surpasses the enveloping matrix in its growth; the womb has be-



come comparatively passive, its cavity is distended by the now rapidly developing ovum and the uterine walls, extending over a greater surface, are reduced almost to their normal thickness, diminishing still more in the later months of pregnancy. The cervical canal does not become distended, and in no way contributes to the enlargement of the uterine cavity, as some authorities still suppose.

The ovum in the fourth month is still freely movable in the uterine cavity, adherent only by its placental attachments,

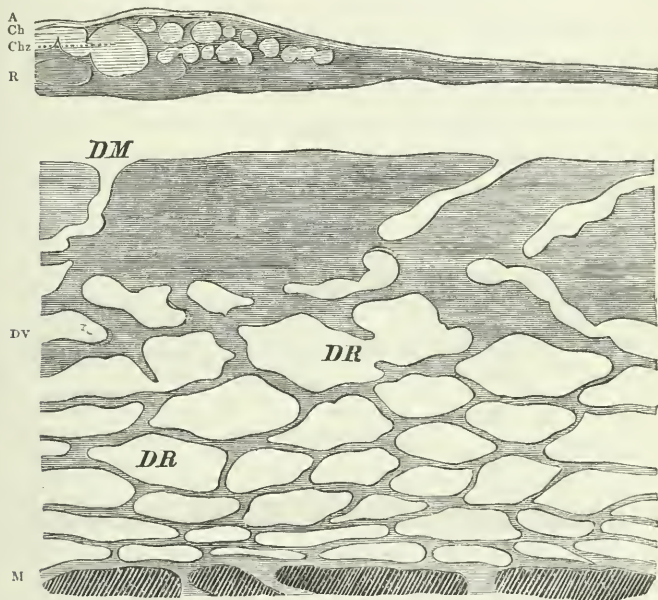


FIG. 6.—Vertical section of the maternal and fetal membranes in the fourth month, magnif.

35 diam.

A. Amnion.

Ch. Chorion.

Chz. Villi of the chorion imbedded in the reflexa.

R. Decidua reflexa.

DV. Decidua vera.

DR. Glandular spaces, giving the meshy appearance to the lower stratum.

DM. Glandular ostia.

M. Muscular wall of the uterus.

while the enveloping reflexa is not yet in any way united to the surrounding uterine walls. This free movable portion of the ovum which projects into the uterine cavity is covered with a delicate, smooth, colorless, almost perfectly transparent mem-

brane, which consists of three distinct layers, the two foetal membranes, the chorion and amnion, and the maternal envelop, the decidua reflexa.

The former are still easily separated from each other, but the reflexa can only be detached from the chorion by the exertion of some little force, and then adherent shreds remain here and there upon its surface.

The decidua vera has changed essentially in appearance; its surface, though still cribriform, is no longer puffy and undulating, as its convolutions have become expanded by the dilatation of the uterine cavity; throughout its entire extent it has been reduced from its greatest thickness of 0.4 inch (1.0 cm.) during the first month to a layer of not over 0.07 inch (1.8 mm.) Transverse sections show an upper, more uniform, dense membranous stratum, and beneath this a spongy laminated tissue, the meshes of which become especially prominent when immersed in water.

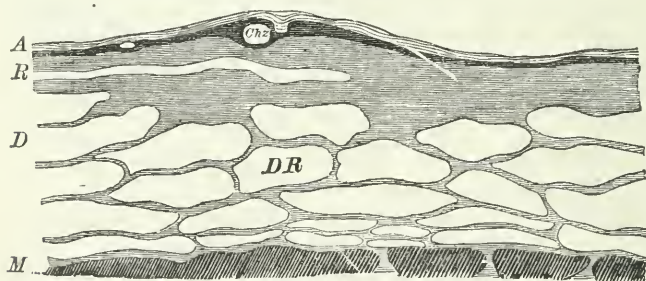


FIG. 7. Vertical section of the maternal and foetal membranes in the fifth month of gestation.  
35 ×.

A. Amnion.

Chz. Villi of the chorion imbedded in the reflexa.

R. Decidua reflexa.

D. Decidua vera.

DR. Glandular spaces of the lower stratum.

M. Muscularis.

After the fifth month that part of the ovum which projects into the uterine cavity with its enveloping membranes, among which I must class the already adherent reflexa, can no longer be so easily removed from the surrounding uterine walls; the opposing surfaces of the decidua vera and the reflexa are now partially agglutinated; in later months it is impossible to separate them without lacerating one or the other of the membranes, and

finally they become firmly united throughout their greatest extent. The decidua vera has not only become still thinner, 0.0472 inch (1.2 mm.), but the appearance of its surface has also undergone a change; instead of the former characteristic funnel-shaped glandular ostia we see but very shallow depressions with a well-defined border. Sections still reveal the same structure, the uniform, dense upper layer and the spongy meshwork beneath.

After the fourth month chorion and amnion are also more firmly agglutinated, though even to the termination of pregnancy the connection is never such that with a little care the transparent, colorless membranes cannot readily be separated from each other. Not so with chorion and reflexa, their adhesion having become so firm that parts only of the membranes can be detached; in those places where the reflexa has been successfully removed the outer surface of the chorion now also appears perfectly smooth.

#### DEVELOPMENT AND STRUCTURE OF THE PLACENTA.

The most important and essential change in the chorion as well as the mucosa has been produced at their point of contact by the formation of the placenta.

The villousities of the chorion, whose development upon this part of the ovum we had already observed in the second month, have now, by a more vigorous growth and the extensive ramification of the individual villi, formed a spongy, tufted mass of the shape of a flattened ovoid, and are so closely united with their terminal sprouts to the protuberances and the surface of the serotina, that they cannot be separated without lacerating its tissue, parts of the superficial layers clinging to the tufts of the removed chorion. After the fourth month, then, the placenta is fully formed, and we can recognize its component elements much more readily than in the mature structure, where they are completely fused. In fact, it is only in the developing organ that we can with certainty distinguish a *placenta fœtalis*, by which I mean the villousities of the chorion, and a *placenta uterina*, the maternal part. Of the latter the text-books give a very indefinite, if not a totally false, conception; it is composed of only the most superficial of the dense upper layers of the sero-

tina, and in its greatest extent of the protuberances which spring from these and envelop the ramifications of the villi in their growth.

From the beginning of the second half of gestation to the end of pregnancy, the union of the foetal and maternal parts of the placenta is continually strengthened by the imbedding of the sprouting, downward-growing tufts in the proliferating tissue of the serotinal protuberances ; the individual cotyledons also become more intimately connected, thus adding to the increased density of the structure.

With the exception of these changes upon its surface the serotina, analogous to the vera, presents the same features which I have already described in the previous months, only that its thickness is somewhat diminished ; it possesses the same dense upper layer, now sending its projections between the ramifying villi, and a spongy, laminated stratum beneath, somewhat narrower than that of the vera. Its structure continues unchanged to the termination of pregnancy, though the layers still decrease in thickness, the lower meshes and fundi of the glands preserve their epithelium more or less perfectly.

At no time are foetal villi found in the glandular spaces of the lower laminated layer, or in the narrow fissures which represent the ducts in the upper strata ; so that the statement I have made with regard to the relation of the chorion to the serotina in the earlier months of pregnancy, that the villi do not penetrate the glandular ducts, is equally true of the more fully developed ovum.

In the trabeculae of the meshwork we find large blood-vessels corresponding to the vascular development in the upper layer of the membrane at this point, and to the blood sinuses in the muscularis beneath. New formed capillaries extend into the protuberances of the serotina.

This, unfortunately, is all that I am enabled to state with positive assurance, upon a point so necessary to a full understanding of the vascular structure of the placenta ; more our specimens have not revealed.

Probably these capillaries grow with the proliferating tissue as it becomes more and more interwoven with the foetal tufts, and thus maternal and foetal vessels are brought into closer contact, but their walls and the surrounding tissue, though thinned



and atrophied, are preserved, so that the villi do not "float" directly in maternal blood.

Such are the facts with reference to the formation and structure of the placenta, as far as I have myself observed them.

I am unable to explain satisfactorily to my own mind the origin, development, and structure of the large blood sinuses of the placental portion of the serotina, which all authors describe as the most important feature of that structure. Ercolani and Braxton Hicks deny their existence; the latter, having examined eight placentaë of different months of pregnancy *in situ*, found no blood between the villi, or only a mere trace, which had come from some ruptured foetal capillaries, and remarks that this fact seems a direct contradiction to the theory of large maternal sinuses surrounding the villi.

Our injections of the placenta would indeed make it appear that the villi of the chorion project directly into the maternal sinuses, being only lined with their own proper epithelium, and not by a second distinct layer which might be construed to be the wall of the serotinal vessel, so that they would seem to be bathed in maternal blood coursing in the spaces between the villi. Though the injections were the very best, made *in situ*, from both maternal and foetal vessels, by the careful and skilled hands of Dr. Fleischel, and the results are analogous to those of other investigators, I cannot preclude the possibility that the injected fluid had ruptured the delicate capillary walls and penetrated the interstices of the tissue, thus producing the puzzling appearance described, so that the sinuses would be mere artificial products. This is all the more probable, as the capillaries are large, and the resistance of their yielding walls is, moreover, greatly diminished by the fatty degeneration which they have undergone, and the post-mortem changes that have taken place.

As regards the character and development of the maternal circulation, I am not as yet prepared to make any definite statement, and hence prefer to give only the positive results of my investigation, without derogating from their merits by coupling them with theories, however plausible.

The facts stated, new and widely different from the theories generally maintained, are not only verified by all the specimens examined, but I also see a convincing proof of their truth in



the analogous development of corresponding structures in the higher mammalia.

As in other instances the study of comparative anatomy has led to a more perfect understanding of our own organization, so the more simple relation of the maternal and foetal membranes in animals serves to explain the more complicated structure of the human placenta, and it is found to be in perfect harmony with the conditions I have described ; we have the same dense, hypertrophied superior layer of the mucous membrane, whose protuberances interlace with the villi or folds of the chorion.

It is almost impossible to conceive how theories such as those generally accepted with regard to the structure of the placenta could so long be taught in this period, which exacts close scientific inquiry. By these current theories the placenta is made to develop regardless of physiological laws and at variance with all known histological processes as well as the teachings of comparative anatomy, so that if those views be true we must allow an utter disregard of fundamental laws in favor of this one structure. I have endeavored most conscientiously to demonstrate the connection existing between the villi of the chorion and the serotina ; we have seen that it is an interlacing of new-formed tissue above the original surface of the membrane, and that the villi do not enter the uterine glands. Thus the very foundation of one of the most widespread theories falls to the ground.

But even less in harmony with facts is the account which has been given of the farther development and the structure of the placenta. It does not explain how the glands can, or do, grow upward, and send out branching diverticula to fit the ramifications of the villi ; and no actual investigations, no specimens, are brought forward to support such a theory. It further changes the uterine follicles into vascular sinuses, in which the villi of the chorion float about ; whereas, it is a well-known physiological fact that blood at once coagulates upon entering such interstices of tissue.

Another theory would have the villi push the tissue of the serotina before them and penetrate the maternal vessels, which again would bring the nutrient blood of the mother in contact with a coagulating foreign body, the surface of the foetal villousities.

The results obtained by Ercolani (*Sul processo formativo della porzione glandulare o materna della placenta. Bologna, 1870*) in his thorough examination of the placenta uterina in man, the ape, and the horse, in the ruminantia, rodentia, chiroptera, carnivora, and insectivora, agree so perfectly with my own observations upon that part of the uterine mucosa that I have passed lightly over the old theories; and the fact that my statements harmonize so fully with those of the learned Italian, of the existence of whose paper I was not aware at the time those researches were made, is a further proof of the correctness of the doctrines I advance.

RETROGRADE METAMORPHOSIS OF THE MEMBRANES.—MACROSCOPIC APPEARANCES.

The condition of all the parts, the chorion, the uterine mucosa, and the placenta, continues about the same as that last described until the termination of gestation. In only one respect is a marked change found to take place, and this becomes apparent, even to the naked eye, during the last month of pregnancy, occasionally at a somewhat earlier period; in cases of abortion, it is observed during whatever month they may take place, at the time of their occurrence.

The covering membranes of the ovum, including the decidua reflexa, first lose their transparency, and become opaque, whitish, with a pale, yellowish tinge; then, too, the upper layers of the decidua vera grow paler and assume a delicate, continually deepening, yellowish hue, which gradually extends to the meshwork of the deeper spongy layers; lastly, the decidua serotina shows signs of a similar change, which is, however, by far less intense. This opacity of the membranes can at any time be readily observed in the mature ovum after expulsion. I have twice seen this discoloration, most exquisitely marked, in the decidua vera, giving it a pale, yellowish-gray color; once in the case of a woman who had died at the termination of pregnancy from embolism of the pulmonary artery, resulting from thrombosis of the vena saphena, and again, in a patient who died of sheer exhaustion during protracted labor.

## DECIDUA VERA.

The decidua vera has, during the fourth month, as indicated by its macroscopic appearance, undergone some very essential structural changes. The upper, uniformly dense, membranous stratum, which certainly forms a part of the vera, and is developed from that membrane only, as is proved by depressions of the glandular ostia upon its surface, and the absence of any connection whatsoever between it and the approximate surface of the reflexa at that period, is composed almost entirely of very large cells which Friedländer calls epithelioid. The resemblance of these cells to those of pavement epithelium is due to the very marked enlargement of the cell body in surface, and the prominence of its nuclei. It is this appearance of the cells, as well as their size, which has induced certain observers to regard them as a proliferation of the epithelia, either of the surface of the mucosa, or of the uterine glands, and it must be admitted that the cells bear a close resemblance to epithelial cells in their general characteristics, especially in sections of hardened specimens. In fresh specimens, however, and those kept in conserving fluids, they reveal a very different character, proving their similitude with the embryonal cells of young connective tissue, especially such as are found in certain myxomata.

In the more fully developed decidua, between the fourth and the eighth month, we distinctly see them to be branching or spindle-shaped, and not round or polygonal pavement cells. At a later period the body of the ramified cells greatly preponderates over the processes which branch out in different directions. During the first months I have observed numerous round bodies, resembling white blood-corpuscles, between the cell processes, which are more rarely seen toward the end of pregnancy. Pl. I., Fig. VI.

As gestation approaches its termination, the spindle-shaped cells disappear in part, while the large, branching cells increase in number, and as their processes contract and become less numerous, these multiform cells assume the appearance of pavement epithelium. Only slight traces of the pale, delicate, fibrillary meshwork of the normal mucosa are now to be found in this layer. The cells seem to be united by a finely granular intercellular substance.

I have dwelt upon the cell proliferation which takes place in the upper layers of the mucous membrane and which we have found even in its menstrual condition, where the lower layers were hardly engaged in the process; but after conception the development of the cells both in numbers and size is found throughout the entire membrane, though still much more marked in the upper than in the deeper strata. This increase in volume, together with a change of form, steadily progresses, so that in the third and fourth month individual cells near the surface of the membrane already assume the epithelioid appearance; and this, be it remembered, at a time when the glands are as yet distinctly visible, lined with well-preserved epithelium, and when the surface too, at least partially, retains its epithelial cells normal in character and arrangement.

At this period, therefore, when the limits of the interglandular tissue on one hand, and those of the glands and of the surface epithelium on the other, are still well defined, it is readily apparent that the large cells above described belong to the interglandular tissue proper. If seen only in the latter months of pregnancy their origin would, indeed, appear somewhat obscure, as they have then assumed an epithelial character almost throughout, and the tension of the vera, together with the pressure of the growing ovum upon its surface, have transformed the upper strata of that membrane into a uniformly dense layer, in which the laminae of the glands have become very indistinct, and the glandular as well as the surface epithelium has almost entirely disappeared.

In extra-uterine pregnancy, and in the non-pregnant horn of a uterus bicornis or bilocularis, the cells of the interglandular tissue assume the peculiar character described, while at the same time the uterine glands and their laminae are well preserved. But even in the normally developed decidua vera we can with certainty trace the laminae of the glands and their ostia in this layer, called by Friedländer the "large cell stratum," and ascertain that these large cells are distinctly interglandular. During the fourth month the openings of the glands, though, indeed, very indistinct, may still be recognized as delicate, oblique, somewhat funnel-shaped depressions, their ducts appearing as oblique fissures mostly narrowed so as almost to bring their walls in contact.



The lower stratum of the decidua vera, which presents that spongy lamellar structure, appears under the microscope as a network composed of several layers of superimposed meshes, mostly rhomboidal in form, whose longitudinal diameter is parallel to the uterine surface, and greatly exceeds the vertical after the end of the fourth month, as seen in Fig. 8, DR.

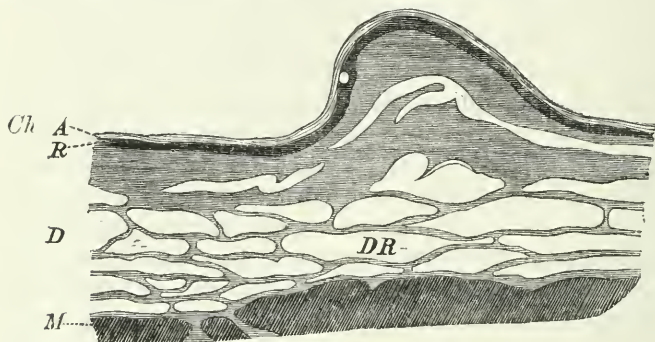


FIG. 8. Vertical section of the maternal and foetal membranes in the last month of pregnancy.  
Lettering as in Fig. 7.

The trabeculae of this meshwork, which in the fourth month have a thickness of 0.00196 inch (0.05 mm.), gradually diminish in size during the subsequent months of gestation, until they are at length reduced to 0.0004 inch (0.01 mm.) or less in their transverse section. They are generally thicker in the upper layers of the meshwork than in the lower, and are directly continuous with the upper cellular stratum of the vera.

That these interstices originate in the glandular ducts and sinuosities is abundantly proven by their contents and the lining of their walls, as well as their development up to the fifth month, after which time there is no farther change.

Friedländer asserts that these spaces are lined with a single layer of very perfect epithelial cells, partly pavement and partly cylindrical. This I cannot assent to, as it is contrary to the facts observed. We have seen that even in the vera of the third month the glandular epithelium has been lost in some of the enlarged and sinuous ducts, and finely granular mucous masses are found in its place, which are probably the product of a disintegration of its cells. As gestation progresses, after the third month, the glandular epithelium gradually disappears



or undergoes the above change; the upper layers of the membrane are first involved, so that in the fourth month an epithelial lining can be found in but few of the glandular fissures; it then begins to disappear in the sinuosities of the deeper strata, and during the last month of pregnancy it is only in the fundi of the glands and in the very deepest meshes, close upon the muscularis, that the epithelia are preserved.

The interstices of the meshwork contain masses which represent the various stages of transition from well-preserved epithelium to the granular conglomerations. The glandular epithelium which, during menstruation and immediately after conception, is higher and larger than the membrane at rest, becomes shorter, more clumsy and flattened as the laminae of the glands are transformed into the compressed sinuosities of the network, so that toward the termination of pregnancy we do find the epithelia in the form of comparatively flat cells, reminding one of pavement epithelium, partly adherent to the walls of the interstices and partly detached.

At the same time some of the cells seem to undergo a very peculiar metamorphosis (Pl. I., Fig. VI. *b*). In those meshes in which the cells of the lining epithelium are already flattened, certain odd formations begin to appear between the individual cells, either singly or in groups, and partially adherent to the walls of the spaces as if belonging to the lining structure. They are very bright, generally cylindrical bodies, which are either wholly homogeneous (Pl. I., Fig. VI. *a*) or reveal more delicate and transparent globules imbedded in their mass (Pl. I., Fig. VI. *c*). The larger of these bodies contain several such globules of varying size, the smaller only a single one. The intermediate stages found and the existence of these bodies between well-preserved, adherent epithelia, as well as the regular arrangement, especially of the smaller ones, along the walls of the meshes, all seem to indicate that they originate from the epithelium of the glandular spaces, and are the product of a retrograde metamorphosis of its cells (Pl. I., Fig. VI.).

The fundi of the glands do not participate in the changes which take place in the glandular ducts and their epithelia. We have seen that during the menstrual period and in the first months of pregnancy the appearance of the fundi is but little altered, some being, perhaps, slightly enlarged. We find them

in the same condition throughout the latter period of gestation ; a change in the direction of their axes, which are always more or less inclined to the perpendicular direction of the ducts, however, becomes noticeable during the fourth month, after which time they lie almost parallel to the surface of the muscularis. The epithelium of the fundi is not destroyed, but its cells appear compressed, broader, and shorter.

The interglandular tissue in the trabeculæ of the meshwork consists of large embryonal cells, similar to those of the uppermost layers ; though in this stratum they are longer and narrower, and do not show that marked increase in size, nor do they undergo the same transformation toward the termination of pregnancy as those of the upper layer. (Pl. I., Fig. V.)

As we approach the muscular stratum the intercellular fibres of the connective tissue become more numerous, and finally interlace with those surrounding the muscular fasciculi.

The vessels supplying the upper layers take their course through the trabeculæ of the network, and it is hence irregular and tortuous ; their laminæ are no longer as large as we have found them during the earlier months, and as gestation approaches its termination they continue to diminish in size as well as numbers.

#### DECIDUA REFLEXA.

The decidua reflexa persists as a distinct membrane throughout the duration of pregnancy, and remnants of that structure are found upon the surface of the chorion after expulsion of the fully developed ovum. Some authorities assert that the reflexa disappears during the latter months of pregnancy, probably because its very delicate tissue, then adherent to both chorion and decidua vera, is blended with each and no longer so distinctly appears as a separate structure. If we, however, follow the development of the foetal membranes we can readily continue to trace the decidua reflexa to the very termination of pregnancy, and it becomes apparent that the tissue which envelops the surface of the chorion, after the agglutination of the ovum to the vera, is that of the reflexa.

We have seen that in the fourth month the ovum completely fills the uterine cavity, and its surface is in close contact with that of the decidua vera, though in no way united with it.

Sections made through the foetal membranes at that period show the outer surface of the chorion covered with a layer of large cells similar to those in the upper strata of the vera, whose surface is intact; the atrophied, widely separated villi of the chorion are imbedded in this layer of cells, which represents the reflexa; traces of glands or blood-vessels I could not discover in this delicate structure.

No farther changes take place, and the reflexa retains this appearance to the end of gestation. After the fifth month its outer surface becomes closely united to that of the decidua vera, but even then both membranes, notwithstanding their identity of structure, can be readily distinguished under the microscope by certain characteristics of the reflexa. The tissue of the reflexa imbibes coloring matter more readily than that of the vera, and whether the specimens have been stained, or simply hardened in the yellowish chromic acid or bichromate of potash, we find a stratum of somewhat deeper color immediately beneath the chorion, which is that of the reflexa. The cells of the vera also are more densely crowded, more compact and flattened towards the surface. In many specimens we can detect a delicate line of demarcation between the two deciduæ.

The villi of the chorion have not entirely disappeared, as is maintained by some, but are seen here and there imbedded in the tissue of the reflexa, thus serving to identify that structure; they are those "bright, spherical spaces," which Friedländer describes in the most superficial layers of the vera, directly beneath the chorion, apparently ignorant of their origin or character.

A farther proof of the continuance of the villi we find in ova of from three to nine months' pregnancy, in which we can separate the decidua reflexa from the chorion near the placental margin, owing to the greater thickness of the reflexa at this point; and in carefully removing it we observe delicate yellowish-white, sometimes branching threads, passing obliquely from the outer surface of the chorion to the inner face of the reflexa, their extremities imbedded in its tissue. These thread-like fibres, which are more numerous near the borders of the placenta, are villi of the chorion, and were formerly erroneously interpreted as obliterated blood-vessels.

The connection between the vera and reflexa is not a very in-

imate one, as indicated by the existence of a line of demarcation, but a mere agglutination of the mechanically approximated membranes. The complete absence of fibrous elements in the contiguous surfaces suggests an agglutination by the same homogeneous intercellular substance which serves to unite individual elements of their tissue.

Both membranes are at that time denuded of their surface epithelium; this is already lost from the outer surface of the reflexa at a very early date when still preserved upon the vera; from the surface of the latter it does not disappear until the ovum has come into close contact with the uterine walls throughout. In several specimens from a gravid womb of the fifth month, in which partial agglutination between the vera and reflexa had already taken place, we found the membranes still separated here and there by a thin layer which was delicately striated in some places, and finely granular in others, and which, from its appearance and position, we could only interpret as a remnant of the disintegrated surface epithelium. There were no indications of previous hemorrhage which would have justified other conclusions.

#### DECIDUA SEROTINA.

As regards the serotina, it is necessary to give a precise definition of the terms used, since the relation of that membrane to the surrounding structures is very much changed after its close union with the chorion in the fifth month. We now distinguish two parts: the placenta uterina and the serotina proper. The placenta uterina is the dense upper layer and the tissue developed from this, which interlaces with and grows around the villi of the chorion; it is that part which, with the villi of the chorion, forms the placenta, and is expelled as such. The serotina proper is the lower stratum, that part of the uterine mucous membrane which underlies the placenta, uniting it to the muscular layer, and which, after the separation of the placenta, remains attached to the uterine wall. The serotina proper is a meshy, lamellated tissue, forming a thinner layer than the corresponding lower stratum of the vera; transverse sections accordingly reveal a network of but very few meshes in thickness. In structure they are identical with those



of the vera ; the fundi of the glands also present an appearance similar to those of the vera ; they are parallel to the surface, are not enlarged, and retain their epithelium. Above this meshwork formed by the enlargement of the glandular spaces is the thin, denser stratum, in which here and there narrow fissures and small interstices appear. This upper portion of the original serotina takes part in the formation of the placenta, and cannot well be isolated ; the processes which we have seen upon the serotina as a proliferation of its sub-epithelial tissue giving first an uneven, then a villous appearance to the surface, have more fully developed and extend into the foetal placenta to over half its thickness, enveloping the individual villi as well as the groups of cotyledons in delicate sheaths of its tissue. Parts of the maternal membrane are thus inseparably interwoven with the foetal villousities ; but those processes which form the placenta uterina do not extend through the entire depth of the foetal stratum to the surface of the chorion itself ; and in this I can but confirm the statements of late investigators.

The fibrous reticula are closer and more prominent in the serotina than in any other parts of the decidua. In the later months but few traces of the surface epithelium remain in the shape of scarce recognizable cells adherent to the villi here and there ; the glandular epithelium too has disappeared, except in the fundi, and nothing remains in the meshes but disintegrated portions, and those opalescent glassy bodies forming larger conglomerations.

The large cells with finely granular protoplasm and large well-defined nuclei, of which several are either bunched together or regularly distributed, seem to belong to the villi of the chorion, apparently to their growing terminal filaments. These elements are often found wholly isolated in the tissue of the serotina, which is probably due to the fact that the connection between the terminal buds and the fully developed branches, from which they spring, is a very loose one, so that they are easily torn from the stem by the teasing of a specimen.

#### RETROGRADE METAMORPHOSIS, MICROSCOPIC.

We have seen that the foetal as well as the maternal membranes undergo a change toward the termination of pregnancy.



which becomes apparent by the opacity of the former and the yellowish tint observed in the latter. Microscopic examination reveals a fatty degeneration, amounting almost to total disintegration of the cells in chorion and amnion, less marked and uniform in the decidua. This retrograde metamorphosis is initiated in the decidua in the last month of pregnancy, but is observed in the foetal membranes at a much earlier period, and at term becomes more intense than in the maternal tissue.

It is impossible to fix the precise period at which the metamorphosis begins, unless a very large number of post-mortem examinations can be made, as the same change is observed in ova prematurely expelled, so that whenever this degeneration of the membranes is found in a gravid womb, I could not say whether it was the result of the physiological process or a pathological condition which would have resulted in abortion had life been preserved.

In the decidua vera this degeneration mainly involves the large cells of the upper layer, those of the lower stratum being less affected. In the serotina it is also well marked and even more fully developed than in the vera.

Though found in cases of abortion and premature delivery, I am not prepared to say that the fatty degeneration of the foetal membranes is the main or primary cause of the separation and expulsion of the ovum.

#### CONCERNING THE EXPULSION OF THE DECIDUA.

*At Term.*—Having studied the development, structure, and connections of the decidua throughout the entire period of gestation, we will now examine the changes which take place during labor, which of these connections are severed, and what part of the maternal membranes, if any, is expelled with the ovum. We are fortunately in possession of all the specimens desirable for a satisfactory investigation of this much-disputed point. Our results are derived from the examination of, 1, aborted ova; 2, after-births at term and in cases of premature delivery; 3, the uteri of women who died during or immediately after delivery.

In the afterbirth we must carefully examine the outer or maternal surface; the layers of the serotina adherent to the

placenta and the shreds of vera and reflexa upon the chorion will indicate the time of separation between the ovum and its maternal envelope, showing how much of the latter has been carried away with the expelled contents.

The foetal envelope is an opaque, whitish membrane, 0.02–0.03 inch (0.5–0.75 mm.) in thickness, with an even, smooth inner surface toward the foetus; the outer surface, however, is soft and irregular, from the adherent shreds of a pale yellowish tissue. It consists not only of the membranes proper of the ovum, the chorion, and amnion, but also of the reflexa, with parts of the decidua vera; these latter are from the upper cellular layer of the vera, either part of the tissue only, or the stratum in its entire thickness; being forcibly torn from their natural connections, they present a soft, villous surface, with the pale yellowish color, due to the fatty degeneration of the vera.

I shall refer only to two of the uteri examined with regard to this point which were most characteristic: one, the uterus (a uterus bicornis) of a woman, who, surprised by sudden and severe labor pains, died in the street from hemorrhage before the ovum was expelled; the other from a woman who died immediately after a very tedious instrumental delivery.

The cavity in both cases was lined with a delicate, soft, convoluted membrane, of a yellowish color, the surface of which showed small, shallow, yet well-defined depressions. In endeavoring to remove this soft, but apparently dense membrane from the walls of the uterus, it was found united to it by a deeper layer of spongy, laminated tissue, which remained, with almost its entire thickness, upon the muscular stratum, a few of its meshes only being separated with the upper layer.

Microscopic examination revealed in this membrane the well-known structure of the decidua vera, with fatty degeneration of its elements throughout, and deprived in part of the more superficial of the large cells of its upper layer. The depressions on the surface corresponded to the glandular fissures here laid open by the removal of the superficial layer. The mesh-work of the lower stratum was in the first case 0.039 inch (1.0 mm.) in thickness, but in the second 0.078–0.118 inch (2.0–3.0 mm.), being abnormally developed. The epithelium in the fundi of the glands was perfectly preserved in both.

Remnants of the decidua were also found in the uteri of

women who died hours, and even in those who died several days after delivery; in but few was the membrane so perfect or so distinctly perceptible as in the two cases cited, inasmuch as the structure is covered with coagulated blood or inflammatory products which penetrate its tissue more or less, and cannot be removed without partially, at least, destroying the membrane itself.

So also remnants of the mucous membrane are always found upon that part of the uterine wall to which the placenta was attached, if no pathological condition intervened and the separation of the placenta has been spontaneous. In this place, however, the dense upper layer of the serotina, forming part of the placenta uterina, is generally detached, and expelled *in toto*, as a part of the afterbirth; the loose mesh-work of the lower stratum alone remains upon the muscular layer, and generally escapes the observer, being concealed beneath the numerous thrombi which protrude from the uterine vessels; moreover, the tissue at this place is generally infiltrated with blood, and the few remaining meshes are still more obscured by being felted with the adherent coagula.

I must add that in most cases the shreds which are found adherent to the womb after parturition, and are generally looked upon as parts of the fœtal membranes proper, are not such, but membranous shreds of the decidua—its partially detached upper stratum. I have myself seen this mistake made in post-mortem examinations in well-known institutions. It occasionally does happen—even after normal labor and spontaneous expulsion of the ovum—that shreds of the chorion remain in the uterine cavity for days and weeks attached to the still firmly adherent decidua. The fatty degeneration of the tissue, which is most intense in the upper layers of the vera and serotina, may well be the cause of their easy separation from the structures with which they are fused.

To recapitulate: the parts of the maternal membranes adherent to the ovum, expelled in normal parturition at term, are the whole of the decidua reflexa, the entire upper cellular layer of the serotina, and the more superficial parts of the upper stratum of the vera; the greatest part of the vera and the spongy lower layer of the serotina remain *in utero*.

*In Abortion.*—It now remains for us to examine the relation

of the maternal membranes to the aborted ovum, exclusive of evidently pathological conditions. Ova expelled during the first month, and mostly those of the second, have no maternal membranes adherent to them, but show the shaggy, whitish surface of the enveloping chorion; at this period the ovum is often aborted in a very marked pathological condition, together with the whole of the upper layer of the mucous membrane, which closely adheres to it. In the third and fourth months the reflexa, more firmly united to the chorion, is expelled with the ovum, forming a smooth covering over the greater part, of its surface.

In the fourth month parts of the vera may already adhere to the outer surface of the reflexa, and traces of the proliferating tissue of the serotina are found between the cotyledons formed by the tufts of the chorion. The envelopes of ova expelled after the fifth month have all the parts which we have found composing the foetal membranes after parturition at term, or adhering to them, the entire structure being, of course, much more delicate; in addition to the parts found in the previous months, a transparent layer from the superficial stratum of the serotina adheres to the maternal surface of the expelled placenta.

A fatty degeneration of the membranes in aborted ova is seen at a very early date.

I have seen the foetal membranes in the fourth month, when they are still firmly united in the normal ovum, discolored and opaque. Often, too, the fatty degeneration is very complete in those parts of the decidua which remain in utero; yet it is sometimes missing, especially when the abortion has been caused by an injury or some other external influence; in such cases the foetal membranes are quite transparent while the serotina is of a gray or grayish-red color.

In aborted ova we frequently see a shred of membrane, perhaps three-fourths of an inch in width, pending from the edge of the placenta over the outer surface of the foetal membranes; it has a jagged, rent border, as if torn, a shaggy outer surface, and an areolated smooth inner aspect toward the ovum; its color, when not altered by an infiltration with blood, is identical with that of the serotina. The outer surface of this membrane is directly continuous with the serotina at the protruding



edge of the placenta, its inner surface with the reflexa. The areolated appearance of this surface extends beyond the line of fusion to the reflexa, but upon the face of the latter the depressions become distorted and appear more like elongated fissures.

This membrane, as is evident from the description of the surfaces, continuous on the one hand with the serotina, on the other with the reflexa, is part of the decidua vera, and is mostly up to 0.18 inch (4.5 mm.) in thickness.

The agglutination between the approximate surfaces of the vera and reflexa near the placental margin is very imperfect, even during the last months, wholly wanting in some places, and the upper layer of the vera is readily separated from the lower, but directly continuous with reflexa and serotina, so that when the ovum is expelled with these membranes, shreds of the vera are torn away with it, and are not fused to the surface of the reflexa, but freely pendent.

The shreds of tissue which are sometimes discharged from the womb for several days after an abortion, and even after delivery at term, are portions of the decidua vera, in abortions occasionally shreds of the foetal membranes; very rarely are they detached portions of the inner muscular layer.

From the character of the membranes adherent to the aborted ovum, as well as from the examination of uteri after their expulsion, we see that the condition of the maternal membranes in abortions after the fifth month is precisely the same as at term, the greater part of the vera remaining *in utero*; previous to the fourth month the entire membrane is preserved and a deeper layer of the serotina remains than is found after parturition; in many of the abortions of the first and second month the maternal membranes, on the contrary, suffer more than at any other period, only the deepest strata of vera and serotina remaining *in utero*.

## 2. *Appearance of the Decidua Enveloping the Aborted Ovum.*

We can readily recognize the individual membranes in ova expelled after the third month, but it is frequently a very difficult matter to distinguish the separate tissues in ova aborted during the first and second month, as they are generally in a more or less pathological condition.



The ova are not only very much disfigured, and their appearance greatly altered by hemorrhage in and between the various membranes, but they are often so deformed by the disintegration of the embryo and the abnormal development of the surrounding membranes, that it is impossible to identify the various parts without a thorough knowledge of their normal condition, and the pathological changes to which they are subject.

In abortions during the first weeks after conception a triangular shaggy mass is sometimes expelled corresponding in form and size to the cavity of the womb at that period; it is a pouch, which is either completely closed or possesses only a small opening with ragged edges at its apex, and is of a deep dirty-red color. The walls of this sac are from 0.118–0.157 inch (3.0–4.0 mm.) in thickness, soft, succulent, intensely injected and infiltrated with blood, of a rough villous exterior, while the inner surface is smooth, somewhat puffy and cribriform. Upon this inner surface, near the broader base, we find a spherical protuberance of the same deep-red color, which, upon section, proves to be a second smaller pouch adherent to the walls of the first, and enclosing an opaque bloody fluid, often with a partially decomposed embryo only a few millimetres in length. These sacs then are a product of conception; the larger outer one consists of the upper layer of the entire mucosa vera and serotina, which has been expelled as a complete membrane enclosing the ovum with the reflexa, the projecting pouch upon its inner surface. In such cases the entire vera is prematurely and abnormally developed; the interglandular tissue already contains the large characteristic cells which are not usually found until the latter months of pregnancy.

The arrangement of these epithelioid cells has, however, been disturbed; they have been separated in many places by extravasations of blood, which are more or less extensive throughout the entire tissue, and often appear upon the borders of larger extravasations or surround smaller effusions in concentric layers. This is the form of aborted ova from which the belief originated that the decidua vera was a closed sac, as the narrow openings which should correspond to the tubar ostia are always closed by coagula, and generally the space of the internal os as well. This occlusion is indeed deceptive, and cannot so readily be distinguished from real fusion of the tissue, since the vera itself,

thoroughly infiltrated with blood, has the same consistency and the same deep-red color as the coagula closing the ostia.

A form of ova greatly resembling these are such as are expelled with only parts of the vera attached ; but in all of these cases the decidua is in a pathological condition prematurely hypertrophied, infiltrated, and distended, by extravasations of blood.

The ova, which are aborted toward the end of the second and in the third month, even in the beginning of the fourth, generally show the smooth outer surface of the enveloping reflexa with jagged shreds of the vera adherent to the base of the former membrane. These ova vary in size according to their period of development and their pathological condition. We often find an accumulation of serum or blood in the cavity of the amnion or between amnion and chorion, or chorion and reflexa ; most common is the extravasation of blood between amnion and chorion. The embryo has mostly died long before the expulsion of the ovum, and is apparently retarded in development, or the delicate tissue has been wholly absorbed, leaving no trace of the embryo, and but little of its proper membranes. Ova expelled at this time, which have better preserved their normal appearance, are round or oval in shape, covered throughout their greatest extent by a smooth, reddish-gray membrane, mostly infiltrated and impregnated with blood ; it is somewhat thicker, 0.04–0.08 inch (1.–2. mm.), than the membrane enveloping the ovum at a later period ; upon the borders of that part of the ovum which was adherent to the uterine wall it is continuous, with shreds of a firmer membrane pending from its sides. At the point itself where the ovum has been detached the villi of the chorion are freely exposed ; in other cases the tufts are so closely cemented by coagulated blood that the smooth and rounded surface of the ovum appears unbroken even at this point ; if the coagulum is large the ovum becomes pear-shaped. Upon section the coagulum with the clotted villi presents a placenta-like appearance.

Still another variation which we find in the shape of the ova is due to a conical protuberance, an elongation, as it were, of the lower, reflexa-covered pole of the ovum produced by coagula in or upon the reflexa, but always attached to its most pendent, lowest point.

These conical elongations of the lower extremity of the ovum are often of considerable length, and are even constricted in places, as if by contraction of the internal os; they generally originate in an accumulation of blood in the lowest portion of the sac formed by the reflexa which is distended and the membrane itself impregnated with blood. This blood, however, does not seem to come from the vessels of the reflexa, as the extravasation in these cases extends between chorion and reflexa up to the former point of attachment of the ovum, while the tissue of the reflexa itself shows no extravasations or other signs of hemorrhage, and is only impregnated with blood where in contact with the coagula.

An early hemorrhage greatly alters not only the shape of the ova, but also their general appearance, color, and consistency. The structure can still be identified, its various parts recognized, and the extent of the membranes traced by the adherent shreds of the vera.

Such is the character of the ova aborted up to the fourth month, as we generally see them; the more normal ovum, as it is often expelled during the first two months, a delicate vesicle surrounded by the villous chorion, is mostly lost amid the clots of blood discharged, and is hence but little known.

### 3. *The Decidua in the Uterus Bicornis and in Extra-uterine Pregnancy.*

It is well known that a tumefaction of the mucous membrane of the womb takes place after conception, even when it does not serve to nourish and protect the ovum, as in the non-pregnant horn of a uterus bicornis or bilocularis, and in cases of extra-uterine pregnancy a decidua is formed which bears a close resemblance to that of the normal womb; I will only refer to certain points revealed by the examination of such uteri, which have a direct bearing upon the questions discussed.

The uterine mucosa at term, in such cases, corresponds in its development to that of the decidua vera in the third month of normal pregnancy, at which period the surface of the decidua is still intact, not as yet compressed by the expanding ovum, nor united to the approaching surface of the reflexa. The resemblance is perfect as regards the general appearance, the sieve-

like surface, the degree of tumefaction, the enlargement of the glands and the proliferation of the interglandular tissue. The difference existing is that the cells of the interglandular tissue assume the same epithelioid appearance which we have found in the upper stratum of the normal vera only during the latter month of pregnancy, whereas the glandular ducts and the intervening tissue of the lower stratum undergo no farther changes, so that we do not have that lamellated meshwork characteristic of the deeper layers of the normal vera; the glandular epithelium is comparatively well preserved.

The possibility of the fixation and growth of the ovum and the development of the placenta upon tissues other than the mucous membrane of the womb can, I think, be rationally explained only upon the basis of the facts demonstrated:

1. *That the decidua, to which, in ordinary pregnancy, the chorion is attached, is not an epithelial structure, but connective tissue.*

2. *That the villi of the chorion are only fused with the proliferating connective tissue of the most superficial stratum of the serotina, and do not enter the uterine glands; that the villi have no necessary but merely an occasional and incidental connection with the glandular structure.*

The occurrence of extra-uterine pregnancy, in which the ovum is surrounded by tissues not of an epithelial character and forms its attachments upon structures which possess no glands, crypts, or follicles, serves on the other hand, to corroborate my results.

#### D. THE REGENERATION OF THE MUCOUS MEMBRANE AFTER PARTURITION.

During the first week after parturition the appearance of the inner surface of the womb changes but little.

It is at first covered with a layer of bloody mucus, which assumes a yellowish-red color and becomes less fluid toward the fourth or fifth day. After carefully removing this more or less tenacious mass, while the specimen is immersed in water, a soft, velvety, yellowish-red membrane is found lining the uterine cavity; it has a thickness of 0.04–0.08 inch (1.–2. mm.), and generally a somewhat uneven meshy surface.



That part of the uterine walls to which the placenta was attached presents a very different appearance, owing to the irregular protrusions caused by the thrombosed vessels, but its surface is covered with a very delicate layer of the same yellowish-red, filmy membrane which lines the rest of the cavity.

Microscopic examination again reveals the familiar structure of the decidua, though in a somewhat altered condition; the dense upper layer is for the most part wanting, and the laminated structure of the spongy lower stratum is exposed; the glandular spaces of the meshwork are filled with blood, its tissue, full of hemorrhagic effusions, is infiltrated with young, round cells, but still shows those large, here more fusiform cells, characteristic of the decidua in an advanced state of fatty degeneration, extending even to the deepest layers.

At this period, then, the lower, more fibrous stratum of the uterine mucosa, in which the type of connective tissue is readily recognized, is exposed. The large cells peculiar to the decidua gradually disappear from the tissue; their connection with the surrounding elements has ceased; they are disintegrated and carried away. The bloody mucus covering those remnants of the decidua contains round cells, detached, fatty, decidua cells and fragments of these, free nuclei, fat granules, and some few epithelia, and, of course, blood-corpuscles in great quantities. The number of cells and the fatty detritus in this fluid give it the yellowish tint which becomes more apparent as the quantity of commingled blood diminishes.

In the second week after delivery only a very thin film, hardly to be called a membrane, is left upon the inner surface of the womb, and it already begins to assume the appearance of an active granulating tissue, although still retaining the well-marked yellowish-red color. The placental site is less uneven, the thrombi are smaller, more resistant and discolored, and the mucus upon its surface has less consistency, and contains fewer cellular elements than that in other parts of the uterine cavity.

The greater part of the decidua has now been expelled with the continued discharge, and only the deepest layers remain; these are thoroughly infiltrated with round cells, and but few of the fatty disintegrating decidua cells are to be seen.

This dissolution of the interstitial tissue lays open the deep-



est of the glandular space and even the fundi, which, as Friedländer correctly states, are no longer parallel to the surface of the muscular layer, but still very oblique.

Although a new formation of surface epithelium is not yet inaugurated, the tissue is no longer wholly devoid of epithelial covering, inasmuch as the exposed fundi, in which the glandular cells have been preserved, now form part of the surface, and their epithelial lining appears as that of the surface. An active cell-proliferation now begins in the remnants of the glandular epithelium, as we see by the segmentation of its nuclei.

In the third week after delivery the inner surface of the womb is in some cases already lined with a very thin smooth membrane of new formation, which is still covered by more or less of a pale-yellowish mucus, sometimes streaked with blood.

The new membrane appears to be of a yellowish color when seen *in situ*, owing to the transparency of the delicate film which transmits the fatty color of the underlying muscular tissue; here and there it exhibits a very decided pigmentation of ochre-yellow, brownish-red and even black.

Upon microscopic examination the structure superimposed upon the muscularis is found to be a layer of connective tissue up to 0.006 inch (0.15 mm.) in thickness, rich in young formative cells; its surface is protected by a delicate epithelium, and the developing glands are represented by short crypts or sinuous ducts. The placental site alone is at this period still devoid of a surface epithelium.

This was the appearance presented by two of the uteri of the third week; in a number of others which were examined at that period the condition of the lining membrane was somewhat backward, corresponding more to the description given of its structure in the second week; but as many of these *post-partum* specimens were obtained from patients whose death had been caused by acute disease, pneumonia, pericarditis, meningitis, etc., and not from cases of sudden death from injuries, I am safe in stating that the restitution of the uterine mucosa takes place as early as the third week after parturition, at which time the membrane, though frail and delicate, is not over 0.006 inch (0.15 mm.) in thickness, is perfect in its elements, the glandular structure, interstitial tissue and surface epithe-

lium being all represented. The farther development of the interglandular tissue which is accompanied by the elongation and extension of the ducts (thus again bringing them into a vertical position) and the proliferation of the glandular epithelium complete the restitution of the mucous membrane of the womb to its normal condition.

In a uterus of the fifth week after parturition the mucous membrane had already attained a thickness of 0.0315 inch (0.9 mm.), the substratum of interglandular tissue contained an excess of round cells, but was in all other respects of normal appearance, the glands vertical and extended, lined with normal epithelia, 0.0007 inch (0.018 mm.) in height.

Upon the placental site I have in many cases found the regeneration of the membrane to progress more slowly; in the two apparently normal cases of the third week, and in one of the fourth, in which, in other parts of the womb, the young membrane was protected by a surface epithelium, no traces of it had as yet appeared upon that part of the uterine wall before occupied by the placenta.

This involution of the uterus and the restitution of the mucous membrane may be greatly retarded by disease, as is evinced by an examination of cases which have lingered with chronic affections, in whom the inner surface of the womb presents a granulating appearance for months after parturition, and is found to consist of young granulating tissue, partly again in fatty degeneration without any trace of glandular structure and surface epithelium.

Such cases would lead us to infer that, although the patient may recover and the disease be cured, the uterine mucosa is in them never perfectly restored.

If the whole of the decidua should be detached, either in consequence of disease, or of tedious instrumental delivery, a perfect mucous membrane could never be reformed; the substratum might be developed from the interstitial connective tissue of the muscularis, but would then line the cavity of the womb as a cicatricial layer of connective tissue, devoid of surface epithelium or glandular structure, as those elements had been destroyed from which a new mucous membrane might be developed. If totally expelled the mucous membrane could not be regenerated; but in those cases, whether tedious instrumen-

tal delivery, or spontaneous expulsion of the ovum, in which the membrane is *apparently* expelled *in toto*, the lowest meshes with the fundi of the glands, or, in extreme cases, only the exposed fundi, still some part of the mucosa remains *in utero*; the muscular fibres are not laid bare, and restitution is readily explained.

The decidua undergoes the same process of disintegration and restitution after abortion and premature delivery as it does after normal parturition. After abortions which occur during the first four months of pregnancy the fatty degeneration of the decidua cells is confined more to the upper layers, and the restitution of the mucous membrane to its normal condition is more rapidly accomplished.

The yellowish, brownish, red and black pigmentary deposits existing in molecular form, or in opaque globules in the tissue of the mucous membrane, are characteristic of deep-seated hemorrhage, and may be looked upon as an almost conclusive proof of recent delivery, whether the ovum has been expelled at term, or aborted in the early months of pregnancy. After menstruation no such pigmentary deposit is found, probably on account of the superficial character of the hemorrhage.

### III.—THE MUCOUS MEMBRANE AFTER THE CHANGE OF LIFE.

With the change of life the mucous membrane of the uterus, which we have seen in a state of continual physiological activity from the time of its complete development at puberty, begins to atrophy. The substratum of connective tissue becomes more fibrous, dense, and homogeneous; its cells contract and lose their succulence, the glands narrow, and are obliterated either entirely or in part. Small cysts are often formed by the partially obliterated ducts, and, when near the ostia, they appear like delicate, glittering dew-drops scattered over the otherwise smooth surface of the now thin, hard membrane.

### PLATE I.

FIG. I.—Transverse section of the inner surface of the pregnant uterus at term, from a woman delivered by Cæsa-rean section immediately after a sudden death, showing the maternal and foetal membranes in their natural relation:

Fig V.



Fig VI

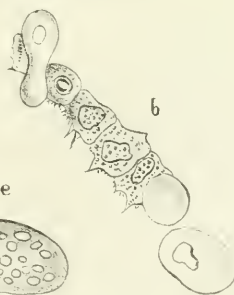
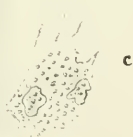






Fig IV.



Fig I.

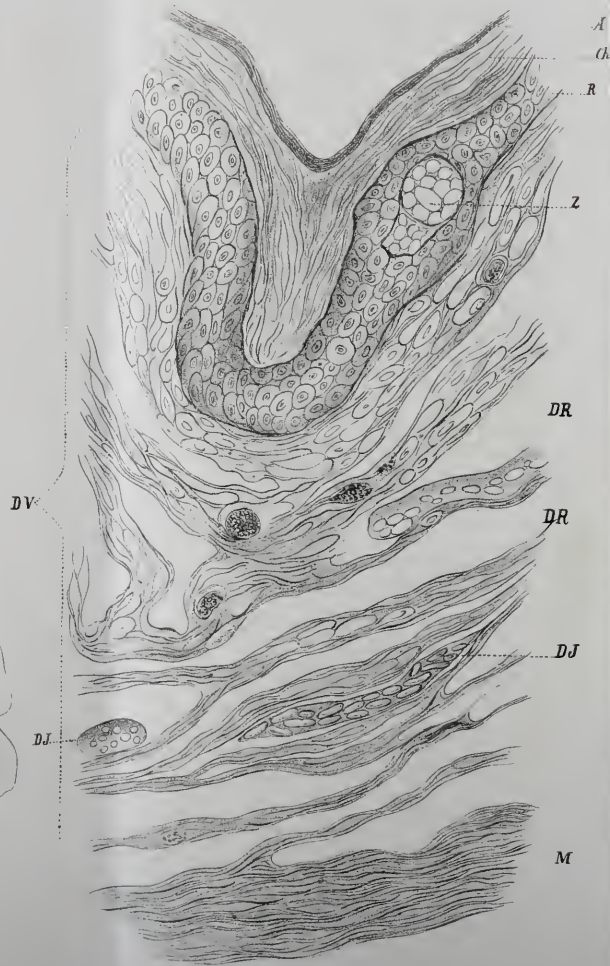


Fig V.

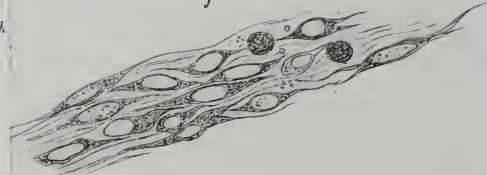
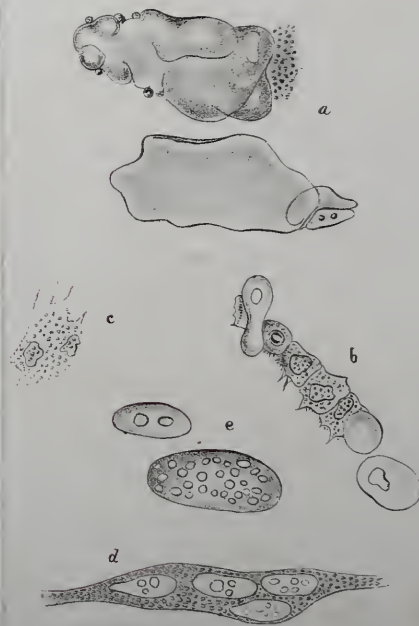


Fig VI





- |                          |                                       |
|--------------------------|---------------------------------------|
| A. Amnion.               | DV. Decidua vera.                     |
| Ch. Chorion.             | DR. Glandular spaces.                 |
| R. Decidua reflexa.      | DI. Contents of the glandular spaces. |
| Z. Villi of the chorion. | M. Muscularis.                        |
- (Hartnack, Obj. VIII., Ocul. III.)

FIG. II.—Transverse section of the placenta, foetal, and maternal membranes, and the uterine wall from the same specimen. Natural size.

Pl. Placenta.

m. muscularis in its entire thickness.

Other letters as above.

FIG. III.—Section of the uterine wall, with maternal and foetal membranes at some distance from the placental attachment. From same specimen. Natural size.

FIG. IV.—The large (epithelioid) cells from the upper stratum of the vera (b), and from the reflexa (a), from a uterus in the ninth month of pregnancy.

(Hartnack, X immersion. Ocul. III.)

FIG. V.—Interglandular tissue from the meshwork; the trabeculæ.

FIG. VI.—Contents of the glandular spaces; metamorphosed glandular epithelium.

- a. Large amorphous glassy globules.
  - b. Glandular epithelium in process of transformation to the semi-transparent globules.
  - c. Granular corpuscles and nuclei, representing the remnants of disintegrated epithelium.
  - d. Glassy globules imbedded in the granular mass.
  - e. Free globules containing smaller transparent bodies.
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ON SOME OF THE APPARENT PECULIARITIES OF PARTURITION  
IN THE NEGRO RACE, WITH REMARKS ON RACE PELVES  
IN GENERAL.

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late Obstetric Physician to the Freedmen's Hospital ; one of the Physicians to  
St. John's Hospital, etc., etc.

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HAVING attended several hundred negro women in their confinements, and meeting with surprisingly few preternatural or instrumental cases among them, I have been interested in making some further investigations upon this subject, and have collected the histories of two thousand negro labors for that purpose. I propose in this paper to institute a comparison between the proportion of "puerperal accidents," cases requiring instrumental interference, length of convalescing stage, with the obstetric average of corresponding cases in white women, as given in our text-books upon midwifery ; and to briefly discuss the causes of the immunity of negro women from these puerperal complications, particularly the anatomical formation of the negro female pelvis, the diameters of the negro foetal head, and the habits of life of those women whose histories are given below.

I had collected for comparison two thousand one hundred labors in negro women ; but as the average in medical literature is given as so many in one thousand I shall calculate upon two thousand only as my basis.

Five hundred of the cases referred to have occurred in my own practice, and I am indebted to Drs. C. B. Purvis and A. T. Augusta, two busy colored physicians, for the histories of about eight hundred more, while the remainder, or seven hundred cases, occurred in the practice of colored midwives, or "grannies" as they are called, mostly among the former slaves of the South. Where there exists so large a colored population as in our city—and it reaches at the present time to upwards of 40,000—and where that population, as in our case, is mostly very poor, physicians are not called in to attend upon confinements unless emergencies arise requiring greater skill

than is possessed by the ordinary colored midwife. These women have no anatomical or physiological knowledge of the "mechanism of natural labor," or the science of obstetrics, but they have accumulated a vast deal of experience from the hundreds of cases which are unreservedly entrusted to their care.

The statistics of Churchill, so universally adopted by obstetrical writers, will be used as the standard of comparisons among white women, in this article.

*Length of Labor.*—Twenty-five of these two thousand labors lasted beyond twenty-four hours. How much longer some of them may have continued I have no reliable facts to show. None of my own five hundred cases lasted a greater time than above stated, and I have no accounts of a single death from prolonged labor. Especial inquiry was made upon this point among the "grannies." It is very difficult to bring them to exact facts. They make a distinction between labor and "hard labor." Negro women have frequently told me that their labor began three or even four days ago, but that "hard labor" had only lasted an hour or two. This I do not understand as an indication that the os uteri had been three or four days in dilating before expulsive pains came on, but simply that they considered their "time up," and the occasional pains in the back and uterus which they experienced, were set down as the beginning of their labor. I have been called to attend a number of women who were said to be in "hard labor," and have gone at once with the messenger and found the child kicking and crying in the bed upon my arrival. This also occurs with white women, but so far as my experience and reading goes, not so frequently. The average length of labor in these women, so far as it has been possible to calculate it, was about three hours. Meigs, in his "Treatise on Obstetrics," p. 292, says: "The average duration of labor has been stated at four hours; I should think it greater. There are many examples of women in labor who are completely delivered in ten minutes from the first perception of the signs of parturition. Very numerous cases occur in which labor is protracted during twenty-four hours, while some of the patients are occupied three, four, and even five days, with continuous efforts to bring the child into the world. I have witnessed one labor of nine days' duration, and

many from three to five days." It is altogether probable that Meigs' experience was confined almost exclusively to white women, and to the better classes of society. I should say that the average among white women in my practice had been at least six hours.

The remark of Dr. Livingston in regard to the natives of South Africa considering parturition to be an act of nature and not of disease, applies almost as forcibly to the negro women of the United States. There is a noticeable difference, however, in the manner and bearing of our dashing mulattoes and quadroons.

The multiparous negress will, as a rule, cheerfully bend all her energies toward the accomplishment of the end, which she knows full well has to be accomplished, and, believing it to be a natural process which will do her no harm, hastens through it as rapidly as her powers will allow.

With the mulattoes this is not so much the case. They appear to have less courage or endurance than the pure negroes, and so far as my experience goes, this lack is a notable feature in most of their diseases.

*Presentations, etc.*—The average of *breech presentations* stated by Leishman is "about 1 in every 45 mature births;" Churchill says about one in every  $59\frac{3}{4}$ ; Collins about one in every 68; Meadows one in every  $72\frac{1}{2}$  cases.

It has been impossible to get reliable details from the colored "grannies" in regard to the variety of presentations, *i.e.*, whether they were feet, knees, or breech. I find, however, what appear to have been breech presentations occurring 4 times in the practice of these "grannies." I have met with 2 cases, and have reports of 4 more. In all 10 cases, making this presentation occur in these 2,000 labors once in every 200 cases.

There are no accounts of *shoulder or face presentations*. One of my cases occurred in the Freedmen's Hospital, and presented one foot, one hand, and the funis, with the head so crowded down by a powerfully acting uterus as to render version very difficult. The child in this case was still-born. The facts in my possession do not enable me to give positive statements that the above ratio is correct, but I feel abundantly warranted in the belief, that the head presents with greater uniformity among the negro women of the United States than

among white women as recorded in our text-books upon midwifery.

Churchill, from an analysis of 466,424 labors, found that *twins* occurred once in every eighty-three cases.

In my analysis of 2,000 labors in negro women, I find that twins were delivered once in every four hundred cases.

*Triplets* occurred in my wards at the Freedmen's Hospital, once.

The *Period of Convalescence* in negro women is proverbially short and uncomplicated. My own experience and the statements of physicians and colored midwives who have had abundant opportunities for gaining information upon this point, confirm this proverb, the letters of occasional medical or historical writers travelling in negro countries to the contrary notwithstanding.

It has been impossible to confine patients to their beds for "nine days." They have frequently been dressed and about their rooms on the third and fourth days after confinement. They had pursued this course in previous labors, and had suffered no harm, and thought they gained strength faster than when lying in bed; and it is rare that I am called upon to treat any disease which originated during the puerperal month. *Prolapsus uteri* I have found to be rather common in negro women, but have been in the habit of attributing it to their modes of life, especially to the lifting and carrying heavy burdens, many of them upon their heads and during menstruation. How far the neglect of proper management after confinement may be causative of uterine displacements, I cannot say. From a knowledge, however, of the time required for the completion of the process of sub-involution, it would seem that this must be its chief cause; but so far as my knowledge and experience go, this has not seemed to be the case.

These women invariably nurse their children from healthy and bountiful breasts. They are unwilling frequently to wean them at the end of nine months or a year. I have seen several negro women nursing children over two years of age, and some over three.

*Puerperal Accidents.*—I am unable to give the average of *stillbirths* and *abortions* among whites or blacks. From the published investigations of Dr. H. R. Storer and others upon



abortion, and its criminal increase in America, it is, however, certain that the negro women of the South are far behind their sex among the more accomplished whites in this regard. The most convincing proof of the truth of this statement is in their greater number of children. The loose notions heretofore entertained by the colored people in regard to the sanctity of marriage, has made it little or no disgrace in their eyes for a child to be born out of wedlock. Thus they have not had this powerful incentive for covering their shame, by bringing about abortion. As they have not heretofore been compelled to support their own children, large families were not so burdensome as they prove now to be.

Among the two thousand labors included in this report there were only seven *stillbirths*. I am convinced, however, that abortion during the second and third months of uterogestation is increasing in frequency among the negroes. My impression is, that this practice prevails to a greater extent among the mulattoes than among their blacker sisters, though I have no facts with which to sustain such a statement. Perhaps an increased sense of the responsibilities attaching to the birth of children, and a greater regard for the good opinion of the community, actuates "unfortunate" colored girls to commit this crime, as well as the married women who have either been unfaithful to their husbands, or who are unwilling to care for more offspring.

The *umbilical cord* was found to be once around the neck of the infant at birth three hundred and eleven times, twice around two times, and twice around the body two times, thus making this anomaly occur once in every six cases. Obstetric authors direct that upon the birth of the foetal head the accoucheur should satisfy himself as to whether the funis encircles the neck of the child or not. I presume few practitioners of midwifery neglect this wise precaution, and occasionally they find need for their skill in its removal; but it is so frequently the case among negro women that I always expect to find it, and never fail to search for it.

Churchill estimates that *rupture of the uterus* occurs once in every 1318 cases.

In the 2,000 cases collected by me it occurred but once. In this case the woman recovered, and subsequently became preg-

nant, and would probably have gone on to full time had she not taken "some powders" which produced an abortion at the sixth month.

*Puerperal convulsions* occur, according to Leishman, once in every 350 cases.

In my 2,000 negro labors they occurred 8 times, or once in every 250 cases. This ratio among negroes seems above the average, not only of the whites, but much above their own average. In conversing with physicians upon this point it has, without exception, struck them as large. Dr. Triplett, now of Washington, who has practised until recently in Virginia, tells me that he "never saw or heard of a case of genuine puerperal convulsions in a negro woman." He says, further, that his father and grandfather both practised medicine all their lives in the same State, and had never seen a *single* case. The doctor had "been struck by this fact," and his memory was "clear upon this point."

It is certain that albuminuria existed in two instances, occurring in my own practice, and it was the united opinion of Drs. Smith, Prentiss and myself, that uremic poisoning was the cause of a third, the case already referred to, where forceps and craniotomy failed, and version was finally necessary to deliver the child. Coma was persistent from the seizure until death, a period of eighteen hours. No urine could be drawn for examination; the face, however, was swollen and puffy, and the limbs and feet excessively cedematous.

Meadows<sup>1</sup> gives three varieties, viz., the hysterical, the apoplectic, and the genuine puerperal. The first variety I have never seen in a negro woman. Hysteria, in any of its numerous guises, is rarely met with among them, and the neuroses are extremely uncommon, with the exception of neuralgia of the womb and ovaries, which I have frequently seen.

Of the hysterical variety of puerperal convulsions Meadows remarks as follows: "Most of the patients in whom these convulsions occur have the nervous hysterical peculiarities very strongly marked. They are generally young, often highly intellectual and refined, and at this time are very nervous and anxious about their condition." The strong-nerved ignorant

<sup>1</sup> Manual of Midwifery, p. 396.

negro women of America are little subject to this form of eclampsia.

The case of coma and death above referred to is a good example of the apoplectic variety.

Possibly a cause for this immunity of negro women from this complication is their failure to receive impressions upon their nervous system which would seriously affect a more delicate organization. They do not suffer from "nervousness," one of the fashionable ailments of the present day.

Eclampsia has been noticed to occur more frequently among unmarried primiparæ, whose shame and distress at their "unfortunate" condition, it was thought, had much to do with its causation.

Meadows quotes from Churchill to show that *placenta prævia* occurs about once in 480 or 500 cases.

In my 2,000 cases it occurred but *once*. In this instance the child was expelled immediately after the delivery of the placenta. No subsequent hemorrhage took place, and both mother and child did well.

There were two cases of severe *flooding during labor*. Both occurred in the practice of the "grannies." Ergot had been administered largely, and pressure upon the abdomen resorted to. In one of these cases, there being some delay, the child was born dead; and in the other, the perineum was ruptured down to the sphincter ani. The mothers both recovered. Had either of these cases occurred in the practice of any reputable accoucheur, it is probable that podalic version would have been performed, and the lamentable results of this unadvised and inappropriate use of ergot been prevented.

Churchill reckons *post-partum hemorrhage* as occurring once in 122 cases.

Among the 2,000 cases collected by me this "accident" occurred 4 times, or once in 500 labors.

In one of these four cases the placenta was adherent, but after its detachment and removal the uterus contracted favorably, and the woman did well.

No fatal cases occurred. They were all controlled by cold and pressure variously applied. The "grannies" seem to understand the value of cold water, ice, pressure, the removal

of clots, and ergot, but, as a rule, they anticipate no trouble, and take no precautions.

I rarely attend a labor without having near at hand ice and ergot, ready for instant use ; and it has become almost a rule of practice to administer a teaspoonful of the fluid extract of ergot as soon as the foetal head has been delivered, partly as a prophylactic against hemorrhage, and partly to hasten the expulsion of the after-birth, and secure permanent contraction of the uterus. After-pains are less annoying in cases where ergot is given, and Credé's method of squeezing the uterus adopted. A firmer and more compact consolidation of the uterus is produced than when both or either, of these aids or precautions have been neglected.

Some of these "grannies" in the South gather and prepare their own ergot powder, and the cotton root they make into a "tea," which they use not only to check flooding, but to bring on abortion.

In the 500 colored labors which I have witnessed, the placenta has been quickly expelled, by strong expulsive pains, in a very short time after the birth of the child. This interval has been shorter in negroes than in white women.

The chief danger to a patient, white or black, of flooding occurs in the few moments intervening between the birth of the child and that of the secundines. The danger, so far as my experience goes, is proportionate to the amount of exhaustion and general relaxation, including the uterus, of course, immediately succeeding the last agonizing pains which accompany the passage of the head through the vulva. That nervous system the best able to endure these pains, and which reacts the most speedily, is least exposed. Many of our strong, robust white women, and the apparently delicate ones also, endure these physiological processes nobly and react quickly. There being so few "delicate ones" among the negro women, statistics show that, proportionately, few are affected by post-partum hemorrhage.

*Puerperal fever* occurred four times in my two thousand cases, or once in 500 labors.

It is difficult, if not impossible, to arrive at any average in this disease. No author, so far as I am aware, states any. It is so dependent upon special and epidemic influences, that cal-

culations perfected upon the study of any number of cases for a given time, would be of no value whatever in calculating for the future, even in the same locality.

It would seem, upon general principles, that this disease would be generated by these ignorant, untidy people living, as most of them do, crowded together in little cabins or badly ventilated buildings, in alleys and back streets, poorly clothed, with cheap, frequently unhealthy food, but this does not prove to be the case.

I have frequently been sickened by the odors which came from the beds and was produced in the rooms of negro patients, confined frequently in the common room used for all family purposes—smoky, hot, with all the combined smells arising from cooking, eating, sleeping, and living, and this one room sometimes neglected since the illness of the mother of the family. I have never seen a single case of puerperal fever in a negro woman in private practice. They seem able to resist the effects of bad air, odious smells, and decaying discharges.

At the Freedmen's Hospital we were compelled to employ colored nurses. One of the greatest difficulties which we had to contend with was, getting them to observe the rules upon cleanliness and ventilation. They never *did* understand the *necessity* for this observance. Having lived in the constant neglect of all such laws, they could not be brought to appreciate their importance. Their confinement cases, under unfavorable circumstances even, had invariably done well when none of these strict laws were enforced. Puerperal fever, in a mild form, broke out in the Freedmen's Hospital during my connection with it. It was thought by the hospital staff that the first case occurred in a woman whose labor came off in the night, and was attended by a resident student who had been assisting at an autopsy during the day. Three women only contracted this disease, and none of them died. The lying-in wards were closed to new patients for a time, and extra care was used in disinfecting the building. The disease did not spread beyond the three cases above referred to.

*Operations.*—After an analysis of 985,446 labors collected from British, French, and German sources, Churchill states that the *forceps operation* was performed once in every 115 cases.

In the 2,000 labors among negro women collected by me, the



forceps were required but 4 times, or once in every 500 cases. The suggestion naturally arises, and it has been made to me by physicians, that while these labors may have been finally terminated without their aid, yet would not the condition of the mothers have been improved by a more frequent resort to these instruments?

I find no evidence that this would have been the case among those labors collected from the midwives, or in any of the others. No deaths from this cause, or instances of vaginitis, pelvic cellulitis, vesico- or recto-vaginal fistulae appear to have followed labor in any of these two thousand cases.

The rules upon which so much emphasis was formerly laid, requiring that the left ear should be distinctly felt by the accoucheur, or that the foetal head should remain stationary for six, or even four hours, pressing upon the maternal soft parts before the operation was justifiable, have, with the advance of obstetrical science, been disregarded.

The practice of Barnes and his school is, instead, governing the obstetricians to-day, viz., that as one woman may suffer as much in one hour from the effects of pressure of the foetal head upon her soft parts as another may in six, the circumstances of each case are to govern the medical attendant in his action, no arbitrary rule as to time being of any value.

Barnes says, "While we are waiting, the woman is suffering—suffering needlessly; her nervous energy is being used up; she is drifting into exhaustion." ("Lectures on Obstetric Operations," page 53.)

Take for example the case of a lady whose already nervous delicate organism has been greatly taxed and overburdened by the demands of fashionable society, anterior to and during gestation. Her pains are feeble and slow, and her strength is beginning to waver. Who will not say that in a case like this the "getting up" of our patient will not only be hastened but assisted, if we, by the use of forceps, save her several hours of suffering and exhausting, but finally successful, pains? This same patient, if in vigorous health, would not have required their aid.

The increasing frequency of this operation is not so much owing to the greater skill of the accoucheur, as in his ability to recognize the indications which imperatively demand such assist

ance as the forceps alone can render. The ignorant negro women, unaffected by the enervating influences of fashionable society, the slavery of stylish dress, and their attendant evils, do not require this operation any more frequently than they did one hundred years ago.

Labor seems to progress in these women naturally, and few of them regard themselves as sick, as we understand that word, when they are confined. Their previous modes of life having been healthful and natural, they have the requisite strength and endurance when their time of need comes. The labor, it seems to me, is normal, the period of convalescence short and uncomplicated, as a consequence of, or in proportion to, their previous simple and vigorous habits of living.

In addition to their universal healthful readiness for parturition, the low development of the anterior cerebral lobes and consequent shortening of the fronto-mental and occipito-frontal diameters of the foetal head diminishes the suffering of the patient. The diameters being shorter the resistance is less, and the greater occipital development is accommodated by the peculiar shape of the negro pelvis.

Tyler Smith, in his "Lectures upon Obstetrics," p. 317, makes the broad assertion that "all ethnological researches tend to show that with the advance of civilization the human head has increased in size."

He goes on to say in his argument that the size of the foetal head is greatly influenced by education and civilization, that the heads of Egyptian and Peruvian mummies are considerably below the size of the European cranium.

"Nothing within the range of human anatomy stands in a stronger contrast than the cerebral size and development of the New Hollander or Bushman and the Caucasian races. The different condition of education amongst different classes of the same race also has its effect upon the size of the brain and cranium. Hatters state that the size of the head is greater in the same classes in towns than in agricultural districts, in the educated than in the uneducated."

Sir James Simpson's memoir upon "Sex of the Child as a Cause of Difficulty and Danger in Human Parturition" states that, in cases of tedious labor, puerperal convulsions, puerperal fever, ruptured uterus, hemorrhage, and instrumental delivery,

by far the greater number of children are males. In cases of pelvic abscess, ruptured perineum and vesico-vaginal fistula, the same undesirable pre-eminence attaches to male children.

His argument is elaborate and exhaustive. I give below some of his principal conclusions, which have of late been acknowledged and proven correct by many obstetrical writers.

1st. Of the mothers who die under parturition and its immediate consequences, a much greater proportion have given birth to male than to female children.

2d. Of stillborn children a larger proportion are male than female.

3d. Of children born alive, more males than females suffer from the morbid states and injuries which result from parturition.

4th. More males than females die in the early period of infancy, and the disproportion diminishes from birth to some time afterwards.

5th. Of children who die *in utero* before labor, as many are males as females.

6th. Of accidents which happen after the birth of the child, as many occur with female as with male children.

If the slight difference—half an inch in the circumference of the cranium—existing between the size of the male and female head at birth, is capable of producing all the disastrous and fatal consequences above detailed in intelligent Caucasians, it is not at all difficult to believe, that in the ignorant negroes of the United States, the cranial development is enough less, in its frontal region especially, to account in a considerable degree for their immunity from the average ratio of puerperal accidents, and tedious and fatal complications.

“It seems a clear inference,” says Tyler Smith, “that the brain and head of the uncivilized and uneducated must be on the average smaller than those of the civilized and educated, and we have seen in the comparison of the male and female head at the time of birth how small a difference in the size of the foetal head is sufficient to increase the dangers, and necessarily the sufferings, of parturition.”

Dr. Simpson is of the opinion that the comparative difficulty of parturition with male and female children extends to the foetus of civilized races, and he refers the increased sufferings

of the civilized women in childbirth to the size of the foetal head.

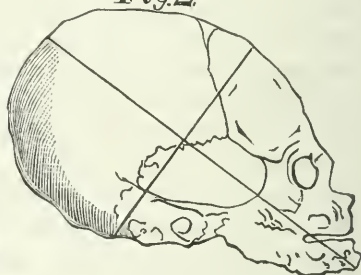
Dr. Livingston says of the negro women in South Africa: "They generally suffer less than Europeans, principally, as it appears, because they consider parturition to be an act of nature, and less of disease, than do the latter."

Tyler Smith continues: "The general bearing of all the knowledge we possess on this subject supports the view that barbarian women suffer far less than women in a state of civilization. The size of the foetal head must be considered the most important element in this matter, though, at the same time, there is the greatest sensibility induced by the habits and social condition of the civilized and highly cultivated female to be taken into account. Civilization not only influences the size, but the relative size of different parts of the foetal head. In uncivilized races the tendency is to increase the occipito-mental diameter, by the protuberance of the occiput and the greater development of the lower part of the face, and to diminish the occipito-frontal diameter by the flattening of the frontal bone and the low development of the anterior cerebral lobes. The diminution of this diameter must lessen the difficulty in the passage of the foetal head. In the case of a negress whose labor I had the opportunity of witnessing, the great mass of the foetal brain was behind the auditory foramen."

**Fig.1.**

Skull of White Fœtus.

(TYLER SMITH.)

**Fig.2.**

Skull of Negro Fœtus.

Notwithstanding the evident exaggeration of these two plates they are good types of the cranial development, the one of ignorance and partial barbarism, and the other of an enlightened Christian civilization.



Meadows quotes Churchill's statistics upon *version* as follows, viz.: "From an examination of nearly a million cases collected from British, French, and German reports, the necessity for this operation arises once in every  $123\frac{1}{3}$  cases."

In the 2,000 cases of labor among negro women collected by me, this operation was performed three times, or once in every  $666\frac{2}{3}$  cases. Once in the Freedmen's Hospital, where the left hand, right foot, and funis presented, with the head crowding down upon them; once when rupture of the uterus occurred, and the child had to be drawn back into the uterus from the abdominal cavity previous to version, and once in a consultation case, where forceps and craniotomy both failed, and the child had to be turned finally and brought down by the feet.

As nearly as could be determined without measurement there was a contraction of the pelvis antero-posteriorly in the last case. Drs. T. C. Smith and D. W. Prentiss, who were associated with me, are confident that such was the fact.

It may be of interest to remark here, that this is the only instance of deformity of the pelvis which I have met with in my investigations among negro women. With all the privations and hardships which the colored people have had to suffer, deformity of the pelvis seems to be little known among them.

At a meeting of the Philadelphia Obstetrical Society for May, 1874, Dr. W. H. Parrish read the history of a case of craniotomy which he had recently performed upon a rachitic negress—death resulting during the fifth week from pyæmic puerperal fever. The following extracts are taken from the November number of this JOURNAL, p. 494:

"During the night of March 18th, 1874, I was called to a colored woman, aged 28, then in labor, on whom it was stated craniotomy was required.

"The woman was at full term, had been in labor at least thirty-six or forty hours, was restless, with a feeble pulse of 120 per minute, and with evident exhaustion. The external genitals were hot and extremely sensitive. The abdomen protruded markedly forward, and the uterus was deflected to the left. Her stupidity was such that no satisfactory history of herself could be given, she stating that on two previous occasions two children had been taken from her, and that in neither was the head crushed.

. . . . .



"The patient was etherized and craniotomy performed, and a male child, which in its entirety would have weighed eight pounds most probably, was delivered.

"The woman was now aroused and morphia and whiskey administered. She soon rallied, the pulse falling to 110. She then confessed to having undergone a similar experience on two previous occasions at the Philadelphia Hospital, and we then recognized her as Josephine Scott. (See AMERICAN JOURNAL OF OBSTETRICS AND DISEASES OF WOMEN AND CHILDREN, February, 1873, p. 646.) . . . . .

"As the subsequent treatment we directed the daily administration of quin. sulph. gr. xij., and morph. sulph. gr. ss., with nutritious and easily digested diet, and the careful use of stimulants. It was easy to give directions, but with none around her willing or fitted to perform the duties of nurse, it was impossible to have her merest wants attended to. She repeatedly passed her urine in bed, because no one would hand her the needed vessel, and on the second night she partook of a hearty meal of fat pork. . . . .

"The autopsy was made thirty-six hours after death. 'The pelvis is distinctly kidney-shaped at its brim. The sacral promontory jutting forward so as to be the essential cause of the deformity. The outlet is not greatly diminished, nor is it ordinarily so in a rachitic pelvis.'

"The measurements of a dried *ligamentous* preparation of the pelvis are :

Superior Strait.		Inferior Strait.	
Antero-posterior diameter. . .	$1\frac{7}{8}$ inches.	Antero-posterior diameter. . .	3 inches.
Transverse diameter. . . . .	$4\frac{3}{8}$ "	Transverse diameter. . . . .	$3\frac{3}{8}$ "
Oblique left posterior. . . . .	$3\frac{1}{4}$ "	Oblique left posterior. . . . .	$3\frac{1}{2}$ "
Oblique right posterior. . . .	4 "	Oblique right posterior. . . . .	$3\frac{1}{4}$ "
Left oblique conjugate. . . . .	2 "		
Right oblique conjugate. . . .	2 "		

"The difference in the oblique diameter of both the brim and the outlet is due to the right sacro-iliac synchondrosis being on a plane posterior to that of the left.

Depth of the symphysis pubis with triangular ligament. . . . .  $1\frac{3}{4}$  inches.  
 Depth of pubic arch. . . . . 2 "

Shortest distance from tip of coccyx to promontory  
 of sacrum..... $3\frac{1}{2}$  inches.  
 Following curve of sacrum..... $4\frac{3}{8}$  “

“On two previous occasions craniotomy was performed on her, by Dr. Gervis in 1869, and by Dr. Parry in 1872; each time at the Philadelphia Hospital. Dr. Parry reported his case in the *Transactions* of this Society of last year, making it the basis of his excellent article entitled ‘Craniotomy and the Cæsarean Section in small Pelves.’ (See this JOURNAL, February, 1873, p. 644.)

“The pelvis and attached bones are now in the museum of the Obstetrical Society of Philadelphia.”

There are other complications which it would be interesting to compare with the ascertained average among Europeans; and in a future paper I hope to follow up this subject, and to point out some of the peculiarities, if there are any, of the hybrids of the negro race, especially in this country, in regard to the process of parturition.

In order to do justice to facts as they are seen by accoucheurs of the present day, I wish to add a word in regard to the statistics of Churchill, which have been so largely quoted. They were collected almost before the present revolution, if I may use so strong a word, began in obstetric operations, especially in regard to the use of the forceps.

Such is the testimony of our later obstetrical authors and writers in regard to the forceps operation and its increased frequency in this country and Europe, that it would be safe to say it is now performed three, and possibly four times, where it was once fifty years ago. This increased frequency is mostly among our refined classes of society, who have added to their higher cerebral development the nervous diathesis, so to speak, and whose vitality is so much absorbed in the social duties of their stations.

On the other hand, these statistics of negro labors have been collected within the past four years—except those occurring upon Southern plantations under the care of the “grannies,”—and it is altogether probable that the demand for the performance of this operation is no greater among them to-day than it was one hundred years ago.

In connection with this subject I would draw attention to the

last annual report of George Johnston, M.D., F.K. and Q.C.P., Master of the Dublin Lying-in-Hospital, published in the *Obstetrical Journal of Great Britain and Ireland*, for March, 1875, p. 798.

In his "Annual Account of the State of the Hospital," the doctor states that during the "year ending Nov. 5th, 1874, there were 1236 deliveries" and only "15 deaths from all causes." . . . "In 138 cases we deemed it advisable to employ the forceps. . . Version was performed in 14 instances." It will be seen that this operation was performed about once in every 10 cases, and what is more remarkable, in a very large proportion of the cases before the os was dilated. The discussion upon this subject in the Dublin Obstetrical Society at its meeting in January last, will be found to be of great interest, giving the views of Atthill Kidd, McClintock, and Johnston upon this subject, while none of the speakers condemn its frequent use in the face of such success—only seven deaths from puerperal causes during the entire year—they all recommend caution for unskilful operators.

I have already referred to the size and shape of the negro head, and have applied the remarks of Tyler Smith in regard to the effects of education and advanced civilization upon the development of the cerebral lobes of the brain, etc., to the ignorant negro women dealt with in this article. I have inclined largely toward the belief that the heretofore universal state of ignorance in which negro children have been born, has had much to do in giving to their crania the peculiar dimensions which are classed by ethnologists and writers upon anthropology as race characteristics. This shape of the negro skull, it must be admitted, is favorable to easy and rapid parturition, and these ignorant negro women have probably been saved many of the severer complications which statistics and our experience show have followed in the wake of education, culture, and refinement in the white race.

The peculiarities of the negro pelvis, which writers have claimed to exist, producing an exact conformity between the shape of the skull of this and other races and their pelvic canals, would to some extent lessen the force of the foregoing proposition. Whether these peculiarities are as prominently marked as

claimed, in the pelvis of the negress, it will be the province of the remainder of this article to discuss.

The study of craniology suggests that the pelvis through which the various crania pass, would be as distinctly characteristic of race as the skulls are themselves; that crania with short antero-posterior and fronto-mental diameters, with great development of the occipital region and of the lower part of the face, must, to pass easily and normally, be accommodated by harmonious race characteristics in the pelvis.

Dr. J. Aitkin Meigs, in his chapter upon "The Cranial Characteristics of the Races of Men," being chapter iii. of Nott and Gliddon's "Indigenous Races of the Earth," refers to the fact of a conformity between the cranial and pelvic types of a particular race, and says that "Vrolik, following up the suggestions of Camper and some other observers relative to certain peculiarities of the negro pelvis, has demonstrated the existence of race form for the pelvis as for the cranium. He has shown that the form of the head is adapted to the pelvic passage which it is compelled to traverse in the parturient act, and that the pelvis, like the skull, possesses its race characters and sexual distinctions sufficiently well marked even at the infantile epoch."

Meigs refers to the denial of Webber of the value of these observations, and accounts for the fact of his finding exceptions to this law in "encountering occasionally the European pelvic conformation among other and very different races, by the mixture of the blood of these races with that of Europeans."

Dr. O. Von Franque has written a very learned article upon "The Female Pelvis of Different Races," which is published in Scanzoni's *Beiträge zur Geburtshilfe und Gynäkologie*, 1869, vol. vi., p. 163. To this most comprehensive paper I am indebted for many suggestions, and have taken advantage of his investigations to present below several of his points, conclusions, and quotations in regard to the negro pelvis.

Von Franque states that, since Camper pointed out the difference of the skulls of different races and nations, a number of the best known naturalists have occupied themselves most searchingly with this question of race-crania. So much has this been the case, that the discussions upon this subject alone form quite a respectable library. The pelvis, on the contrary, espe-

cially of the female, has received very step-motherly attention, only few investigators having made the female pelvis of various nations and races the subject of their observation. We possess but very few useful descriptions and measurements of such non-European pelvises. The most important reason for this lies particularly in the fact that it is more difficult to obtain pelvises and entire skeletons than skulls. Von Franque states that a further reason of the absence of such pelvises, amid entire collections of pelvises, may perhaps be found in the fact, that such a subordinate significance was always ascribed to the pelvis, that it was hardly thought worth while to deal with this unimportant portion of the human skeleton.

The material is as yet very small; only of the pelvises of Malays and negresses has a large number been described exactly.

Von Franque gives descriptions and measurements of the pelvis of the Flat-head Indian of Vancouver's Island, the Malay woman, the Chinese woman, the Negress, the Papua Negress, and the Negress (?) from America. Added to these he gives the pelvis of a female Gorilla.

It has been the opinion of many men, myself included, that the negress gave birth to her children more easily than the white female, on account of her having a larger and more roomy pelvis. But my attention has been drawn frequently to the graceful carriage of well-dressed negro and mulatto women in the streets, and their walk compared with that of white ladies, suggesting that they possess more narrow loins and hips than white women, who, to use the expression of Meigs, "waddle when they walk."

Von Franque tells us that the first to write on the pelvises of non-European races was Soemmering: "Physical Differences of the Negro and the European," 1785. "Loins and hips of the negro race narrow, entire pelvis more narrow." In regard to the female negro pelvis, he quotes Camper regarding the angle formed by the descending rami of the ischium; this amounted, in a woman from Celebes, to  $90^{\circ}$ .

The first real measurements of pelvises of non-European nations seem to have been made by Rollin ("Mémoire Physiologique et Pathologique sur les Américains) being part of the work, "Voyage de La Pérouse autour du Monde," 1797, tome iv. He speaks of the "easy labors of the women in the vicinity of



La Baie des Français, N. W. America, and of the wide pelves of these women, and states that difficult labors are very rare." E. V. Siebold ("Diseases of Women," 1821, p. 15) states that "all the diameters of the negress at puberty are reversed, that the conjugate diameter of the inlet represents the large and the transverse the smaller space."

Vrolik treats this subject more elaborately in "Observations upon the Differences of Pelves of Various Races," 1820. He is the first to make a positive statement based upon investigation, that race differences may be recognized in the pelves as well as in the crania.

In 1830 M. J. Webber published his paper upon "The Original Race Forms of Skulls and Pelves of Men." He divided pelves into oval, round, four-sided, and globe-formed. He holds that one or other form may preponderate in this or that race, yet should not be considered as peculiar to this race; each original form may be found in every race, but more frequently in one than in the other.

J. Müller describes in detail the pelvis of a Bushwoman. ("On the External Sexual Parts of a Bushwoman," *Müller's Archiv*, 1837, p. 332.)

Kilian published Vrolik's drawings in his "Atlas of Midwifery," 1835, pl. xii.

Busch gives drawings in his "Midwifery" (1838, p. 40, pl. ii., figs. 10-15.)

Busch thinks it too early to lay down a certain normal rule in this respect, believing the number of pelves examined to be as yet too small.

Stein opposes the existence of race-pelves in his "Opinions upon Race Differences of Pelves." (*Zeitschr. für Geburtshunde*, vol. xv., I. p. 41, 1844). He denies any difference between the European and non-European pelvis. Von Franque thinks he goes too far, and says that modern investigation proves its existence, at least as regards the Malay pelvis. He does not appear to have seen pelves of other nations and races, much less examined them.

Hyrtl takes the directly opposite view. ("Topog. Anat.," 1853, vol. ii., p. 19.) He holds that in the intimate relations existing between head- and pelvis-forms the race characters expressed so distinctly in the former must be found in the latter (which,

however, has not as yet been confirmed). This explains why women, impregnated by men of another race, give birth with so much more difficulty, than if impregnated by their own race. (This statement taken from Vrolik and frequently repeated by Vrolik, finds sufficient refutation in Lehmann's observations, who saw in Amsterdam the labors of Javanese girls impregnated by Europeans, run their course easily and quickly.) In 1855 J. Struthers gave the description of a female Esquimaux pelvis. It is very roomy.

Ekir gives, in 1861, measurements of the pelvis of a native of Murray River, South Australia.

F. Zaaier, 1862, gives measurements of Malay pelves. Lehmann corroborated Zaaier's results.

Joulin (1864) published two treatises upon this question. He endeavors to prove that the differences between the pelves of the various races pointed out by Vrolik do not exist. From examinations of pelves he accepts two principal races, Arabian or Caucasian and Mongolian, including the negroes. He states that the difference in the pelves of the Arabians and Mongolians between the transverse and oblique diameter of the inlet is  $\frac{1}{2}$  ctm. in the Arabian, and only a few mm. in the Mongolian race! He does not consider this difference sufficient to be taken as race peculiarity, and holds that the transverse diameter of the inlet is greater than the antero-posterior in all races; a statement certainly correct in general, yet exceptions have been found. (Von Franque.)

Martin (1866) published measurements of Brazilian women, taken from the living subjects, compared with like measurements of European and mixed races. He obtained in these accurate measurements essential differences in the external measurements between European women and those of other races, and these divergent external measurements correspond in several points to the measurements made of the bony pelvis of the same races.

Goertz, in his inaugural thesis, "Upon the Pelvis of a Bush-woman," 1868, says that "the pelvis of a negress from Africa (Fig. 3), in the Wurzburg collection, strikes us on account of its considerable smallness, but gives the impression of an otherwise regular female European pelvis, only too narrow everywhere; this, of course, is without reference to the peculiarities

of the negro pelvis. Its bones are not of particular fineness or delicacy."

Next to the pelves of Malay women, the pelves of negresses ought to be best known, since more pelves have been described of this race, yet their number is not nearly as large as that of the known pelves of the Malay race. Neither do the descriptions of these negro pelves correspond in such a degree as to afford a certain conclusion in regard to the pelves of this race in general, such as was reached in reference to the Malay pelves. (Von Franque.)

Vrolik gives the following peculiarities for the negro pelves: "The difference between the pelvis of the male negro, which could not be of firmer mass or have stronger bones, if taken from some wild animal, and that of the negress, which unites delicacy and lightness with roundness, is much more pronounced than in European pelves, the vertical direction of the ilia, which in spite of delicacy of bones were deprived of the usual transparent spot, their height at the posterior superior tuberosities, the great proximity of the anterior superior spinous processes of the ilia, the lesser width of the sacrum, the smaller circumference of the hips, the shorter conjugate, the shortness of the transverse diameter, and the spinous processes and tuberosities of the ischium, the oblong form, which the pelvis thereby receives, all this recalls to memory the pelvis of the ape."

Von Franque is unable to find a resemblance between the negro and gorilla pelvis. This has also been stated by Joulin, who also denies the peculiarities claimed by Vrolik for the negro pelvis, and does so, as we shall see, with perfect justice.

Several peculiarities, indeed the majority upon which Vrolik bases the monkey-like character of the negro pelvis, do not exist at all in the monkey pelvis, at least not in the pelvis of anthropoid monkeys, the chimpanzee and gorilla, and these of course can only be regarded here. "In the monkey pelvis the anterior superior spinous processes of the ilium are far apart, the fossæ ilii are nearly completely turned anteriorly, are very transparent in a great extent, and the antero-posterior diameter is much larger than the transverse, the sacrum is very narrow. All these peculiarities are absent in the negro pelvis; the only point in which we might find an approximation of both, is the height of the crests of the ilium, whose highest point in the

gorilla corresponds to the upper edge of the last lumbar vertebra, and in the negro, to the middle of this vertebra ; a circumstance which by no means justifies us in speaking of a resemblance of both pelves."

As regards the narrowness of the sacrum of the negress, affirmed by Vrolik, the tables of Von Franque show that it is not so very narrow ; its mean width, 107 mm., is larger than in the Malay woman, 97 mm. ; it is 2 mm. more than in the European woman, 105 ; Martin also states that the sacrum is broad.

"Comparing the negro pelvis with other races, it must be defined as much smaller in its capacity than the pelves of other nations ; with this it has a comparatively greater depth, at least in the posterior pelvic region ; the sacrum of the negress, curved regularly anteriorly, is 101 mm. long ; that of the Malay woman, less curved in general, 91 mm ; that of the European woman, 100 mm. With the exception of both measurements of the sacrum, its width and length, all the measurements of the negro pelvis fall below the European measurements, but here also the relation of the antero-posterior diameter of the inlet to the transverse is different, the conjugate is relatively greater, 102:118 ; the transverse is only 16 mm. greater ; in the European, 291 ; in the Malay, only 9 mm. Busch affirms that in some negro pelves he has found the antero-posterior diameter absolutely greater than the transverse, without, however, giving the respective measurements."

"The negro pelvis is also smaller than the Malay pelvis, at least as regards the most important internal measurements, especially of the inlet ; the measurements of the separate bones are indeed larger in the negress ; this, however, is to be placed to the account of the greater development of the bones in general ; the pelvis is not fine and elegant, like the Malay pelvis. As regards the transparent place produced upon the ilia by want of diploë, it is by no means as constantly absent in the negro pelvis as has been stated by Vrolik and Busch ; it was absent in the three pelves of Vrolik, and in one of Busch ; according to Martin, the ilia are mostly opaque ; Joulin, on the contrary, found in most of his pelves the transparent spot, although it occurs less frequently and distinctly than in the European women. In the pelves in question it is distinct."

"Do the existing statements and descriptions which we possess



of fifteen negro pelves, justify us in drawing a conclusion in regard to the peculiarities of the negro pelvis in general, such as we were able to draw from the Malay pelvis? Do these peculiarities, if they indeed are present, justify us in establishing distinct characteristic differences between the pelves of the negress and the European in particular? I should prefer to answer, No."

"If the description of the fifteen negro pelves corresponded with one another in the same degree, as did the thirty-eight Malay pelves, at least, in the grand total, we might, perhaps, be permitted to draw a conclusion, but only, perhaps, since the number fifteen is indeed too small. As this correspondence does not exist, we can, until the examination of a greater number of pelves has been made, only say, the pelvis of the negress appears to be less capacious in general than the European pelvis. The antero-posterior diameter of the inlet is relatively greater than in the European. It is not permitted to draw further conclusions."

*Gorilla Pelvis.*—Conclusions reached by Von Franke, l. c. p. 203.—Although in a comparison of the gorilla pelvis with that of the human female, a resemblance in general must be found, a resemblance of the certain form of the pelvis, yet, in a comparison of the component parts, especially of the true pelvis the difference is so striking that we are forced to the conclusion that between the pelvis of the anthropoid monkeys, chimpanzee and gorilla, and the pelvis of the human female, there is yet a wide gap, and least of all are we able, at least according to the specimen before us, to find an approach of the pelvis of the negress to that of the anthropoid monkeys, as was asserted by Vrolik; we might sooner, perhaps, find a correspondence in several points between the monkey and the Malay pelvis. Granting that several points of the negro as well as Malay pelvis have a very distinct resemblance to the respective points of the monkey pelvis, there still remains, according to the hitherto made examinations, this difference between human and animal pelves, that in the latter, especially in those of the anthropoid monkeys, the antero-posterior diameter of the inlet is considerably longer than the transverse, which is found in the human pelvis so rarely, so exceptionally, and also in a much less degree, that we can justly, with Joulin, lay



down the following dogma: *In all races of men the transverse diameter exceeds the antero-posterior in length.* The difference between the two seems indeed to vary in different races, so that in the lower, uncivilized nations the antero-posterior diameter increases in length as compared with the transverse, without, however, exceeding the latter, and in so far we may indeed speak of an approach, but only of a very distant one, to the pelvis of the anthropoid monkey, without, however, as yet at least, being able to demonstrate a gradual transition from one to the other; this question must as yet remain open, to be decided by more extended investigations.

In Von Franke's paper the three following conclusions are given, viz. :—

1. The number of known carefully examined and measured pelves of various races is as yet too small to enable us to draw conclusions in regard to the pelves of these races, with the exception of the Malay race; the pelves of the latter were described so correspondingly by all in their principal points, that we may justly draw a conclusion in regard to the character of the pelves of this race. As regards the pelves of negroes, of which race next to the Malay, the majority of pelves are known, such a conclusion is, for reasons already stated, inadmissible.

2. The size of pelves, according to investigations, seems to increase from south to north, but in southern nations the conjugate seems to increase in proportion to the transverse diameter.

3. A transition from the pelves of anthropoid apes to that of the human female, much less a similarity between the two, can as yet not be demonstrated; the above differences show the wide gap between both. If, for other reasons, the statement may be justified, that the distance between anthropoid apes and men of the lowest order is smaller than between the latter and the European, yet this statement is not justified in reference to the character of the pelves.

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We are able as yet to state very little positively in regard to the course of labor in different races. Referring to our want of knowledge in regard to the pelves of different races, and to the course of labor itself, Von Franke says that "we may accept,

in all probability, that differences in the course of labor exist, considering the different characters of pelves, especially of the inlet; especially must we accept that, as well in the entirely round pelves as also in those whose conjugate has increased in length in comparison to the transverse diameter, as seems to be the case in most of the Southern races, the mode of passage of the head through the pelvis is different from that in the European pelvis. That this is the case we may conclude from the fact, that in the different forms of pelves seen in Europe, a different entrance and passage of the head can be observed. It is indeed true, that in the formation of our judgment of the course of labor in the various races, even if we were better acquainted with the pelvis than we are, an important point is absent, *i.e.*, the proportions of the object of birth, especially of the foetal head. I could nowhere find measurements of neonati; only two physicians mention the foetal head when speaking of the labor."

L. von der Steige writes, March 10, 1802, in sending a pelvis of a Java woman to Dr. Deimann (who sent the pelvis to Vrolik): "You will doubtless be surprised by the smallness of the conjugate of the small pelvis, and by its small depth; yet they bear children easily because their children have a small occiput, and because also their ligaments are very elastic. No woman has ever needed any assistance except such as were impregnated by Europeans," etc.

\* \* \* \* \*

We possess, therefore, upon the course of labor in uncivilized nations only very general observations, from which, however, it is certain that labors in these uncivilized nations run their course more easily and quickly than in European women, and that difficult labors in them are more frequently due to anomalies in position of the child, than to mal-proportion between the size of the child and the pelvis, to narrow pelves; the latter, however, are said to occur (Rollin).

Von Franque reports as follows what he has been able to find in literature in regard to this question:

2d Moses,<sup>1</sup> ch. i. vs. 15-21. "The Israelitic women are not like the Egyptians, they are robust, and have given birth to children before the midwife can reach them."

<sup>1</sup> Exodus.

Barlæus (1660) speaks of the easy labors of Brazilian women.

Ælianus says the same thing of the ancient Egyptian and Phœnician women.

Siculus speaks of the easy labors of the inhabitants of ancient Liguria.

Strabo of those of Spain. Ludolf praises the easy labors of Abyssinians.

Kolben says of the Hottentot women: "When the labor becomes hard the midwife administers a cold decoction of tobacco and milk, which produces its effect at once."

Garcilano de la Vega speaks of the easy labors of Peruvian women.

Charleroix writes of the easy accouchement of North American Indians, as does Rollin.

Barrere, Bouguer and de la Condamine say the same thing of South American Indians. Also Bancroft and Langsdorf state that the inhabitants of the Isle of Hakahiwa have easy labors.

Morier says the same thing of the Persians. The women of Kamschatka are said to have easy labors, also the Esquimaux. Dr. Henschel, of New York, writes to Prof. d'Outrepoint, of Würzburg: "I have attended 43 labors in colored females (mulattoes and quadroons included). The first two stages ran much more slowly than *ceteris paribus* in the white women, but as soon as the head enters the outlet it proceeds more rapidly.

"Of these 43, 2 were face presentations, 2 right-hand, once the left, and once both hands alongside of the head.

"This proportion is indeed rather considerable, yet not perhaps sufficient to base a theory upon, and may be accidental.

"These experiences coincide with those of several colleagues, whom I questioned in regard to it."

D'Outrepoint adds: "These expressions were called forth by a question by me, whether differences of the pelves of various races had no influence upon the position of the fœtus, especially whether the latter had not a greater resemblance to the position of the fœtus in animals, *i.e.*, whether the hand did not present much more frequently, whether facial presentations were not much more frequent than in the Caucasian race."

I believe that this latter should be mentioned here, because

the observations are the only ones which have been communicated upon position and presentation of children at birth in non-European races. Unfortunately, on account of want of accurate observation, it must as yet remain undecided, whether, indeed, facial births are more frequent in the blacks to such a degree, than in our women, in whom, as well known, facial births are much more rare, or whether Henschel's figures are merely accidental (Von Franque).

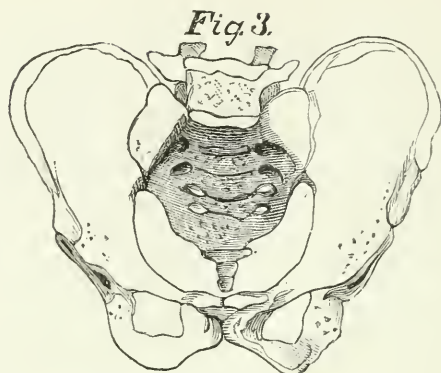
In the 2000 cases analyzed by me in this paper, no case of face presentation occurred so far as I was able to learn. Certainly no case in the 500 labors attended by myself, and the records of the Freedmen's Hospital of this city show no cases. I think, therefore, that the above record of cases by Dr. Henschel must have been accidental.

Dr. Henschel, at the same time, sent to Prof. d'Outrepoint the pelvis shown in Fig. 4, and gives its history as follows: "I send you herewith the pelvis of a negress, which is old, to be sure, but authentic; I will not assert, however, that it is from a pure negress, more likely from a mulatto or quadroon." Von Franque says that the size of the pelvis, the circular character of the superior strait, and the general rounded outline of the whole pelvic canal, as well as various other minor peculiarities, indicate that it is not a pelvis of pure race, but probably of mixed breed.

Fig. 3 represents the pelvis of an African negress, Fig. 5 a normal white female pelvis, and Figs. 6, 7, and 8 show the outlines of the superior straits of the negro, mulatto, and white female pelvis, clearly proving their dissimilarity.

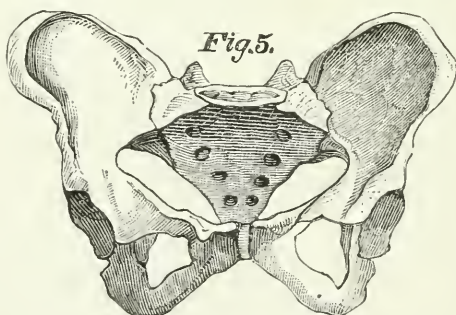
Finally, we must give the observations of Martin. He also, according to his own experience, as well as to information derived from colleagues, must leave it undecided whether the mechanism of birth in other races is different from that in our women; whether the foetal head in the comparatively longer conjugata of the inlet places itself at the very beginning more into this diameter. According to Martin's observations, the blacks in Brazil are said not to bear children with much greater ease or more difficulty than our women.

Dr. E. Charles, Professor of Midwifery, of Calcutta, also told him, that he had not noticed any essential difference in the course and mechanism of labor of the native women in and around



Pelvis of African Negress—quarter natural size.  
(Reduced in size from Von Franque.)

Calcutta. The labors have, at most, been a little more rapid, and are rarely ever complicated by narrowness of pelvis, in consequence of rachitis or osteomalacia. The same statement was obtained by Martin from a black midwife of Senegambia, according to whom the labors there ran their course the same



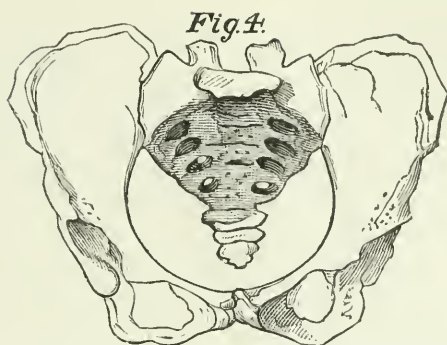
White Female Pelvis—quarter natural size. (Leishman.)

as in Dublin; easy and difficult labors were in the same proportion in Senegambia as in Europe.

To these last mentioned statements, statistic proof by exact figures is wanting, without which they have only a subordinate value. (Von Franque.)

It is hoped that the statistics submitted in this paper may be of some value, so far at least as the negro women of the United States are concerned.

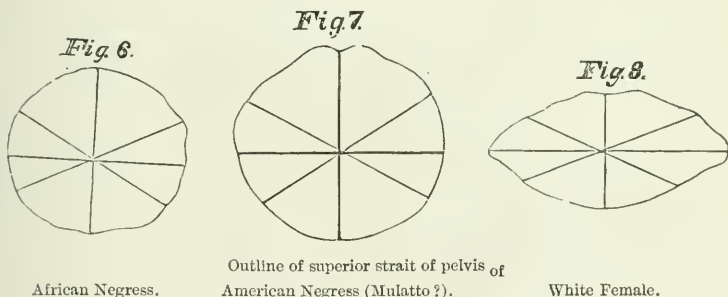




Pelvis of American Negress (Mulatto?)—quarter natural size.  
(Reduced in size from Von Franque.)

Von Franque, whose statements in regard to the female negro pelvis I have already so largely quoted, closes his admirable article as follows :

“Comparing now these different observations upon the course of labor in different uncivilized nations, as we have quoted



them from ancient and modern literature, this much is evident, no matter how generally they may be held, that the labors, as already pointed out above, run their course more easily and rapidly with them than with our women, the influence exerted upon this more rapid course by the form and shape of the pelvis can certainly not be set aside. We must, however, not forget, in this question, the *influence of culture*, which certainly cannot be estimated too highly, so that with increase of culture and super-refinement of customs, not only the most various diseases appear more numerous, but that also, in the same

measure, the labors become more difficult and of longer duration ; that especially complications step in, which are conditioned by anomalies of the bony pelvis, and which are, in general, met with but rarely, almost not at all, in uncivilized races."

That there are race peculiarities belonging to the negro female pelvis, I think, is no longer to be doubted.

The conclusions of such a cautious writer as Von Franque settle, beyond doubt, that the antero-posterior diameter of the inlet of the female pelvis in the negro is longer than in the European female.

The relation which exists between the skulls of a race and the female pelvis of the same race is referred to by Wood, in his article upon the pelvis, in *Todd's Cyclopædia of Anatomy and Physiology*, in the supplement to Vol. V., p. 146. He says : " The relative development of the pelvis seems to extend not only to sex, but to the varieties of mankind, either as an irrespective consequence of primitive formative type or in regard to the adaptability of the foetal head to the process of parturition. In either case this adaptation is strikingly illustrated by the different pelvic forms prevailing in different races of men, which will be found, when considering that branch of our subject, to be markedly assimilated to the form of the skull."

Again, upon page 148, after referring to the effect of the different varieties of the pelvis upon the form of the body, in the different races of mankind, he speaks of the writings of Camper, and after him, of Soemmering, and refers to their declaration, that the negro had more slender loins and hips than the European, consequent upon narrower pelvis. He quotes a comparative measurement of the diameter of the brim of a pelvis in a negro and a European, of adult size, from Soemmering, which Von Franque does not give.

In the negro he " found that the long or transverse diameter was 3 inches and  $11\frac{1}{2}$  lines. In the European, 4 inches and 6 lines. In the negro the short or conjugate diameter was 3 inches  $7\frac{1}{2}$  lines, and in the European 3 inches and 11 lines.

From Camper's measurements, the long diameter was to the short one as 39 to  $27\frac{1}{2}$  in the adult negro, and as 41 to 27 in the adult European, who, nevertheless, was of much less stature

than the negro. The measurements given on page 151 were taken in the dissecting-room of King's College, from an adult negro, six feet in height. From the measurements of this pelvis, the antero-posterior diameter seem to prevail in the negro, and the whole pelvis to be smaller than the European. This is seen remarkably in the limited breadth of the sacrum, 3 inches and 9 lines, and in the approximation of the ischial spines, 3 inches, both much lower than the average European; the latter less, indeed, than in the chimpanzee. In fact, I have never met with a European sacrum so narrow as in the negro above-mentioned, especially in an individual so tall as six feet."

Wood says that "it is supposed that in negro women, generally, *from the easy labors which they undergo*, there is much more proportionate pelvic capacity. The dimensions of the pelvis of a negress, of small stature, contained in Bonn's museum, at Amsterdam, are given by Dr. Hull, in his second letter to Simmonds, as follows: at the brim the conjugate diameter was  $4\frac{3}{8}$  inches, the transverse  $4\frac{1}{8}$ , the oblique also  $4\frac{1}{8}$ . From inner extremity of the superior pubic ramus to the sacro iliac joint of the same side,  $4\frac{1}{2}$  inches; at the outlet the antero-posterior diameter (measured from the apex of the sacrum) was  $4\frac{1}{2}$  inches, the transverse  $3\frac{1}{2}$  inches; the breadth of the sacrum was  $3\frac{1}{2}$  inches, and the length the same. The angle of the sub-pubic arch measured only  $67\frac{1}{2}^{\circ}$ . In this pelvis also, although a female, the prevailing size of the antero-posterior diameter, and the limited breadth of the same, and transverse diameter of the outlet, as well as the exceedingly small expanse of the sub-pubic arch, are very remarkable, and are hardly accordant with easy labors unless from the special adaptation of the foetal head."

Wood refers to the writings of Vorlik, and copies his remarks in regard to what he considers marks of degradation of the negro female pelvis, viz.: "The vertical direction of the ilia, their elevation at the posterior superior spinus, and the approximation of the anterior iliac spinus to the cotyloid cavity, together with the narrow transverse and antero-posterior diameters, the anterior sacral projection, the general elongation of the pelvis, and the greater acuteness of the sub-pubic angle."

Wood gives Professor Webber's four conclusions, the same

as Von Franque, and expresses about the same views in regard to them. He goes so far as to say, that "all differences in the human pelvis, in the various nations and races of mankind, although they may vary greatly from their special national peculiarities, yet they present characteristics which belong to other varieties of the *human species*."

"The coincidence between the prevailing form of the skull and that of the pelvic brim in these classes of the human race is worthy of special remark and influences materially, as before mentioned, the adaptation of the foetal skull to the pelvic passage during labor. After the form of the skull that of the pelvis is, perhaps, the most characteristic of race of any in the body, because of its great influence upon the shape of the trunk; and yet, from Webber's researches, it would appear that it is not sufficiently so to constitute a greater distinction than that of variety, and is not exclusive enough in its peculiarities to establish separate generic classifications of the human species."

I have made great efforts to secure some negro female pelvises for the purpose of taking measurements, but am able to find only two specimens, and they are of negro girls, about twenty years of age, who died of phthisis. Their bones are surprisingly small, and poorly nourished. Dr. Otis, of the Army Medical Museum, very kindly gave me permission to make measurements of all the negro pelvises under his charge; but in tracing the histories of their owners during life, the only ones which were *surely* female negroes were the two above referred to, and they are of but little value for this article.

I give below, however, by the kind permission of Dr. Otis, measurements of the pelvis of one of these negro girls, aged 20, whose entire skeleton is mounted in the Army Medical Museum.

These figures are contrasted with similar measurements given by Leishman in his "System of Midwifery," page 45, of the average female pelvis.

Measurements of pelvis of negro girl, 4 ft. 9 in. tall, age 20, mounted in the Army Medical Museum at Washington, D. C.

- |   |                          |
|---|--------------------------|
| 1. Circumference of the brim.....                                   | 13 $\frac{3}{4}$ inches. |
| 2. Between widest part of iliac crests external meas. 8, internal.. | 7 $\frac{1}{2}$ "        |
| 3.      "      anterior superior spinus of ilium.....               | 6 $\frac{1}{4}$ "        |
| 4.      "      front of symphysis and sacral spinus.....            | 5 $\frac{1}{4}$ "        |

5. True pelvis.	Conjugate.	Transverse.	Oblique.
Brim.....	$3\frac{3}{4}$ .....	$3\frac{3}{4}$ .....	$4\frac{1}{2}$
Cavity.....	$4\frac{1}{4}$ .....	$3\frac{7}{8}$ .....	$4\frac{3}{8}$
<sup>1</sup> Outlet.....	2.....	$3\frac{1}{8}$	
6. Breadth of sacrum.....			$3\frac{1}{2}$
7. Length of sacrum.....			$4\frac{1}{6}$

Measurements of average adult female pelvis (Leishman), undoubtedly European :

1. Circumference of brim .....	17 inches		
2. Between widest parts of iliac crests.....	10 $\frac{3}{4}$ "		
3. "      anterior superior spinus of ilium .....	10 $\frac{1}{4}$ "		
4. "      front of symphysis and sacral spinus.....	7 "		
5. True pelvis.	Conjugate.	Transverse.	Oblique.
Brim .....	4 $\frac{1}{2}$ .....	5 $\frac{1}{4}$ .....	5
Cavity .....	5 $\frac{1}{4}$ .....	5.....	[5 $\frac{1}{4}$ ] <sup>2</sup>
Outlet .....	5 <sup>3</sup> .....	4 $\frac{1}{4}$ .....	[4 $\frac{3}{4}$ ]

The number of negro pelvises which have already been measured and described, does not seem to be greater than fifteen, and as Von Franke and others remark, it is hardly permitted us to draw positive conclusions in regard to the pelvis of a race from the examination of so small a number. Yet it has been demonstrated, so far as these fifteen pelvises can show us anything, that the negro pelvis is, or appears to be, less capacious than the European, and that the antero-posterior diameter of the inlet is relatively greater. These few measurements seem to have been the authority upon which writers upon ethnology and anthropology, and even upon obstetrics, have based their statements, that in the negro pelvis there is a degradation of type so great, some have declared, as to place them in a grade lower than the chimpanzee. All writers I have examined on this subject quote the statements of Camper, Soemmering, Vrolik, and Webber, and as we have their measurements, and the number of pelvises described, we can attach as much importance to their conclusions as their number and scope demands.

Leishman, hardly two years ago, states in the introductory

<sup>1</sup> I am indebted to Dr. J. E. Cheney, of Dr. Otis' office, for his aid in taking these measurements.

<sup>2</sup> The oblique diameters of the cavity and outlet are placed in brackets, as not being taken from fixed bony points they are of comparatively little importance.

<sup>3</sup> 6 when coccyx forced back.



chapter to his work upon midwifery, p. 30, which is pronounced by our medical journals to be the finest text-book upon this subject in the English language—"that an examination of negro, bushmen, and other pelves, shows in many instances a remarkable degradation of type, such as a vertical direction of the ilia and their elevation at the posterior superior spinus, narrowness of the sacrum, and acuteness of the sub-pubic angle." He also says immediately, after speaking of the peculiarities of the negro pelvis: "An occasional peculiarity of some of the lower races, and one which appears even more to approach to the ape type, is the preponderance of the conjugate over the transverse diameter of the brim." Many other authors refer to the negro female pelvis as possessing peculiar race characteristics, as being oblong, deeper, more narrow, and with longer conjugate diameter than the European female pelvis.

The discussion on the anatomy of the negro cranium and of its comparative diameters and capacity, opens up too large a subject for consideration in this article, already much longer than I had originally intended; and I shall content myself with the statement of what must be apparent to any observer, viz.: that the negro head has, as a rule, a more receding forehead, a greater development of the occipital region, and of the lower part of the face than the European. There are so many exceptions to this rule among *educated* negroes, that I have little doubt but that education, culture and refinement, continued through successive generations may produce a change in their peculiar type of cranium; in fact, there may be, as generations increase in intelligence, such a development of the anterior lobes of the brain, as to overbalance its present race distinctions, which are possibly distinctions of uncultured intellect as well as of race, and these alterations in culture may in time bring about changes of type in the cranium itself.

The present differences of type in the negro and European crania will be admitted probably without discussion.

In conclusion, I refer to Leishman, his being the latest standard obstetrical text-book. He says, page 30, referring to parturition in different races, "We have just seen that the form of the pelvis corresponds to the shape of the head."

If this be so, what is the form of pelvis which would most naturally suggest itself to our minds as "corresponding in

shape" to the average negro head? Apparently such a pelvis as has been described by Vrolik, Soemmering and others—a pelvis, deeper, more narrow, and with longer conjugate diameter at the brim, than exists in the average European female pelvis.

937 NEW YORK AVENUE.

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## THE VESICO-VAGINAL AND VESICO-RECTAL TOUCH.

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### A NEW METHOD OF EXAMINING THE UTERUS AND APPENDAGES.

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A Paper read before the New York Obstetrical Society, February 16th, 1875.

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By E. NOEGGERATH, M.D.

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THE last steps made towards extending the sphere of physical examination relating to the organs hidden inside the pelvic cavity and beyond the roof of the vagina, were: the introduction of the uterine sound, the examination through the rectum, the use of sponge-tents, the introduction of a catheter into the bladder, while one finger explored through the rectum. Although a very large amount of pathological conditions is thus brought within the limits of physical diagnosis and within the scope of treatment, there remains a great deal to be accomplished, especially with regard to our means of diagnosis of alterations occurring in the uterine appendages. But even when exploring the uterus itself, the size of all its diameters, the density of its tissue, its true shape, the exact seat of painful affections, the extent to which a benign or a malignant deposit in the neck involves the parts above, escapes our appreciation in the ordinary run of cases, and this is accounted for partly by the thickness of the abdominal walls, partly by the rigidity permanent or excited in consequence of the examination itself, of the lower section of the rectus muscles, and finally by the position and manner of suspension of the uterus in the pelvic cavity. In most instances, where we are called upon to examine a woman suffering from a uterine or a peri-uterine affection, we find the uterus slightly anteverted or anteflexed, and in attempting to move it forwards towards the hand resting on the abdominal walls, by pressing upon the infravaginal portion, we only succeed in elevating it, leaving its axis in the same relation to that of the

pelvis in which it was before the taxis, and although we are thus enabled very often to grasp the fundus and part of the upper section of the uterus adjacent to it, most of the anterior wall remains distant from the fingers examining outside. Alterations in or near the broad ligaments and in the tissues of the ovaries, if not of considerable size and density, are beyond our means of appreciation. If a method were at our disposal which permitted to touch the uterus and its surroundings without the interposition of the abdominal walls, a host of alterations would be cleared up during life which were out of our reach heretofore. By a lucky accident I have found, and now propose to your consideration and trial, a method for which I claim that it accomplishes the object in question. It consists in the palpation of the uterus and its lateral appendages by the finger introduced through the urethra into the bladder. This proceeding, although apparently very simple, has, as far as I am aware, hitherto escaped the notice of gynæcologists. I have looked over all the more recent text-books of gynæcology and gynæcological operations, but I was unable to find even an allusion to it in any shape whatever. When I had succeeded for the first time to complete the examination in the manner I am going to describe to you, I became greatly impressed with the new revelations which were imparted to the sense of touch. I had the sensation that I could feel the uterus as distinctly as we do it during an autopsy. The first case which offered an opportunity for the vesico-vaginal touch was the following:

Mrs. L——n, 53 years old, sterile, menstruated for the last time three years ago. She called to consult me in the spring of 1873, about a very troublesome affection of the bladder. I had treated her before for varicose veins in both the lower limbs. I learned from her now, that for several years she had to pass water frequently during the day, and that for the last six months the evil had much increased; she was forced to get up several times during the night.

On making a digital examination, I detected a tumor of the size of a small walnut close to the left side of the fundus uteri. I could grasp it between two fingers, one in the vagina, and one above the symphysis pubis, and when doing so, the patient remarked that this manœuvre gave her a desire to urinate. Thinking that I had to deal with a tumor in the bladder, I began to

dilate the urethra with steel bougies of gradually increasing calibre. It was done in my office, and gave very little pain, so that I was soon enabled to introduce, first the fifth, and immediately afterwards the second finger of my left hand into the bladder. I now detected a small tumor, somewhat flattened from above downwards, protruding into the cavity of the bladder, to the left of its apex. It appeared to be attached to it with a base about one-third smaller than its largest circumference.

Not being thoroughly satisfied with the results of this first examination, I requested Mrs. L. to call again in a fortnight. On this second occasion the patient expressed her satisfaction about the result of the foregoing manipulation, because she had now been enabled to retain her urine much longer, and could rest through the greater part of the night. On examining the urethra I found that it had not contracted to its former size, but I could without any further preparation introduce the fifth, and immediately after, the second finger of the right hand. I now recognized that the membrane lining the tumor felt exactly like the mucous membrane of the bladder itself. I could easily move it over its body. I began at once to displace the uterus by pressing upon the vaginal neck, and with it I felt the tumor in the bladder move from side to side.

The patient called again and again, in intervals of from two to six weeks, to have the benefit of urethral dilatation. I repeatedly introduced the sound into the uterus, and moved it far into the sides of the pelvic cavity, the tumor following all the movements. On one occasion I pulled it downwards, and I was now able to so far displace the tumor from the bladder that I touched only a very small section of its surface. I could on several occasions feel a pedicle connecting the tumor with the left horn of the uterus, and I came at last to the conclusion that I had to deal with a diseased and misplaced left ovary. The fact that it protruded far enough into the bladder to convey the impression of an intravesical tumor is no longer surprising to me, and I will have occasion to mention a similar observation in the course of this paper.

Mrs. R. B——, 36 years old, a German widow, had been always healthy in her youth, menses always regular, two normal confinements. She began to suffer in the month of April,

1874, when she was seized with a dull pain in the sternal and epigastric region, palpitation, headache, singing in the ears, general weakness. About five weeks before I saw her, she remarked a profuse white discharge from the vagina, and a fortnight later she had a pretty severe attack of hemorrhage from the womb, which lasted for six days, when it was arrested by the use of cold applications to the abdomen. The hemorrhage did not coincide with her menstrual periods. As long as the bleeding lasted, all those symptoms described above disappeared, but returned when the hemorrhage had ceased.

Mrs. B. called at my office on the 15th day of November, 1874. On making a digital examination, I found the uterus enlarged, the neck painful to the touch, hardened, with a partly everted, granulating membrane lining the os. By external manipulation an oblong tumor, situated in front and towards the left side of the uterus, of rather indistinct outlines and not very solid consistency, could be perceived. It was of the size of a hen's egg, and painful to the touch. Mrs. B. mentioned the fact that she had been seen by Dr. Byrne and Dr. Hesse, of Brooklyn, who had discussed the advisability of an operation, deferring, however, its execution until there was a more urgent call for interference. As the patient appeared to be very anxious to have this question decided, and since the result of this examination was neither satisfactory in regard to the exact location or the nature of the swelling, I proceeded to dilate the urethra in my office with Ellinger's uterine dilator, and was soon able to pass the little finger of my left hand into the bladder without giving much pain.

On sweeping it over its surface I felt that a mass, corresponding very much in size, shape, and position with the tumor described above, protruded from the left and upper section of the bladder into its cavity, in such a manner that I thought I could feel the broad base with which it was attached to it.

The sense of touch in the fifth finger not being educated sufficiently to rely on the impressions received by it, I asked the patient to enter the German hospital for a couple of days, in order to decide the question as to surgical interference by undergoing a more thorough examination under chloroform. She was admitted on the 26th of November. Mrs. B. told me that the pain from the first examination had left her immedi-



ately, and that she made the journey to her home in Brooklyn without any inconvenience.

On the day following she was put under the influence of chloroform and ether, and examined again. I found by the ordinary double-touch that the tumor was not quite as perceptible as on the first occasion. I now passed first the little and then, without the least difficulty, the index-finger of my left hand into the bladder and touched again the protruding mass, found during the first exploration, but I was now able to ascertain that the tumor was not growing from, but simply crowding into, the bladder from outside, and covered with its tissues. I could roll it between my finger in the bladder and the hand placed upon the abdomen. I became convinced that it was an oblong membranous sac, with semi-solid contents, and was inclined to take it for a hydrops of the left Fallopian tube.

A slight hemorrhage followed this examination. When the patient had recovered from the effects of the anæsthetic, she suffered for a little while from pain about the orifice of the urethra, but slept well during the night and was perfectly comfortable on the following day.

On the 8th of December I proceeded to repeat the examination. The urethra had remained distended, so that I could at once introduce the index-finger of my right hand. I felt the same oblong, soft mass as on the first occasion. In order now to ascertain the connections of the tumor in question with the uterus or appendages, I passed my double hook into the cervical canal, and had the uterus drawn by an assistant as far down into the vagina as could be done safely, without exerting an undue amount of traction. Now the second finger of the right hand was introduced into the rectum, and that of the left into the bladder. I could in this manner touch and examine the fundus of the uterus just as distinctly as if I had it between my fingers, without any tissue intervening, very much as when the organ is examined after the operation for ovariectomy. I could distinctly feel two fibroids, one on each side near each cornu, just protruding from its tissue, both not larger than a lentil, which were quite imperceptible with the ordinary mode of examination. I could further roll between the finger in the bladder and my right hand, now placed above the pubes, a number of sections of the small intestines. As to the tumor in question, I was unable to

trace any connection between it and the uterus, and I became convinced now, by the peculiarly smooth and slippery surface of the swelling in question, that I had to deal with a portion of an intestine, probably the S. Romanum, contracted around its natural or some abnormal contents, probably adherent to the bladder, the more so, as I thought to feel distinctly a membrane connected with it in the manner the mesenterium is with the intestinal tube.

I will here remark a circumstance which happened during the examination, namely, that I withdrew my finger from the vagina and passed it directly into the bladder again.

The examination was not followed by hemorrhage as on the first occasion. On the following day Mrs. B. complained of headache, and a burning sensation about the urethral orifice. Ordered one-half of a grain of morphia.

December 10th.—Patient had a restless night, complained of burning about the external genitals. Slight chill in the night, with increased pain in the region of the bladder. Acid urine of dark color.

December 11th.—Pains in the bladder not quite so severe.

On the following day Mrs. B. left the hospital in a carriage. I ordered her to take a dose of sulphate of magnesia every night for a week, and to call at my office after that time. I saw her again at the end of the month, when she told me that the pain in the bladder and during micturition had not left her for some days, but that she was quite free from it now. The laxative had acted freely. I passed again my finger into the bladder without previous dilatation and without giving any pain. No trace of the tumor could be found any more. On my request the patient presented herself to Drs. Byrne and Hesse, and the former told me at one of our meetings that he also was unable now to find the tumor.

Mrs. Bertha B—, 26 years old, born in New York, of an apparently healthy constitution, had always been well during her early youth. She married a sculptor about twelve years ago who had had gonorrhœa for six weeks—three years before his marriage. The patient became pregnant during the first year and was delivered of a healthy child; the labor had been normal. Soon after delivery she had an acute attack of what appeared to be pelvic peritonitis, from which she recovered

very slowly, and she has been ailing ever since. A white discharge was first remarked soon after marriage, which has never left her. The principal pains are in the left side of the abdomen, extending towards the back. They are increased before and during the scanty menstruation. She suffers also from pain in the occipital region of the head and from infra-mammary pain.

On examining the sexual organs in September, 1874, I found the vulva reddened and slightly excoriated. The uterus was in right latero-version, the neck slightly hypertrophied and indurated. The womb, as well as all of the lateral appendages, as far as they could be touched through the rather rigid vaginal roof, both laterally and behind the uterus, were extremely tender, the greatest amount of pain being excited by producing even a slight concussion of the womb in lifting it suddenly upwards.

By the speculum the neck appeared almost normal; very little congestion, a small superficial erosion on both lips, a slight ropy discharge issued from its orifice.

The sound passed into the body without difficulty, but the canal appeared very sensitive to the most careful touch of the instrument.

Diagnosis: Chronic endometritis, chronic perimetritis.

The patient was subjected to a treatment, consisting of small doses of the bichloride of mercury and the application of iodoform to the neck, with occasional local depletions by means of Buttlers' lance.

After she had undergone this treatment for about three months, she began to feel somewhat better, but lately the severe pains before, during, and after menstruation occurred again in a severe form. I therefore proposed to her to make a more careful investigation under chloroform. This was called for, as well by the unusual thickness and tenderness of the abdominal walls, as by the rigidity of the vaginal cul-de-sac, in consequence of which it was impossible to feel anything beyond the cervical portion of the womb.

Mrs. B. was put under the influence of ether by Dr. Mackenzie on the 12th of January of this year. The urethra was dilated rapidly by bougies, and an instrument, which I shall exhibit to you, introduced into the uterus. I now passed the forefinger of my left hand into the bladder and that of the right into the rectum, while the doctor pulled the uterus down-

wards and turned it alternately into the right and again into the left half of the pelvis, according to my directions.

By this manœuvre I was enabled, first, to touch the whole of the uterus, which appeared in its supravaginal portion not enlarged in either sense, but somewhat softer than usual. When the womb was turned far towards the right, I could distinctly feel and roll between my fingers the left Fallopian tube to quite a considerable distance from its uterine insertion. It was enlarged, and irregularly so, to the size of an ordinary goose-quill, and so painful under palpation that the patient moaned even under the influence of the anæsthetic when I touched it. In pushing the right latero-version, thus artificially produced, to its fullest extent, I could just touch the left ovary, which appeared to be rounded off and softened. The touch of the right tube was equally distinct; it was normal, as far as I could judge. The right ovary was out of reach.

My diagnosis was thus completed: chronic pelvic peritonitis from catarrh of the left Fallopian tube.

The reaction, after this necessarily rude examination of parts already very sensitive, was pretty severe. The patient was confined to bed for four days from subacute perimetritis and cystic catarrh. The symptoms from the former yielded pretty rapidly under the use of morphia and quinine. Remains of the latter lasted for several weeks, and were at last entirely removed by the use of ethereal extract of enebis.

Mrs. A. G——g, 38 years old, born in New York City, has been a patient of mine for the last fifteen years. Was married to her first husband about twenty years ago, and to her second husband four years ago. Had three healthy children from the first and one from the second husband. During the first years of her marriage I treated her for anteversion of the womb and various ailments resulting from this cause. I have seen her every year since that time, and although there was a considerable diminution of all the rational symptoms referred to the dislocation, the womb was always found to be slightly anteverted.

After a long interval she called on me again on the 10th of December, 1874, complaining of abdominal pain, a sensation of pressure and bearing down in the lower part of the abdomen. Her menses had been regular. On examination I found

that there existed now a decided right latero-flexion of the womb, the body of which appeared to be somewhat enlarged.

On the 23d of that month she called again, complaining of more intense suffering, and especially of severe backache. On examining the uterus I found it now in a state of retroversion and retroflexion. To relieve her pains I introduced a double-lever pessary, which replaced the uterus entirely.

I saw her again on the 23d of January, and, on examining, I thought I could feel by the ordinary double-touch that the fundus uteri was in its normal position, while that what had at first appeared to be a retroflected uterus was a soft tumor protruding from the posterior wall, which part of the uterus appeared to be softer and larger than when I first detected it. I now began to think that I might have to deal with an extra-uterine pregnancy. Although the menses had appeared at their regular time (which had never occurred in one of the four previous gestations) there was now morning sickness and stitches in the breasts.

I began to carefully dilate the urethra with my bongies, and succeeded in introducing them up to No. 18 without giving much pain. The patient returned to her house in Williamsburgh without much inconvenience.

She called again on the 26th of January, when I began dilatation with No. 16, and succeeded in passing No. 24, and after that the fifth finger of my left hand. This latter proceeding was painful, and was followed by a slight hemorrhage from a small rent in the upper wall of the urethra.

On February the 1st I succeeded (without the least obstacle and without giving pain) in introducing the small and immediately afterwards the index-finger of the right hand. The womb was again retroverted, and I only succeeded in touching part of the anterior wall through the bladder. I could feel distinctly that it was swollen and softened; and running upwards from the left supravaginal portion towards the right horn there was distinctly perceptible an enlarged vein of the size of the radial artery.

On the 6th of February I again introduced the fifth and immediately afterwards the index-finger of the right hand. I then introduced the forefinger of the left hand into the vagina, and pushed the cervix backwards as far as I could manage to



do. Thus I succeeded in touching the greater part of the anterior wall of the uterus, the ridge formed by the apex of the fundus, and about one inch of the uterine part of the right Fallopian tube, while the end of the left tube was just perceptible where it joined the fundus. I now introduced the finger from the vagina into the rectum, thus pushing the body a little more forwards, when I succeeded in touching the greater section of the posterior wall with my finger in the bladder.

The impression which I received from this mode of examination was that the uterus, from the fundus down to a section of the cervix, situated somewhat above the vaginal junction, was enlarged in all its diameters, but more so in its posterior than in its anterior surface; that it was succulent and softened throughout its entire mass. I could distinctly feel a number of bundles of its muscular layer somewhat more prominent than others, and two or three enlarged vessels. The sensation was so characteristic that I became at once convinced of the existence of intra-uterine pregnancy—a pregnancy where the ovum had probably been attached to the right wall and somewhat posteriorly, in consequence of which those parts had become enlarged sooner than other portions of the womb.

*Sphere of Usefulness and evil Consequences of the Operation.*—It is needless, after the foregoing communications, to bring forward new arguments in favor of the vesico-vaginal and rectal touch.

I have examined now thirteen cases in this manner, partly for affections of the bladder itself, partly for diseases of the uterus and appendages, and lastly, in a few instances, with a view of testing the efficacy, the advantages and disadvantages of the proceeding.

I have only succeeded lately in obtaining all that I desired to gain from this investigation, because it requires a certain experience in the management of the parts involved in order to extend the sphere of the touch. I have thus become enabled to feel the outside of the whole of the uterus itself, one or both of the Fallopian tubes, either in part or to their full length, and in two instances I could feel the ovary.

The reaction following the operation, consisting either of retention of urine for a short while, followed by frequent

inclination to pass water, with a sensation of burning about the vulva, or more or less severe abdominal pain, amounting in one instance to a renewed attack of perimetritis,—this severe reaction took place in a more or less marked form in six cases. In one of these the examination was borne without any inconvenience the first and second time, while the third examination produced a pretty severe catarrh of the bladder. In all of these, rapid dilatation under chloroform was performed; in five of them there existed signs of chronic perimetritis; in the seven other cases there was observed very little irritation. In six of these, rapid dilatation was resorted to; in one the slow method was employed.

In none of the thirteen a permanent painfulness of the parts involved, or incontinence, resulted from the examination; in a great many of these the orifice remained large enough to introduce the forefinger without previous dilatation.

Hemorrhage occurs in all cases, but ceases of itself. In one instance where I examined a patient for the second time, when she was near her menstrual period, she bled profusely, an accident which did not occur on the first examination.

*Indications.*—The vesico-vaginal and vesico-rectal touch is to be confined in its application to certain morbid conditions of the womb. It is not to be considered a supplement to the ordinary gynaecological examination.

Its sphere of application applies, however, to all those cases where an ordinary examination, performed by an experienced and skilful specialist, has not succeeded in giving full satisfaction, on account of thickness or rigidity of the abdominal and vaginal walls.

It is indicated:

1st. For the diagnosis of obscure tumors in the tissue or in the neighborhood of the womb. I need hardly recall to your mind or specify the instances of small tumors located within the pelvis, the nature of which was never cleared up by the ordinary method. I have no doubt that in cases of this kind the new *modus examinandi* will reap its most welcome harvest.

2d. To complete the diagnosis of inversion of the womb.

From my impressions as to the distinctness of touch, I do not hesitate in stating that it will remove all doubts under the most difficult circumstances.

3d. In cases of suspected congenital absence or malformations of the uterus we will be enabled to make both fingers meet, introduced into the bladder and rectum, with the interposition only of the thin membranes of the bladder and rectum; in cases where the uterus is absent, and where there exists an arrest of development, its shape will be recognized distinctly.

4th. For the early diagnosis of pregnancy. Under these circumstances, this mode of examination is destined to supplant the sound, the use of which is not permitted under the circumstances. By measuring beforehand both the forefingers of the right and the left hand, we will be enabled to get at an accurate measurement of the uterus itself. If we take into consideration its volume and the nature of its tissues, of which we may gain a perfect knowledge by the vesico-rectal touch, we will be enabled to exclude or to recognize an altered physiological development of this organ. In cases of extra-uterine gestation we will not only feel the peri-uterine tumor, but also the enlarged and softened uterus itself.

The 5th indication for the employment of the vesical touch I will call Huguier-Pippingsköld's indication. Both of these surgeons have succeeded in guarding the bladder from injury, in removing part of the supra-vaginal neck, by introducing one finger into the bladder while the operation was performed. Prof. Pippingsköld, of Helsingfors, has published four cases in "*Beiträge zur Geburtshülfe und Gynäkologie*," vol. iii., sect. 2, Berlin, 1874, in which he employed this proceeding. In this he followed the example of Huguier, who had applied the same manœuvre in two cases.

The 6th indication comprises the diagnosis of the extent of heteroplasmic tumors of the neck.

The principal question in ventilating the chances of success in an operation for cancer, caneroid, or sarcoma of the neck, centres in the decision how far the supra-vaginal part of the neck has been involved. The new method of exploration will dissolve all doubts in this matter.

#### DESCRIPTION OF OPERATION.

*Preparation of the Patient.*—It is of importance to empty out the bowels, previous to the examination, in order not to have any encumbrance to hinder the finger, placed in the rectum, to

receive as complete an impression only from the parts, to which its sense of touch is to be applied, as can be possibly done. Before dilating the urethra, it is of the utmost importance to thoroughly wash out the vagina with carbolic acid and water. I have seen that utero-vaginal secretion was carried into the urethra by the bougies employed for dilatation. The fingers to be used in the examination through the bladder, vagina, or rectum must be anointed by a substance which contains one or the other of the disinfecting agents. All these precautions are necessary to counteract the effect of vaginal mucus on the urine, as a cause of alkaline fermentation.

After sufficient dilatation the patient must be placed in the so-called sacro-coccygeal position, the thighs being well flexed on the abdomen. This is necessary for the reason that the urethra runs parallel to the posterior surface of the symphysis pubis. If, therefore, we place the patient in the position indicated, a line drawn through the urethra and prolonged will touch the upper part of the womb. After sufficient dilatation, the forefinger of the left hand is now introduced into the bladder, previously emptied, while that of the right hand is placed into the vagina or the rectum, or alternately into the one or the other. It will then strike, when the uterus is in normal position, a point half-way between the fundus and the inner os.

In cases where the parts immediately above the neck, or morbid conditions of the regions in this neighborhood, are to be explored, it is not necessary to dislocate the uterus.

If, however, we attempt to explore the upper section of the uterus, it must be pulled down by means of a double hook, the points of which are turned outwards, introduced into the cervical canal, and it is perfectly safe to dislocate the uterus downwards about an inch and a half. When this is done, with one finger in the bladder and one in the rectum, we are enabled to thoroughly explore the whole of the uterus, from the fundus down to the external orifice.

In cases where it is desirable to push the examination beyond the uterus toward its appendages, the broad ligaments, Fallopian tubes, or even the ovaries, an instrument must be employed, by which we are enabled not only to pull down the uterus, but to turn its body either towards the right or the left side of the pelvis. For this purpose I have added to my double hook the

upper  $2\frac{1}{2}$  inches of the ordinary uterine sound. By its means we cannot only explore the sides of the uterus, its posterior surface, but even its lateral appendages to a certain extent. The

introduction of the sound has the further advantage, that we can crowd the uterine tissue against it, and thus judge of its thickness and density. If the posterior surface of the uterus or of the broad ligaments should become special objects of investigation, it might be well to substitute the uterine redresser, usually applied for the replacement of a retroversion.

After the examination has been completed, the bladder should be washed out with a weak solution of carbolic acid.

*Methods of dilating the Urethra.*—We may divide these methods into two different classes, the rapid and the slow process.

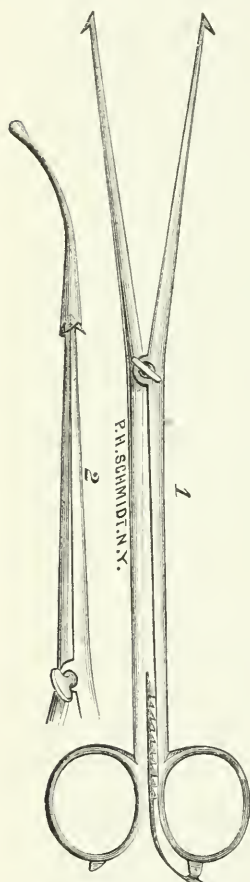
*First*, dilatation in one session. This may be effected in three different manners.

The quickest way of proceeding consists in introducing a Holt's stricture dilator and push the largest steel bougie at once between the two blades of the instrument, and to follow up the dilatation thus obtained, by using Busch's dilator sufficiently to permit the fifth finger to enter the urethra, after which the second passes in without difficulty.

The next, somewhat more protracted method, consists in using steel sounds of gradually increasing calibre, the last of which equals in size the forefinger.

Lastly, rapid dilatation of the urethra may be effected by Molesworth's hollow india-rubber bougies, to be expanded by forcing water into them from a syringe.

For the slow process we have two methods.



No. 1. Double hook for dislocating the uterus downwards.  
2. The same with intra-uterine part of sound attached for downward as well as lateral dislocation.



The first consists in the use of graduated bougies, the same as employed in rapid dilatation. They may be employed two, three, or four at a session, in intervals of two, three, or four days, according to circumstances.

The second method is that proposed by Prof. S. Pippingsköld (Helsingfors, Finland) preliminary to the amputation of the cervical portion of the uterus. He introduces three small, smooth laminaria bougies into the urethra and leaves them in place all night.

Among these several methods we have to choose in a given case.

From the as yet limited experience I have received, the impression that the reaction following dilatation of the urethra and examination of the bladder, depends not so much on the manner or rapidity of dilatation, but rather on other circumstances, such as state of general health, power of resistance of the patient, and elasticity and vulnerability of the tissues constituting urethra and bladder. In cases where the individual is very sensitive, or where the dilatation precedes an operation on the neck of the womb, it would certainly be better to perform it in one session and under chloroform. I have seen, however, two instances, where rapid dilatation could be performed to its utmost extent in one session without the use of an anæsthetic.

In cases where the examination permits ample time for preparation, and where the patient is not very sensitive, where the introduction of the smallest dilator is not followed by severe reaction, it would be better to resort to slow and gradual dilatation.

If the urethra is very sensitive, where there exist symptoms of perimetritis, and where the history reveals the existence of urethral or vesical catarrh at a former time, more or less severe reaction is pretty sure to follow, no matter whether dilatation be performed slowly or rapidly.

The reports of four cases, treated by Prof. Pippingsköld's method, are so favorable that I recommend this method for trial. We must, however, bear in mind that a proceeding which is borne very well by patients of that robust, northern physique, as we find them in Finland, is very likely to lead to very different results among our weakly, irritable New York female population.

In conclusion, I have to remark that the novelty of the proceeding presented to you is not in the dilatation of the urethra for the purpose of entering into the female bladder, but in its application for examining the female genital organs.

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### THE USE OF THE HAND TO CORRECT UNFAVORABLE PRESENTATIONS AND POSITIONS OF THE HEAD DURING LABOR.

By JOHN S. PARRY, M.D., Attending Accoucheur to the Philadelphia Hospital, Vice-President of the Obstetrical Society of Philadelphia, etc., etc.

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(Read before the Obstetrical Society of Philadelphia, Nov. 6th, 1873.)

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It is only after much thought and a series of observations, which have extended over a number of years, that it has been deemed proper to direct attention to the method of changing unfavorable presentations and positions of the child's head which is about to be described. The recommendation will be sharply criticised by some and emphatically condemned by others, but it is made, after no inconsiderable observation, with the earnest hope of diminishing the suffering of woman in the discharge of her highest function, as well as to preserve the lives of her children. The responsibility of making the suggestion is fully realized, and at the very outset it is desirable to distinctly reassert the truth of the trite maxim, that "Meddlesome Midwifery is bad." The faithful student soon perceives how wonderful Nature is in her resources, how marvellously she adapts means to ends in emergencies, and he becomes convinced that, though labor is likely to be more painful, more tedious, and consequently more dangerous in the class of cases alluded to, the child will usually be born without assistance. As will be stated more fully in the sequel, it is not intended to recommend indiscriminate interference in unfavorable cephalic presentations and positions, but to suggest a method of rapidly terminating labor in certain cases in which Nature *is unable* to complete her work.

The hand may be employed to facilitate delivery in a series of widely different conditions, but the following are those to which attention will be directed in this paper:

1. To flex the head when partially extended in all its presentations.

2. To transform occipito-posterior into occipito-anterior positions.

3. To change presentations of the face with the chin behind, into those of the vertex with the occiput in front.

Of the first class of cases nothing will be said at present. In regard to those of the third no excuse need be offered for any reasonable attempt to mitigate their severity or to remove their dangers, which all accoucheurs recognize and deeply deplore. In relation to the posterior positions of the occiput it is not improper to make a few preliminary remarks.

It cannot be denied, as was shown by Nægèlè, that in the large majority of these cases the head will rotate, and labor be terminated by the spontaneous change into right or left occipito-anterior positions of the vertex. It cannot, however, be admitted with the same high authority that in these cases labor terminates as quickly, with as little suffering, and as favorably both for mother and child, as if the occiput was originally directed to the anterior portion of the pelvis. The experience of most accoucheurs will confirm the opinion of Hodge, America's most eminent obstetrical authority, that, in consequence of the distance which the head has to traverse, labor is more painful, tedious and dangerous.

In consequence of these facts, most authorities in obstetrics insist upon the necessity of carefully watching the progress of the labor in occipito-posterior positions, and of facilitating rotation if it does not occur spontaneously. This is to be effected by pressure made either by the fingers applied to the temple of the child, or the vectis over the occiput, while Simpson and his followers direct the accoucheur to turn the head of the child with the forceps when it has descended to the floor of the pelvis. The latter proposition need not be discussed here. The former methods are entirely practicable in most instances, and at the bedside it will rarely be found that any other manipulations than those with the finger or vectis are necessary; but it is unfortunately the occasional lot of the obstetric physician to fail in his attempts to produce anterior rotation by any of the means which are usually advocated by authorities upon this subject. Under these circumstances the forceps might be applied and an

attempt made to drag the child into the world in its unfavorable position, or, failing in this, version might be resorted to, an operation which in cases of delay is very difficult to perform, and is dangerous alike to mother and child. Besides these, the only other resort is craniotomy, the most terrible of all operations in obstetric surgery. It is to add another to the obstetric physician's resources, and to enable him to avoid this terrible alternative that this paper has been prepared.

In the ordinary manipulation to rotate the occiput in front in posterior positions, the operator is told to change the position slowly in order to avoid injury to the child by twisting its neck too suddenly. That accidents have arisen from this cause, and the child's life been sacrificed in consequence, is probably true, but it seems not unlikely that the shoulders rotate in the cavity of the uterus more frequently than is usually supposed, and that the child will bear, without injury, more manipulation than is generally believed. As this point is not fairly established, and as change of the occiput from an unfavorable to a favorable position may involve rotation of the head over a considerable portion of one side of the pelvis, it is to be distinctly understood that the manipulation, which will be described presently, is only to be resorted to in those rare cases in which nature is not equal to her work, and after the more common means have been fairly tried and have failed, when the only other alternatives are version under great difficulties, or if this proves unavailing, craniotomy.

The procedure is applicable to cases in which the head is arrested either at the brim or in the upper portion of the cavity of the pelvis. Before it is resorted to the accoucheur must be absolutely certain of his diagnosis. About this there must be no error, or otherwise he will convert a favorable into an unfavorable case. I have never allowed myself to employ the hand in the manner and for the purposes about to be described, until the diagnosis had been confirmed by a thorough examination with the whole hand introduced into the vagina. As this occasions pain, it is almost always necessary to make the examination with the patient under the influence of ether.

The operation may be performed with the patient upon her back or in the ordinary obstetric position on the left side. The former is far preferable in most instances, as it allows more

freedom of manipulation, and the back of the hand applies itself more perfectly to the hollow of the sacrum. The patient should be brought close to the foot of the bed, with her buttocks at the edge of the side, and her feet supported on two chairs or by assistants. The physician now takes his position at the foot of the bed and at the right side of the woman. Previous to proceeding with the manipulation the patient should be thoroughly etherized, since the best directed efforts may fail if she is not perfectly relaxed. This done, and the bowels and bladder having been emptied, the accoucheur is to pass his right hand, well oiled, into the cavity of the pelvis, the dorsal surface of the fingers being passed along the hollow of the sacrum over the posterior portion of the presenting part, and the thumb behind the pubis over its anterior portion. In the meantime the left hand has been applied to the fundus of the uterus to steady that organ. This accomplished, the next movement is to carry the head of the child, which lies in the palm of the hand in the vagina, well up above the brim of the pelvis. The following steps of the operation vary with the presentation and position. If it is simply a partially extended vertex, or a brow-presentation with the occiput in front, the head is simply flexed, after which the case may be left to nature, or the forceps applied, as may seem best.

If the case be one of a face-presentation, with the chin behind, the head is to be completely flexed, and the presentation and position changed to the most favorable of all others, an occipito-anterior of the vertex.

In occipito-posterior positions of the vertex more is needed than simple flexion. The head of the child is grasped in the hand with the fingers over the occiput and the thumb over the forehead or temple. Having lifted it above the brim and secured flexion, the left hand is to be removed from the fundus of the uterus where it has been employed in simply supporting the organ. It is now to be used to force rotation of the body of the child by external manipulation, the anterior shoulder being the point against which these efforts may be directed with the most effect. While the shoulder is being pushed to the opposite side of the cavity of the uterus, the hand in the vagina acts upon the child's head and rotates the occiput from a posterior into an anterior position, of course imitating nature in the



manœuvre, and changing a right posterior into the right anterior position, and a left posterior into the left anterior occipital position.

If the uterus will now contract strongly, the hand may be retained for a little time until the head is fixed in its new position, when the case may be left to nature. If pains have ceased the forceps are to be applied above the superior strait, before the hand is removed from the vagina. Inasmuch as this may be necessary this instrument should always be at hand before commencing the manipulation. For the same reason the patient is to be placed in a position in which the application of the forceps may be made at any time, and because the blades of the instrument have to be carried high up, it is necessary that the buttocks be placed close to the edge of the bed, so that the handles can be pushed well back on the perineum. For precisely the same reason the right hand only should be used to act upon the child's head, since the right or male blade of the forceps has to be introduced with the left hand.

The application of the forceps is not more difficult under these circumstances than in the ordinary high operation, except that the blades have to be passed up rather higher than when the head is driven down upon the brim by the uterine contractions. Some care has also to be taken to prevent the head leaving the new position during the introduction of the first blade, but more especially immediately after the removal of the right hand and during the preparations for the introduction of the second blade. The first blade having been put in position, the hand should not be removed too soon, not until the head has been carried to the opposite side of the pelvis, when the blade of the forceps fairly applied to the side of the child's head is to be used as a lever in the absence of the hand to fix the presenting part against the pelvic wall of the opposite side, in order to prevent the possibility of its returning to its original position. At this point in the operation an intelligent assistant, or at least one who can be trusted to execute all directions faithfully, is necessary to steady the blade while its fellow is being introduced. The left blade of the instrument is to be introduced as in ordinary cases, the accoucheur having always assured himself that the head has not changed its position previous to its introduction.

To illustrate the opinions which have been enunciated the following histories of cases are related. Others might be published, but these are sufficient to illustrate all the principles to which attention has been directed. The first, though a very difficult labor, is one of the simplest of the class of occipito-posterior positions. In this instance the combined manipulation was successfully resorted to without a previous trial of the vectis, which was not at hand. In the second, though anterior rotation was accomplished with the forceps after considerable difficulty, the woman could not be delivered until flexion and rotation had been secured by elevation of the head above the brim. In the third the manœuvre was successfully employed in a case in which the face presented with the chin behind, and in which my conviction is, that it saved the life of the child. In the fourth it was an important aid to delivery in a difficult case of craniotomy.

CASE I.—*Right occipito-posterior position. Rotation of the occiput with the hand. Delivery with the forceps applied above the brim.*

Mrs. Dr. W., æt. twenty-six, primipara, was seen in consultation with Dr. Harrison Allen, at 11 o'clock A.M., on June 14, 1872. She had at that time been in labor for nearly twenty-four hours and the first stage had terminated two hours and a half before I reached her. During that time there had been no advance of the head, though she had strong pains. Although a vigorous woman, she was much exhausted, with a furred tongue, rapid pulse, hot skin, and hot, dry vagina.

There was a large caput succedaneum which, with the violent bearing down efforts produced by the examination, prevented the recognition of any suture or fontanelle, but the position was supposed to be the left occipito-anterior. The head was arrested and seemed to be fixed at the superior strait.

Wallace's forceps were applied at the superior strait, and though strong traction was made, the head could not be made to descend in the least, while the patient complained of severe pain in the abdomen and back in the region of the promontory of the sacrum. She said that it felt as if something was fast inside of her and was being dragged away. It was painfully evident that the obstruction could not be overcome by any justifiable traction, as the forceps were applied; and it appeared

probable that there had been an error in the diagnosis of the position. Re-examination did not enable us to correct this, owing to the presence of the large caput and the violent expulsive efforts produced by the manipulation.

She was now thoroughly etherized, and a right occipito-posterior position diagnosticated, after which I passed the right hand into the vagina, and placing the fingers over the occiput and the thumb over the brow, with considerable effort raised the head above the brim of the pelvis and made forcible flexion. Then placing the heel of the hand upon the exterior of the abdomen, below and to the right of the anterior shoulder of the child, I attempted to rotate the body in the uterus, while at the same time I easily rotated the head to the right occipito-anterior position. The male blade of the forceps was introduced without removing the hand, after which Dr. Allen supported the handle, and using it as a lever made mild pressure upon the side of the head. An examination proved that the position was unchanged, and the second blade was applied. The instrument (Wallace's) locked easily; the lock being in contact with the vulvar orifice. Strong traction was required before the head passed the pelvic brim. It descended rapidly through the cavity, but was delayed upon the perineum, owing to the extreme distention of this structure which was necessary before the head could be born. This occurred a little more than an hour after the application of the instrument. The body was born in a few minutes, restitution occurring as in cases of primary right occipito-anterior positions of the vertex.

The child, which was above average size, breathed feebly at first, but soon cried lustily. There was temporary paralysis of the portio-dura of one side from pressure of the forceps. The mother's recovery was uninterrupted and perfect.

CASE II.—*Left occipito-posterior position of the vertex. Rotation with straight forceps. Attempt to deliver with Hodgè's forceps by rotating the occiput into the hollow of the sacrum. Anterior rotation of the head by manipulation. Delivery of a living child by Wallace's forceps. Recovery of the mother.*

E. B., æt. 20, single; born in Ireland, fell into labor at full term, in the Philadelphia Hospital, about noon, April 25, 1871,

under the care of Dr. A. W. McCoy, resident accoucheur. The duration of the first stage was eighteen hours and a half. The membranes ruptured at 6 A.M. on the 26th, and during the succeeding hour the vertex descended into the upper part of the cavity of the pelvis. At this point the progress was arrested, though the pains continued strong. I reached her at 10 A.M. She had a cool skin, clean tongue and strong pains. The vagina was cool. The head, a large one, was in the left occipito-posterior position, and the occiput was disposed to rotate into the hollow of the sacrum. There was a middle-sized caput succedaneum and the head, which was partly extended, had commenced its descent into the cavity of the pelvis. The anterior portion of the head was tightly wedged against the anterior parts of the pelvis, though there was more room behind. The labor had not advanced any for three hours, and the patient, though not exhausted, was beginning to show the effects of violent exertion, and begged that the labor be terminated with instruments. The forceps were therefore resorted to.

Straight forceps (Beatty's) were applied without great difficulty, and the head was rotated from the left occipito posterior into the anterior position of the same side. This was effected with some difficulty, and only after the exertion of more force than we thought good for the child. As soon as the force was removed, the head with the forceps would immediately return to the original occipito-posterior position. Strong traction was now made and continued for some time, but without producing any progress in the labor. The straight forceps were removed with great difficulty, an ear of the child being caught in the fenestrum of one of the blades.

An attempt was now made to secure perfect flexion, but was unavailing. Wallace's forceps were then easily applied with the head nearly in the first position of the vertex. Very powerful traction was continued for an hour, but without advancing the labor. The instrument was then removed, when the head returned spontaneously to its original position. Hodge's forceps were then tried, with the head in the left occipito-posterior position, and the delivery attempted by rotating the occiput into the hollow of the sacrum, but though violent efforts were made the head did not move in the slightest.

These manipulations occupied more than three hours, and

the waters had been evacuated for more than seven hours, yet the fœtal heart was still beating loudly. In consequence of the partial descent of the head in the cavity of the pelvis, and the rigid contraction of the uterus, version had been considered, but declined. No alternative seemed to be left but craniotomy, when she was brought thoroughly under the influence of ether, and the whole hand passed into the pelvis, with the fingers over the occiput and the thumb over the temple. With a strong effort I succeeded in carrying the head above the pelvic brim, after which flexion was easily perfected, and by combined manipulation the occiput was brought round into the first position of the vertex, and the body was turned in the cavity of the uterus. It is possible that version could have been performed at this time, but after a little thought I determined to repeat the attempt to deliver with the forceps. Wallace's instrument was easily introduced, and after strong traction the head was dragged through the superior strait and pelvis, and was born at two P.M. Much force was necessary to effect delivery of the shoulders and pelvis of the child. The child, which weighed nine pounds, cried feebly when born, but was soon resuscitated. There were superficial abrasions beneath each ear. The caput succedaneum was very large, and the head was much distorted from the prolonged pressure. There was some hemorrhage, and the patient had a violent attack of puerperal fever, which was epidemic in the hospital at the time, but finally recovered.

CASE III.—*Face-presentation. Chin behind and to the right side. Failure of attempts at flexion, rotation, and version. Application of Hodge's forceps, and failure to produce rotation or to deliver by traction. Introduction of hand; elevation and flexion of the head. Delivery of a living child with the forceps.*

Mrs. P., æt. about 30, was seen in her sixth confinement in consultation with Dr. Elliot Richardson, at 9½ A.M., on June 23d, 1871. She had reached full term, and labor had commenced some time during the previous day. The first stage terminated between 11 and 12 o'clock on the preceding night. When Dr. Richardson reached her the face was presenting



with the chin to the right sacro-iliac junction. The pains were strong, and continued so throughout the night.

When I saw her at 9.30 A.M. the next morning her condition was fair, but the position remained unchanged. Her pains were moderately strong, her mind was wandering, and she was fearful that her labor would terminate badly. The face had descended almost to the inferior strait, and the anterior lip of the uterus was compressed between the brow and the symphysis pubis. There was plenty of room posteriorly, but the forehead was tightly jammed against the left anterior portion of the pelvis. An attempt was made to transform the face-presentation into one of the vertex by pushing up the chin and bringing down the brow. This failed, although the whole hand was introduced into the vagina, because the chin caught against the brim of the pelvis behind, and the forehead would not descend in front. We likewise failed to rotate the chin in front, though the hands and vectis were both used. Three hours had now passed and the patient was both alarmed and exhausted. Version could not be performed, so Hodge's forceps were applied, and an attempt made to force rotation. Traction was continued for nearly an hour without effecting anything. Almost in despair, I passed the whole hand into the pelvis, placing the thumb over the brow and the fingers over the superior maxillary bone, and pushing forcibly upwards, the head was easily raised above the brim of the pelvis. It was then flexed without any difficulty, and a mento-posterior position of the face was converted into an occipito-anterior of the vertex. As the pains had almost ceased, Wallace's forceps were applied, and at 1 P.M., a few minutes later, the child was born. It was of average size, was almost still, but after a little effort was fully restored. The mother recovered without any unfavorable symptoms.

CASE IV.—*Right occipito-posterior position. Prolapse of the Cord. Pelvic deformity. Failure to deliver after violent efforts with the forceps. Craniotomy. Rotation with the hand, and delivery with the forceps.*

Mrs. O——, a primipara, aged about 20 years, was seen in consultation with Dr. James F. Wilson, at 4 P.M., July 10, 1873. Labor had commenced early on the previous day. The bag of

waters ruptured early on the morning of the 10th. In the afternoon the cord prolapsed opposite the sacro-iliac junction, and Dr. Wilson was unable to return it. The head was in the right occipito-posterior position and well flexed. The pelvis was narrow, the conjugate diameter scarcely exceeding three and a half inches. When seen at 4 P.M. the os was dilated, the large firmly ossified head at the superior strait, a large loop of cord prolapsed, the vagina was hot and rather dry, the skin moist and warm, pulse frequent, and patient anxious about the result. She was immediately placed upon her hands and knees, but though prolonged and careful efforts were made to reduce the cord, we failed. She was then allowed to lie upon her back, and seizing the cord it was carried upwards into the uterus, it having been decided to place it if possible out of danger, and then to go on and seize the feet, turn and deliver. The right hand was used in the manipulation, and when it was in contact with the thorax of the child, the whole of the cord being within the cavity of the uterus, a strong convulsive movement of the infant was felt, and immediately afterwards the funis was perfectly flaccid in my fingers. It was too evident that the child was dead, and it was determined at once to apply the forceps and attempt to deliver.

Simpson's instrument was used, but the utmost force which Dr. Wilson could exert did not move the head in the slightest. The effort was continued as long as was deemed safe, when, the child being dead, it was determined to perforate. This was done about 6 P.M. The brain was cleared out, but delivery could not be affected by powerful traction either with crotchet, cranio-clast, or forceps. The upper portions of both parietal and occipital bones were pulled away with Meigs' forceps and the cranio-clast, but without enabling us to drag the head through the superior strait. The front portion of the head overhung the pubic bone to such an extent that we could not tear it away with these instruments, and for the same reason it was impossible to bring the chin down and to convert it into a mento-anterior position of the face, so that the latter could be crushed with the cranio-clast. No cephalotribe was at hand.

It seemed almost as if we would fail to deliver, when, with Dr. Wilson's concurrence, I determined to resort to the manipulation which has been described. The head was carried above

the brim, flexed, and the occiput rotated in front, after which Wallace's forceps were applied, and a few minutes before midnight the child, which was a large one, was dragged into the world after severe traction.

The patient suffered from retention of urine, slight abdominal tenderness and pain for a few days, but recovered perfectly.

The success of an attempt to perform this manipulation depends in a great measure upon thorough etherization of the patient. When the woman is entirely relaxed by the anæsthetic, it is very surprising what can be done by forcibly pushing the head upwards. Not only does the child ascend, but if the lower portions of the uterus have been carried with the head into the cavity of the pelvis, it may be lifted with its contents above the pelvic brim, when the latter become movable and easily manipulated. Both in the pregnant and unimpregnated women the degree of stretching and movement of which the generative organs are capable when the patient is completely anæsthetized, appears very remarkable to one who has never employed this important agent in such cases. Not only may the womb be depressed, as has been done during amputation of the neck of the organ, but it may be pushed upwards and the vagina stretched till it is above the brim of the pelvis. This was fully illustrated in the case of face-presentation which has been related. The same thing in the unimpregnated woman was illustrated with equal force after the preceding remarks had been written. A patient with a tumor which was lodged, and apparently fixed in the cavity of the pelvis, was thoroughly etherized, and the left hand, as recommended by Prof. Simon, was introduced into the rectum to make an exploratory examination, when the tumor was raised out of the pelvis, after which it could be pushed almost anywhere in the lower part of the abdomen. Not only was this the case, but the womb was likewise pushed upwards by the hand in the rectum, until the fundus, which could previously be felt in the usual position just below the upper margin of the pubis, was now just below the umbilicus, and the os uteri could be felt above the brim through the thin abdominal walls. The os was thus lifted above the point previously occupied by the fundus.

One other point is worthy of mention in connection with this

use of the hand to produce anterior rotation in occipito-posterior position of the vertex. It is that in cases in which the body of the child cannot be rotated in the uterus by external manipulation, the head may be turned and the occiput brought in front without any great danger to the child. It is certainly less dangerous than Simpson's manipulation with the forceps, because, in the former, the hand, the most delicate and useful of all instruments, is the agent employed.

The study of the literature of obstetrics, so far as the writer has been able to complete it, has failed to reveal any account of the manipulation which has been described, yet a point to which attention has been directed can scarcely be deemed strictly original, since obstetricians have at various periods, from the time of old Cosmo Viardel, directed attention to the use of the hand in obstetrics. It is only the new combination of various manipulations which have been recommended at various periods to which attention is directed.

1513 ARCH ST., PHILADELPHIA, Nov. 1st, 1873.

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## A CONTRIBUTION TO OBSTETRICAL SURGERY.

By ALEX. J. C. SKENE, M. D.,

Professor of Clinical Obstetrics in the Long Island College Hospital, Brooklyn, N. Y.

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CRANIOTOMY is comparatively a rare necessity, and one of the most troublesome and difficult operations in obstetrical surgery; consequently there are few, even among accomplished obstetricians, who acquire anything like dexterity in performing it. At the same time any unnecessary delay or error in manipulating increases the dangers of the operation, which, at best, is a very serious undertaking.

From my own observations I have learned that in operating according to the established methods, it is impossible to be sure of not wounding the mother, and thereby increasing the risks to her life. I have also found that after perforation and removal of the brain matter, delivery cannot be effected in all cases by the forceps or cranioclast, because, while one diameter of the head is reduced by compression, the measurement in the other direction is increased. After vain attempts in this way,

it is often necessary to reduce the head still further, by removing the cranial bones, either whole or in fragments. To do this in the usual way without wounding the mother, is often impossible.

This experience led me to adopt a modification of the ordinary operation of craniotomy, based upon one of the principles of gynæcological surgery, *i.e.*, operating through a Sims' speculum. In this way the operator is guided by the sense of sight in place of the sense of touch, and the cervix uteri and vaginal walls are guarded from injury by being kept out of the way.

The details of the operation are as follows: The patient is placed upon the left side, in a semi-prone position, and a Sims' speculum of the largest size is introduced. The anterior vaginal wall and the anterior lip of the os uteri are raised by an elevator or retractor. The cranium is then opened by the perforator—that of Professors Thomas or Braun being the best. A crucial incision should be made, and the corners of the bones broken off with the bone-forceps. This will give room for the rapid removal of the brain, which should be broken down and scooped out with a curette and a sponge in a holder. The bones of the head should then be separated from the scalp by a uterine sound, and each one removed by carrying one blade of the forceps into the cavity of the head, and the other in between the scalp and the bone, and twisting each piece from its attachment. If the space is small, each bone may be broken into fragments. By operating in this way the scalp protects the mother from laceration by the sharp edges of the broken and detached bones. Sometimes, when the bones are imperfectly ossified, they can be removed with more facility by using an ordinary bullet forceps.

When the cranial bones have all been removed in this way, the remains of the head can be delivered by the blunt hook and craniotomy forceps, or by applying the ordinary obstetric forceps. When the pelvis is very much contracted, the whole of the head can be removed in pieces in the way described.

Having found this method of operating advantageous in my own practice, it is offered to the profession in the hope that it may prove generally useful.

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## CLINICAL CASES.

### A PECULIAR CASE OF INSOLATION DURING PREGNANCY.

By M. B. WRIGHT, M.D., Cincinnati, Ohio.

EDITOR AMERICAN JOURNAL OF OBSTETRICS:

THE following case is presented, more on account of its novelty, than from deep considerations of practical value. Still, it is not without interest to the every-day practitioner. Those who are engaged in the investigation and elucidation of physiological phenomena may find it worthy their attention. It will greatly oblige me if some one of your numerous readers will communicate, through your columns, his views of the case.

Very truly,

M. B. WRIGHT.

CINCINNATI, Aug., 1874.

Early in July, 1874, Mrs. E. complained of great exhaustion, which was attributed to the high atmospheric temperature then prevailing. The pulse ranged from 120 to 140 per minute. The skin was hot and pungent—the severe pain throughout the head, and the extreme pressing weight along the parietal junction, were almost beyond endurance. Sleep could be procured only by enveloping the head in cloths wrung out of ice-water. These symptoms continued more or less until July 20th. At noon on that day, the patient was delivered of a fœtus, two or three weeks in advance of full term, weighing about six pounds.

Slight pains had occurred at irregular intervals for several hours, but the second stage lasted about one hour only. The membranes descended during each expulsive effort, and reached the vulva, while the head was at the superior strait. By strong pressure with the finger during a pain, they were ruptured, and now came the unusual features of the case.

There was nothing in the temperature or condition of the vagina to attract special attention. The liquor amnii, however, was so hot on the rupture of the membranes as to occasion a sudden and involuntary withdrawal of my hand from the geni-

tal region. With a few pains the fœtus was expelled, and the temperature of its skin corresponded with that of the fluids by which it had been surrounded. The placenta soon descended into the cavity of the pelvis, and when withdrawn felt as if it had been taken from a heated oven.

Having never witnessed, or rather felt such a case, I was inclined to attribute, after some reflection, the feeling of high temperature to an over-sensitive condition of my hands. The next morning I was convinced otherwise. The arrival of the nurse having been delayed, a lady friend had been entrusted with the care of the child. She had observed its condition, and asked if it had not a great deal of fever. Said she: "When I rubbed the lard over its body, it melted, and ran as if it had been put on a hot plate."

The next morning the patient was in a comfortable condition. The pulse had fallen to 80 per minute—the skin was natural, and all pain in the head had disappeared.

During most of forty-eight hours, the child moaned feebly, swallowed with difficulty the few drops of fluid placed in its mouth—gave but little motion to its limbs, and, in a word, looked and acted as if premature. It has been gradually improving, and is at this date in a good living condition.

On account of greater precision, and for the satisfaction of those whose sole reliance for temperature is on the thermometer, it is to be regretted that I could not use that instrument.

Previous to the rupture of the membranes I did not anticipate anything unusual. By the time they had reached the vulva bag-shape, the liquor amnii had accumulated in the cavity of the pelvis—the head of the fœtus being at the superior strait—and escaped in a single gush. There was no time for the use of a thermometer. Indeed, my surprise was too great for haste in judgment.

But I have been so long accustomed to the use of my fingers that I seldom call any other instrument to my aid. That there was unusual and extreme heat in this case within the cavity of the uterus, is beyond a doubt. The questions to be answered are, whence came the heat? how produced? What occasioned such a sudden and favorable change in the general condition of the patient?

The external temperature of the mother during the three

weeks preceding labor, was as great to my hand, as in any case of fever I had ever witnessed, and that of the fœtus and placenta seemed in greater excess. The sudden dash of the liquor amnii, may have added somewhat to the effect, but never before, during a practice of half a century, had I experienced the same sensation.

Again, does it not seem strange that such a combination of extreme symptoms in the mother should have disappeared in less than twenty hours, not to return? Some of my medical friends have said, "The child had fever and imparted it to the mother." Others say, "It is an unusual case of insolation." My own impression is, that all the phenomena are attributable to indirect solar heat. I can account for them on no other principle.

There was the sense of exhaustion—the frequent pulse—heat of skin—extreme pain in the head, with great weight at the vertex—an anxious and distressed expression of face. There was an absence of drowsiness, not to say stupor. At all times the mind was clear. These symptoms, while they indicate danger, directly or remotely, are not necessarily fatal. The death cases, such as I have seen, have been attended by more or less unconsciousness. Insolation in a pregnant woman is new to me, and the question presents itself, has the grave condition anything to do with the oppressive heat of July—the mean temperature from 1st to 20th of that month being 83 degrees? On the 7th of July the thermometer indicated 100 degrees in the shade. Scarcely an individual subjected to the "heated term," retained his elasticity and energy. Some exposed directly to the rays of the sun were suddenly killed, while others who were carefully guarded in their own houses were enfeebled, without appetite, nervous and wakeful. The patient before us, had been free, most of the time, from out-door heat. Her immediate surroundings were comfortable and cheerful. No cares pressed upon her, except those which appertain to the superintendence of household affairs.

At the time this patient was suffering, not delivered, a woman was delivered in the hospital. Five days afterwards she complained of great oppression from heat; she had a convulsion, the reaction was attended by rapid pulse—heat of surface—highest point  $109\frac{1}{2}^{\circ}$ —coma nearly profound. She lived ten days. To the touch, the first patient was hotter than this one.

The aggregate symptoms of insolation or sunstroke, differ in different individuals, whether the tendency is to death or recovery. There are some symptoms, however, which seem to have been present in all cases—cerebral disturbances, partly cardiac actions—undue heat of surface generally, or in particular regions.

Did the gravid uterus derive its heat from the general circulation, or was it the origin of the stated phenomena? <sup>1</sup>

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FROM THE TRANSACTIONS OF THE MEDICAL SOCIETY OF  
THE COUNTY OF ALBANY, N. Y.

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SEMI-MONTHLY MEETING, JANUARY 6TH, 1875. THE PRESIDENT, DR.  
JAMES S. BAILEY, IN THE CHAIR.

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TWO CASES OF INVERSION OF THE UTERUS.

DR. A. W. SHILAND reported the following cases:—

CASE I. Mrs. M. had had two children. The third labor was natural and without indication of the serious accident which followed.

Labor pains were severe, and did not abate with the delivery of the child. Her agony afterwards was fearful.

An examination found the placenta nearly expelled, and firmly adhering to the inverted uterus, which filled the vagina, and protruded from it like the head of a fœtus.

The placenta was separated with difficulty, after which the inverted uterus was carried as high up as possible on the palm of the hand, the os uteri was dilated, and the fundus restored to its normal position.

A few minutes was all that was necessary to accomplish the result. The recovery was rapid and complete.

Dr. Shiland remarked that in carrying the inverted uterus upward on the palm of the hand, the os dilated more readily than if pressure was made directly on the fundus.

CASE II. Was most distressing, and one of complete inversion.

<sup>1</sup> [It is greatly to be regretted that the omission to take the exact temperature with the thermometer, not only of the liquor amnii, but also of the mother and the child, materially impairs the scientific value of this certainly very unusual observation.—ED.]

The torture and agony incident to this accident has been endured for years by some women, but the careful physician will feel the terrible responsibility of the occasion, when noticing the pallor of death, and hearing the parting words of wife to husband and friends while anticipating a speedy demise. This condition is often owing to the incompetence of the midwife, and death may ensue from shock or hemorrhage.

The subject of this case was a primipara. Her pains were quite irregular until the last one, which was very severe, and the inversion immediately followed the expulsion of the child. The placenta was completely adherent to the inverted uterus, with the membranes attached.

The whole uterus lay external to the vagina, and appeared like the body of another child.

With the thumb and forefinger the neck could be encircled. The placenta was separated with difficulty. The woman was suffering the most intense agony, and assured her friends that she was dying. It seemed as if her convictions would prove a reality unless relief was soon afforded. No anæsthetic being at hand, Dr. Shiland at once endeavored to restore the inverted uterus, and found it a most difficult operation. The placenta was so firmly attached that its removal seemed impossible before the inversion was corrected, yet the inversion could not be restored without the separation of the placenta.

The removal of the placenta was effected and the uterus restored to its natural position as in Case I. The shortness of the cord had much to do in producing the inversion as it was not more than twelve inches in length. The patient recovered without untoward events.

In either case the accident was not caused by traction on the cord by the accoucheur.

Dr. Shiland remarked that he had attended the most difficult cases in obstetric practice, yet these cases were the most appalling. The suffering was so intense that immediate relief was necessary, which taxed the intelligence, skill, and immediate action of the accoucheur.

#### UNITED TWINS (*Xyphopages*).

DR. A. W. SHILAND presented the accompanying illustration of this interesting malformation, which occurred in his practice



in 1868, together with the following history of the circumstances of the birth : Mrs. Burns, Irish, æt. 25, had previously had four children, and since this anomalous birth has had six more, all of which are well-formed and healthy.



No case of twins before nor since this birth has occurred.

She had never before called a physician to assist in her confinements, and it was only when this case seemed desperate that she consulted Dr. Shiland.

The patient was above the medium size, and well-formed ; she had already been in labor several hours before the doctor was called, and then the pains were very severe and almost constant. The os was fully dilated, and the head was lying in the lower part of the pelvis, the vertex presenting, the advancement was slight, although the pains were constant. The forceps were sent for, and as there was some delay in obtaining them, the head had advanced until he could reach above the chin, and thus attempted to aid in its delivery, but it was firmly held by some connection which was not understood.

The severe pains, with the assistance he was enabled to render, brought the head so far down that respiration was possible, but there were no signs of life. More than two hours were consumed in the delivery thus far, and further advancement

seemed impossible. By pressing the hand up beside the fœtus the sternum was found firmly held, and continuous with what proved to be the body of another child.

It was with great difficulty that the arms and shoulders of the first child were brought down, and when this was accomplished there was even greater difficulty in advancing the body. Until this stage, Dr. Shiland had no suspicion of the union that existed. If the fact had been known no better service could have been rendered to the woman nor could the lives of the children have been saved.

After delivery had thus far been accomplished, the remaining portion, with the second child doubled upon itself, presented first the sternum, then the abdomen. With the gradual advance and delivery of the first child, the abdomen, hips, and lower extremities of the second child followed, and lastly the shoulders and head. At no time was there any indication of life.

The children were united by a firm bony and cartilaginous substance like the sternum. In fact, the sterna of both children were continuous, and like one breast-bone. The union extended backward as far as the median line, corresponding with the right and left sides of the bodies of the children.

The connection was four inches in length by one and a half or two inches in thickness.

Both children were females, and weighed jointly eighteen pounds.

Their bodies could not be obtained for preservation or dissection. Their faces were slightly inclined towards each other. The drawing was made by Dr. Lewis Balsh, of Albany.

#### OCCLUSION OF THE VAGINA COMPLICATING LABOR.

DR. A. P. TEN EYCK said that occlusion of the vagina occurs but seldom, and when complicating labor is exceedingly rare. After diligent search among the literature accessible to him he had not been able to find a similar case. The case was related as follows: Ester Cenix, colored, æt. 29, medium size, and general good health. In September, 1868, became pregnant, though unmarried; labor set in June 4th, 1869, and continued during that day and the following, when the attending physi-

cian arrived. An examination was made, when he declared that delivery could not be accomplished without the aid of instruments. The patient consented, and an effort was made to apply them. After many trials he failed, and he claimed that in order to save the life of the mother he must resort to craniotomy. The patient again consented, and a pair of rusty scissors were obtained, and thrust into the vagina, and after repeated efforts, the child's cranium was punctured. The brain was removed, and so were the fragments of the skull, piece by piece, and lastly the body, which was not large, and did not present any unusual development. The placenta followed quickly, and the patient was left complaining of a severe smarting pain in the vagina.

The physician did not return again to visit his patient. She suffered continually for several months after the confinement, from pain in the vagina, accompanied with a profuse discharge. During this time she was not seen by a physician, and on account of not being married she felt a delicacy in consulting one. In 1871 she stated her case to a physician in Bath-on-the-Hudson, who, on examination, found complete occlusion of the vagina, whereupon he advised her to enter the Albany City Hospital, which she did, and was examined by the attending staff, also by the faculty of the Albany Medical College. She was operated upon, but was discharged unrelieved.

In 1873, Dr. Ten Eyck first saw her, and found on examination that there was no vagina, but immediately behind the labia was a firm, tense, smooth surface, with no apparent opening. She menstruated regularly, which convinced him that an opening communicated with the uterus. He found a small opening, through which he passed a probe for about an inch; here it was arrested, in consequence of the canal taking a short turn and passing in another direction.

An operation was decided on, but she soon married and did not return to him; soon after marriage she became pregnant, and on the 7th of November, 1874, the first stages of labor commenced. The pains continued at long intervals during the four days following, when the membranes ruptured and a small stream of water passed through the opening.

On examination nothing could now be found but the same tense, smooth surface, as before. The pains were now severe,

and occurred at short and regular intervals. The case was allowed to progress until the following day, when an operation was commenced in the following manner: The patient was placed in the usual position for using the forceps, and brought under the influence of chloroform. An incision was made through the smooth surface, behind the labia, to the depth of a quarter of an inch; the finger was then inserted in the opening and used as a guide in cutting. The adhesions, which extended the whole length of the vagina, were severed. The opening to the uterus was now large enough to admit the index-finger, but this was not large enough to allow the child's head to pass through, nor did the tissue seem at all elastic. By partly cutting and partly tearing the bands which constricted the vagina, an opening was made large enough to apply the forceps. After they were locked there was still great difficulty in delivering the child, in consequence of the unyielding condition of the soft parts. She was delivered of a still-born child.

The patient was unconscious of suffering and did well after delivery, and was more than thankful that her life was saved. The after treatment consisted in the introduction of a hard rubber vaginal syringe of considerable diameter, well lubricated with oil, in order to dilate the passage. An injection of warm water was also employed twice each day. Four weeks after the operation the patient was well, and the vagina was completely healed and capacious enough for all practical purposes.

#### CESAREAN SECTION ONE HOUR AFTER DEATH—REMOVAL OF A LIVING CHILD.

DR. J. H. BLATNER reported the following: Mrs. M., æt. 29, Irish. Multipara. Was taken in labor and seemingly progressing favorably, when suddenly she was heard to scream loudly and immediately expire.

A messenger was despatched for the doctor, who, with a knowledge of the case, seized a pocket case of instruments, and on his way thither summoned Dr. H. S. Case to assist him.

On arrival he found the woman apparently dead. The usual tests were resorted to, in order to ascertain if life was really extinct. When the question was settled he proceeded to per-

form the Cæsarean section, opening the abdomen on the line of the linea alba, from the umbilicus downward, to near the pubis. On incising the uterus the hemorrhage was considerable. It was a vertex presentation, and the fœtus was livid and apparently asphyxiated. It was estimated that the mother had been dead fully one hour. By extraordinary efforts the child was resuscitated and breathed for ten minutes and then expired.

The family would not consent to a thorough post-mortem examination, but an incision was made through the diaphragm and portions of diseased lung tissue were torn away. Dr. Blatner remarked in reference to the importance of this case in a medico-legal point.

The Society then adjourned.

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[Purely *Clinical* cases of interest will hereafter be published in this department.—Ed.]

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## TRANSACTIONS OF THE NEW YORK OBSTETRICAL SOCIETY.

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REPORTED BY PAUL F. MUNDÉ, M.D., SECRETARY.

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STATED MEETING, OCTOBER 6, 1874. THE PRESIDENT, DR. PEASLEE, IN THE CHAIR.

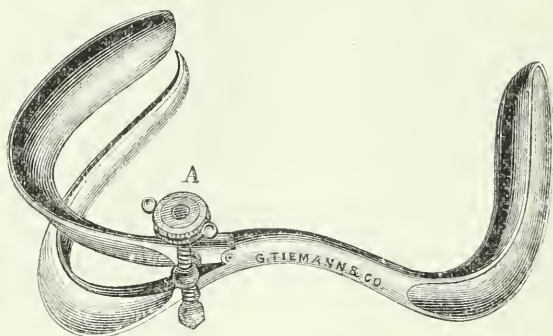
### MODIFICATION OF SIMS' SPECULUM.

DR. B. F. DAWSON exhibited his modification of Sims' speculum for the examination of very capacious vaginae. It consists of an ordinary Sims' speculum, the blade of which is divided in the longitudinal direction, and arranged to dilate by means of a screw. It is introduced and dilated before traction is made, it having been found that the dilatation is more difficult after traction. The instrument is serviceable in operations on the posterior wall of the vagina, which can be easily reached in its whole length when full dilatation is made, and also when the relaxed and pendulous vaginal walls would be likely to overlap the sides of the ordinary Sims' speculum and obstruct the view.

DR. PEASLEE said that in this latter class of cases he generally uses an extremely large speculum, but thinks that Dr.

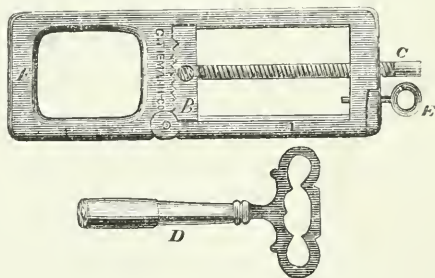


Dawson's instrument is decidedly more simple and convenient.



NEW OVARIOTOMY CLAMP.

DR. DAWSON also exhibited an ovariectomy clamp devised by himself, by means of which a complete and more equable compression of the pedicle is obtained than by Spencer Wells' clamp. The compression is exercised by two steel crossbars which are made to approach each other by means of a screw worked by a removable key, lateral compression being secured by the immovable steel shafts in which these crossbars slide.



The clamp is of such a length that its ends rest on the abdominal parietes and aid in uniting the edges of the abdominal wound. Spencer Wells has used it, praises it highly, and has had several made.

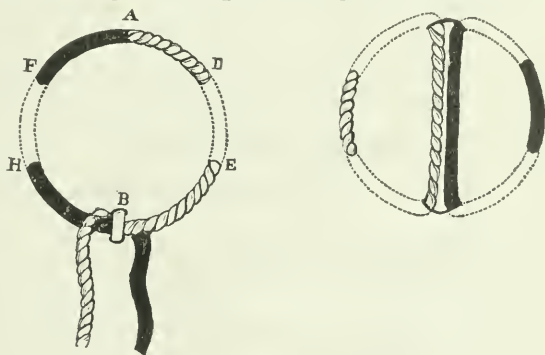
DR. PEASLEE thought it was the best clamp he had seen, and that it secured perfect compression. The opening seemed somewhat small for some pedicles, but there are cases in which no clamp would properly secure and compress the large fleshy pedicle.

## NEW FORM OF LIGATURE FOR THE PEDICLE OF OVARIAN TUMORS OR FOR OTHER GROWTHS REMOVABLE BY LIGATURE.

DR. PEASLEE presented the model of a new form of ligature for the pedicle of an ovarian cyst or for vascular tumors of the skin or other tumors removable by ligature, which had been sent him by DR. ALFRED HOSMER, of Watertown, Mass. The ligature is described by its inventor as follows:

"By a method of application about to be described, a single ligature may be made to effect the strangulation of pedicles and vascular masses, whose size (or shape) has required by the modes usually employed the use of two or more ligatures.

"Let the enclosed card represent the plane of ligation at the base of a naevus, for instance, and proceed as follows: Double a small, but strong, thread and carry the two cut ends together through the eye of a rather small needle. Let the point of the needle enter at A, and emerge at B, and carry it through with its double thread, leaving a short loop projecting at A. Then select a ligature of suitable size for the case in hand, and carry about half its length through the loop already left at A.



"By means of the small thread draw the ligature through the mass, from A to B, leaving at B a short loop, and at A the two free ends, or branches, of the ligature. Separate these two branches, and bring them to the point B, one passing through F and H, and the other through D and E. Carry these two branches through the loop (colored for the sake of distinctness) at B, in *opposite* directions, and tie a single knot within the loop; then make a square knot outside the loop. If thought desirable, the two ends of the ligature can be carried again around the outside of the mass, and tied at A.

"The supposed advantages of this mode of using a ligature

can be briefly stated. By using a small needle in introducing a large thread, the punctures made in the tissues are completely closed, and all hemorrhage is avoided.

"It will be found that the action of the loop at B upon the single knot made within it will maintain any degree of strangulation for a short time. And if it appears upon making everything tense after tying this single knot that the ligature is not properly adjusted, it is very easy to loosen it, and readjust before committing one's self to the final knot outside the loop.

"With a single ligature, the tension must everywhere be equal.

"If a mass be tied, each half by an independent ligature, a certain portion of its substance is beyond the influence of any strangulating force exercised by either of these two ligatures, for the reason that they are dragging one against the other, in opposite directions; each one has a different centre of action, and the two may vary in tension.

"The single ligature here proposed exerts a constricting force, which seems to act towards a common centre for the whole mass to which it is applied, and by approximating the external portions of the ligature to the form of a circle, secures for a given length of thread the maximum quantity of included tissue.

"This point was well illustrated in the case of a rather large naevus, with one diameter much longer than the other, upon a child's forehead. The mass came off with the ligature adhering *in situ*. The disposition of the thread marked what looked like a perfect circle, with a double line for its diameter.

"By a slight modification of the foregoing plan, one ligature may be substituted for three separate ones."

DR. PEASLEE said that, in cases of naevi on the face or exposed portions of the body where it is desirable to leave the least possible scar, this ligature would be preferable to most others; this seemed to him the only particular advantage which it possesses.

#### CASES OF IMPERFORATE ANUS—COLOTOMY.

DR. ALFRED C. POST, who was present as a guest, related the case of a male child,  $3\frac{1}{2}$  days old, which was born without an anus. The attending physician had made an incision into the perineum and sought for the blind pouch of intestine in vain. The discharge of urine was free, slightly tinged by meconium, the abdomen was much distended. Lumbar colotomy was performed, and a large amount of meconium discharged. The child is now, 10 days later, doing well, nurses readily, and

appears likely to recover. The urine is still turbid, but contains no meconium.

Dr. Bunn asked for the rate of success of lumbar colotomy for congenital atresia ani.

Dr. Post said that he had met with only one other case, which terminated fatally solely on account of the poor hygienic circumstances of the patient. Inguinal colotomy is generally preferred in children, but he prefers the lumbar operation.

Dr. POOLEY said that, so far as he knew, no person upon whom inguinal colotomy for congenital deficiency had been performed had reached the age of 30 years. Of Amussat's cases only two reached adult age. One of the two cases operated upon by himself lived nearly five years, and died of rupture of the already diseased and brittle colon after the reduction of taxis of the prolapsed intestine through the artificial anus.<sup>1</sup> He would ask Dr. Post whether there is any difference between lumbar colotomy in the child and the adult.

Dr. Post said only as regards the difference in size of the parts. Two years ago he made a perineal anus, found the pouch of intestine 2" up, drew it down and united it to the integuments; the operation was perfectly successful, and the child is alive now. The slight cicatricial retraction which followed was overcome by soap suppositories.

Dr. JACOBI had operated once in the inguinal region; the child died of peritonitis on the fifth day.

#### CASE OF BURSA HYPERXYPHOIDES.

Dr. JACOBI mentioned a case of bursa mucosa of the size of a hazelnut over the ensiform process, which he had lately seen in a rachitic child four years of age. This affection is rare. Hyrtl some 13 years ago published a case in a Vienna paper, and called it bursa hyperxyphoides; besides this one Dr. Jacobi has been able to find only two other cases described by Haymann in literature. The friction of the skin over the projecting ensiform process in rachitic children seems to be the cause of this bursa, which is situated at about the middle of the cartilage.

<sup>1</sup> See this JOURNAL, Vol. VII., No. 1, p. 129.

## TRANSACTIONS OF THE PHILADELPHIA OBSTETRICAL SOCIETY.

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REPORTED BY J. V. INGHAM, M.D., SECRETARY.

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STATED MEETINGS, OF JUNE 4TH, AND JULY 2D, 1871. DR. ALBERT H. SMITH, PRESIDENT, IN THE CHAIR.

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### CYST OF BROAD LIGAMENT.

DR. J. V. INGHAM presented a specimen of cystic degeneration of the organ of Rosenmüller. The cyst was connected with the broad ligament of the left side by a fibrous band. The cyst, which was as large as an orange, was situated at the distal extremity of the ovary. There were cysts also in the ovaries.

DR. GOODELL remarked that there was a difference of opinion with reference to the origin of these cysts of the broad ligament. One cause might be the dilatation of the tubules of the parovarium. Another was probably the degeneration of the vesicle of Morgagni. The peculiarity of the cystic fluid is its high refractive power in the transmission of light. He did not think it possible to decide which of these was the true origin of this cyst.

On motion, a Committee was appointed by the President to examine and report on this specimen.

### REPORT OF THE COMMITTEE.

The Committee to whom was referred the specimen of cystic tumor, presented by Dr. Ingham, report that they have examined the specimen, and find that the tumor was situated between the layers of the broad ligament below the fallopian tube, and extended from its fimbriated extremity to the ovary. The contents of the cysts were very limpid; but a minute examination could not be made, because of changes caused by the fluids in which the specimen had been placed for preservation. In the broad ligament of the opposite side the organ of Rosenmüller could be traced in a corresponding position, but no other evidence of its existence than this cyst was found on the side occupied by the tumor.

The Committee are of the opinion that the position of the cyst, its contents, and the apparent absence of a normal paro-



varium, indicate that the tumor is a cystic degeneration of the parovarium, or organ of Rosenmüller.

H. LENOX HODGE.

WM. GOODELL.

JAMES V. INGHAM.

W. F. JENKS.

DR. GOODELL then exhibited some fluid which he had removed from a lady on the previous day. Thirty-four pints were drawn off. From the very marked fluctuation of the tumor, from the scaphoid condition of the belly after the operation, and especially from the remarkably clear and limpid character of the fluid, he had diagnosticated the case to be one of cyst of the broad ligament. In this opinion Dr. Hodge, who was present at the operation, concurred. In these cysts one tapping usually suffices for a cure; but this was the second tapping to which the lady had been subjected. If the cyst refilled it was his intention to adopt Dr. Atlee's plan of snipping off a piece of the cyst-wall, and of making the peritoneum absorb the fluid. Its bland character precludes the hazard of peritonitis.

#### CRANIOTOMY IN A CONTRACTED PELVIS.

DR. PARISH then read an account of a case of craniotomy. (See AMERICAN JOURNAL OF OBSTETRICS, November, 1874, p. 494.)

In the discussion that ensued, Dr. J. S. Parry remarked that the history of this case confirmed his opinion as to the advisability of Cæsarean section in this patient. The cause of death here was that form of puerperal fever now prevailing in this city, characterized by the presence of diphtheritic deposits on the vulva, vagina, etc.

DR. GOODELL, after complimenting Dr. Parish upon the pluck he showed in this his first case of craniotomy, remarked that he could not agree with Dr. Parry. Cæsarean section should never be resorted to whenever there was a chance of a delivery *per vias naturales*. Statistics are most untrustworthy. The fatality attending the operations of craniotomy and cephalotripsy is in a great measure due, not to the operation itself, but to the delay on the part of the medical attendants. For these operations are rarely undertaken until the child is dead and the mother in an exhausted condition. This false philanthropy is the cause of death. Even in the present case, had the forceps not been used, and had the head been at once opened and broken up, the woman might at least have escaped the vesico-vaginal fistula.

DR. PARISH replied that the fistula could not be attributed

to the use of the forceps, for no long-continued traction was made with that instrument, since it was employed simply as a means of determining the amount of contraction. Nor could it be attributed to bone-spiculæ, for he took good care to preserve the scalp. He thought it was due to the traction made by the embryulcia forceps.

DR. PARRY thought that the subsequent inflammatory trouble could not have caused the fistula, for too short a time had elapsed. But it could have been caused by the rapidly erosive action of a diphtheritic sore. He called attention to the fact that in puerperal inflammations suppuration was most commonly on the left side. This had been attributed to the left lateral position usually assumed by the parturient woman.

THE PRESIDENT suggested bone-spicula as the probable cause of the fistula. In this case the necessity for rapid delivery was urgent. But in most cases perfect safety is secured to the maternal passages by a slow delivery. The head would then become wire-drawn or mould itself to the inequalities of the bony canal.

DR. LUDLOW recalled some experiments that he had made with the late Prof. Hodge, in which fetal heads were completely crushed by repeated applications of the cephalotribe, and yet no bone-spiculæ pierced through the scalp. He was therefore inclined to look with much favor on that instrument in desperate cases of pelvic deformity.

STATED MEETING, AUGUST 6TH, 1874. DR. A. H. SMITH, PRESIDENT,  
IN THE CHAIR.

#### CHOREA IN PREGNANCY.

DR. TAYLOR stated that he had a case of chorea in pregnancy about which he felt very anxious. He therefore asked for information concerning the prognosis and the treatment of the disease.

DR. GOODELL replied that this complication in pregnancy is a very fatal one. He knew of no specific, but would be inclined in a desperate case to induce labor as early as is compatible with the safety of the child. He had seen but one case of chorea in pregnancy, and never wished to see another. Its history he had reported to this Society (see AMERICAN JOURNAL OF OBSTETRICS, May, 1870, p. 140).

DR. C. R. PRALL then read the following history of a successful case in point:

"On the 21st day of April, 1873, I was called to see Mrs. S——s, 23 years old, in her first pregnancy, twenty-five days be-

fore confinement. She had had chorea from the commencement of her pregnancy; at first mild, but gradually increasing until it became so exceedingly severe that when I first saw her I had very little hope of her recovery. Almost every muscle seemed to be in a state of extreme agitation. Her upper and lower extremities were so completely beyond her control, that to keep them from being injured against surrounding objects it was necessary to have them constantly restrained by two assistants; and then, as she was sitting on a sofa her body was so violently moved about that her head was several times brought forcibly against the wall before I could have her removed to a place of safety. She was then carried to an adjoining room and placed in the middle of a large bed, and in my examination I discovered that her movements had been so constant and long continued that the skin was worn off the elbows and knees and that the clothing was stained with blood. Her pulse was accelerated, but otherwise natural; tongue coated; bowels constipated, and appetite not much impaired. I ordered a bottle of citrate of magnesia at once, and bromide of potassium grs. xx. + elix. valerianate of ammonia f 3 j. every two hours.

"On the morning of the 22d, found her without any improvement. The bowels had been moved. Ordered the bromide and valerian to be taken four times a day, before meals and at bedtime, and zinci. sulph. grs. ij. three times a day after meals, which was increased to grs. iv. on the 23d, and to grs. vj. on the 24th, and to grs. viij. on the 25th, and to grs. ix. on the 26th, and to grs. xij. on the 27th, and to grs. xv. on the 28th, which last increase was rejected by the stomach on taking the second dose. From that time the symptoms of chorea declined so rapidly that in twenty-four hours she could use her hands to feed herself, and even to sew, and in a few days she went about as well as any other person. On May 16th she had a very easy, natural labor, and a speedy recovery.

"I did not hear from her chorea again till May 25, 1874, in another pregnancy, 115 days before confinement. This time her symptoms were not so severe as they were when I treated her before, which can be accounted for by my being called so much earlier in the pregnancy, as her friends informed me that she was going on with a gradual increase precisely as she did the first time. I gave her the zinc treatment again, with the same good result. It was also necessary this time to increase the dose to grs. xv. three times a day before the disease began to decline; and then, as before, the stomach rejected the medicine on the second full dose. After which, on both occasions, the dose was gradually reduced to grs. ij., and then stopped. Large doses of the zinc could not be retained in any other form

than in pill, nor at any other time than after a full meal. And in the last attack, by commencing with the zinc alone, I found that the stomach would not bear the increased dose without a dose of the bromide half an hour before eating.

"This patient had chorea at the age of thirteen years, and her catamenia made its first appearance shortly after the chorea commenced.

"The disease then continued about two years. In that time she had a great many doctors, but does not remember their names, nor has any idea of what cured her. She never had rheumatism.

"My attention was directed to the treatment adopted in this case by reading the cases communicated by Mr. H. T. Butlin, in the January and February numbers of the *London Lancet* of 1872.

"I believe the bromide of potassium did little or nothing in this case, aside from its helping the stomach to retain the zinc; for I know she had the bromide well tried in the early stage of her first pregnancy, and it failed."

STATED MEETING, SEPTEMBER 3. DR. A. H. SMITH IN THE CHAIR.

#### UTERINE POLYPUS.

DR. HORACE WILLIAMS related the history of a case of uterine polypus, which presented some interesting clinical features.

A young lady was brought to him, complaining of acute suppression of the menses, she having missed two monthly periods. Her menstruation previously had been normal.

The two periods passed over without any other indication than simple suppression, but on the week following the time on which the second was due, she complained of backache and slight leucorrhœa. Soon after this she became unwell, with a free flow and violent tenesmic pains.

There was great prostration, with abdominal pain. When he saw her again she was weak, and the discharge was brownish in color and somewhat offensive. An examination being refused, he ordered an antiseptic wash. Two weeks passed by, during which she had hectic fever and symptoms of blood poisoning. An examination was now permitted, and he found resting in the vagina a polypus, part of which was in a sloughing condition. It bled freely to the touch.

After removing it with an *écraseur* the patient did well. The point of interest in the case was the two months' suppression of menses, like that of pregnancy, followed by symptoms resembling an abortion of two months, also followed by symptoms like those caused by a retention of a part of the ovum.

The suppression of the menses in this case, Dr. Williams thought, was due to the ball-valve action of the polypus previous to its expulsion from the uterus. It was attached by a single pedicle to the inner os.

DR. CHARLES H. THOMAS recalled the case of a lady who had had several miscarriages with an excessive flow each time. The ovium always came away complete, but the discharge never ceased until several days afterwards, when, according to the patient's account, a fibrinous clot came away. Four days after the last miscarriage Dr. Thomas detected a polypus attached by a slight pedicle to the posterior wall of the uterus near the fundus and removed it with the forceps. The patient told him that after each miscarriage, several similar masses came away from her.

DR. H. L. HODGE did not agree with Dr. Williams in his explanation of the suppression of the menses in his case by a mechanical cause.

In the cases of retention of menses by an obstruction which he had seen, there were always labor-like pains caused by the efforts of the uterus to expel the accumulated discharges. He related a case in which he was called upon to operate for a supposed polypus of the uterus, but upon examination he found that the patient was pregnant, no polypus existing.

DR. WILLIAMS also gave an account of a case in which a polypus coexisted with pregnancy. At about the third month of gestation he was called to see the patient on account of symptoms of miscarriage. On examination he found a small polypus attached to the cervical canal. At that time he had brought the question of operation before the Society, and its opinion was unfavorable to the removal of the polypus.

He then made a strong application of tamin to the polypus, so that it became yellow in color, and after this there was no further bleeding.

In a few days the patient noticed a peculiar discharge. Two months latter there was no trace of the polypus.

DR. W. H. PARISH gave a detailed account of a case of menorrhagia in an unmarried lady of 35 years of age. The patient had profuse continuous hemorrhage lasting five weeks, which was not controlled by the usual remedies, including the use of the tampon. A weak mixture of Monsel's solution and water had little or no effect, but by washing the cavity of the uterus with a mixture of equal parts of the Monsel's solution and water, the bleeding was arrested. The point of interest to him was the strength of the solution used, and without bad effect.

DR. PRALL said that he was in the habit of using the half strength Monsel's solution, and he had recently used it in its



full strength without any bad symptoms resulting. He had never seen any slough from the use of the pure Monsel's solution. The moment the liquid touched the bleeding surface a coagulum was formed, which prevented the absorption of the liquid. He had never seen any clot form in the veins.

DR. HODGE had seen bad effects from the use of the Monsel's salt, both in a dry state and in solution. In the case of a young girl with menorrhagia, the injection of a dilute solution into the vagina was followed by inflammation of the vagina and abscess of the labia majora.

DR. McCALL expressed similar views to Dr. Hodge.

STATED MEETING OF NOVEMBER 5TH, 1874. DR. ALBERT H. SMITH,  
PRESIDENT, IN THE CHAIR.

CASE OF HYDATIDIFORM DEGENERATION OF THE CHORION.

DR. R. A. CLEEMANN exhibited the specimen and read the following history of a hydatidiform degeneration of the chorion:

"This small portion of a large vesicular mole was removed from the cavity of the uterus of a patient under my care. The specimen has lost, in some measure, the very characteristic appearance it at first presented, owing to the mode of its transmission to my office—it being simply wrapped in a bit of newspaper—and to the delay of nearly twenty-four hours, which occurred before it was placed in the preservative solution of chloral hydrate. This appearance is generally described as racemose, or like that of a bunch of white currants, or it may be more fitly represented by rows of colorless oval beads of varying sizes, closely strung and tangled together.

"Instances in which the chorion has undergone the hydatidiform degeneration are not very infrequently met with, yet they are rare enough, and have sufficient clinical interest to justify my giving to the Society a brief record of the case from which this specimen was obtained.

"I was called in haste to visit the patient on the night of October 11th, 1874. She proved to be a young girl, of small and delicate frame, presenting an almost bloodless and anxious countenance. Her pulse was running 120–125 beats to the minute, while she was complaining of acute and nearly constant pain in the abdomen: her lower garments were wet and stained with blood. It was stated that she was only sixteen and a half years old, but had been married twenty-one months. Eleven months after marriage she had been delivered, after a tedious labor, of a male child, of large size, but probably feeble, since it lived only three weeks. Subsequently, her health had been good and the catamenia regular for the six successive periods,

after which the flow had ceased, leading her to believe that she was now in the fourth month of a second pregnancy. Two months after the arrest of the menses, the patient noticed that her abdomen was enlarging, but had no other evidences of her being with child. Nothing unusual, save a constant drowsiness, presented itself, till two weeks before my visit, when profuse metrorrhagia suddenly came on at night, without pain, while she was lying quietly in bed. The 'flooding' had continued ever since, in considerable quantity, without her recognizing any watery discharge, but during the last three hours intense abdominal suffering had been superadded.

"Placing my hand on the abdomen, I discovered it to be hard and exceedingly sensitive, and enlarged to the size it attains at the seventh month of utero-gestation. A vaginal examination revealed the lower segment of the uterus expanded, hard, but yielding an obscure sense of fluctuation, and the cervix undistinguishable; the os dilated to the diameter of an inch, and occupied by a ragged-feeling mass which suggested a degenerated placenta. The examining fingers were slightly stained with blood. Dilatation of the os progressed rapidly, and when a diameter of  $2\frac{1}{2}$  inches was reached, a large quantity of cystically degenerated chorion, more in bulk than a full-sized placenta, came away; smaller masses with blood clots, old and more recent, followed this, with the assistance of the fingers, the introduction of which within the os gave rise to great pain. The uterus, still so sensitive as to make firm pressure unsupportable, seemed now to be about the size of a small foetal head, and felt hard, while the os had become firmer, and presented nothing within its grasp; a very small amount of watery blood escaped from the vagina, and the pain had ceased. Under these circumstances a binder was applied, and further manual interference discontinued; a small tablespoonful of wine of ergot, with thirty drops of landanum, being given to the patient. She soon slept, but would wake at intervals with slight pain, while the same discharge came slowly away. During the two subsequent hours I remained by her, fearing an access of dangerous hemorrhage, and during this time the dose of ergot was repeated twice, and once more on my leaving, on this occasion with an additional dose of landanum. The next morning there was some febrile reaction, the pulse being still at about 120. The ergot had been given at intervals of two hours with occasional doses of the anodyne and stimulants. In the following days she took iron and good nourishment, and with the exception of severe cephalalgia did well, making a rapid recovery. This headache was obstinate, resisting several remedies, but yielding finally, apparently to large doses of quinia. The

uterus diminished gradually in size, always remaining tender, and the watery discharge, containing at first small clots—which were not examined, but were probably fragments of the decidual membranes—had ceased in six days, when it returned for one day only purulent and offensive. During the second week she left her bed, and by the third was attending to some household duties, declaring herself quite well.

“The nature of the hydatidiform masses, such as the one before us, in so far as they are the result of an enlargement and distention of the young villi of the chorion, is now well established; but the influences which lead to the striking alteration are still obscure. Dr. Graily Hewitt<sup>1</sup> supposes the degeneration due to the cessation of life in the foetus, while its membranes still maintain their nutritive hold upon the walls of the uterus. Under these circumstances the assimilative materials yet find their way to the ovum, but having lost their normal end in the development of the embryo, are expended in this unnatural growth. Following this theory, which probably explains correctly the phenomena in a certain number of instances, may we not find the remote cause of the abnormality, in this special case, to be the early marriage of the patient, whose immaturity for child-bearing did not permit the full development of two children in such closely recurring pregnancies?<sup>2</sup>

“The most important diagnostic symptoms observed in the case were the early and rapid enlargement of the abdomen, and the sensation communicated by the presenting object which I have described as ragged: these served to exclude “*placenta prævia*” for which vesicular mole has been mistaken.

“There is so much satisfaction in cases of abortion with much hemorrhage, in sweeping with the finger the uterine cavity till it is free, that the temptation was strong to employ the same manœuvre here; but it was rejected on account of the great sensibility of the womb, combined with the exhaustion of the patient. The administration of ether seemed to be undesirable from the subsequent risk of secondary hemorrhage. In the more tedious method of expectancy and ergot-giving, which was adopted, the tampon was omitted on account of the size to which the uterus had attained.”

DR. CHESTON MORRIS asked whether Dr. Cleemann had noticed any remains of the foetus.

<sup>1</sup> Trans. Obst. Soc. of London, vol. i., p. 258.

<sup>2</sup> Dr. J. Matthews Duncan quotes Aristotle in support of the opinion that very young mothers bring forth puny children, and in confirmation of so old a view adduces a table from the Journal of the Statistical Society of London, showing a greater mortality among the offsprings of such mothers as compared with that of those somewhat older (Fecundity, Fertility, Sterility, etc., pp. 236-287).

DR. CLEEMANN replied that he had not.

DR. MORRIS then referred to a case in which a cystic mass had been expelled at about the fifth or sixth month of pregnancy, the foetus being felt within the walls of the cyst.

DR. CLEEMANN remarked that instances had occurred of children born alive in conjunction with this form of degeneration of the placenta.

THE PRESIDENT remarked that in the majority of cases no foetus has been found, and this fact is important in view of the question as to whether the hydatid mole is an evidence of pregnancy or not. Some years ago Dr. Moore Madden doubted the fact that this mole is an evidence of pregnancy. We can as easily account for the existence of a degenerated chorion as for the presence of a dermoid cyst without the existence of pregnancy. This may be a question bearing upon the purity and chastity of the woman.

DR. CLEEMANN said he believed that the weight of testimony was against the view presented by Dr. Smith. As to the dermoid cyst, it could not fairly be brought into analogy with the vesicular mole, for though the cyst contained various appendages of the skin and mucous membrane, such as hair and teeth, these bore no determinate relation to each other or to the parts with which they are normally connected; while the altered chorion was found in its natural situation in contact with other foetal membranes, without other eccentric development. The logical inference was, therefore, that in the latter case, the usual sequences had occurred up to the formation of the chorion, which would include natural conception.

DR. H. LENOX HODGE then read a paper of the late Prof. Hugh L. Hodge upon the forceps and cephalotribe.<sup>1</sup>

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## QUARTERLY REPORT ON OBSTETRICS AND DISEASES OF WOMEN AND CHILDREN.

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REPORT ON THE PROGRESS OF OBSTETRICS FOR THE YEAR 1874. BY  
DR. M. D. MANN.<sup>2</sup>

THE first subject to which I ask your attention is a new method of *pelvimetry* proposed by Dr. J. R. Hardie.<sup>3</sup> His directions are as follows: First empty the bowels and bladder, then press down, at a point about one inch

<sup>1</sup> See this No. of JOURNAL.

<sup>2</sup> Read before the N. Y. Medical Library and Journal Association, March 26th. The abstracts of articles which have been published in this Journal are mostly omitted, references only being given.—ED.

<sup>3</sup> Obst. Jl. of G. B. and Ire., No. 16, p. 234.



below the umbilicus, with the hand, until the promontory of the sacrum is reached. When this is made out, conduct to it one end of a measuring tape and keep it in place with the thumb of the left hand, and carry the tape to the upper and thin border of the symphysis pubis. Add to the measurement thus obtained the thickness of the abdominal walls, say one-fourth of an inch, or more, if the individual is very fat. This method is only applicable to non-pregnant women, and certainly possesses some advantages.

The subject of *uterine thermometry*<sup>1</sup> has lately attracted considerable attention.

Schroeder has derived the following conclusions from his observations: If it be found that the temperature of the pregnant uterus be higher than that of the vagina, and if such excess is derived from the warmth produced by the fetus, and if with the death of the fetus the source of caloric is dried up and the uterus is also forced to supply heat to the dead mass, then we may infer that the death of the fetus has taken place when we find that there is no difference, or only a very slight one, between the temperatures of the uterus and vagina. More lately this proposition has been expanded by employing the thermometer as a means of diagnosing pregnancy in the early months, when aid is so much required. To derive positive results it is necessary to know if there be any normal difference between the uterus and vagina in the non-pregnant condition. Schlesinger gives as the result of his investigations on this point that the uterine cavity, both pregnant and non-pregnant, possesses a higher temperature than the vagina, but the gravid uterus is of a higher temperature than the non-gravid, and the parturient uterus has a higher temperature than the non-parturient.

Many plans have been proposed for *determining the sex of the fetus in utero*. That by counting the fetal pulse has seemed to offer the most probable success. Drs. Steele and Strong<sup>2</sup> give as the result of 100 observations that—

1. In the majority of cases the male foetal heart beats slower than the female.

2. 132 foetal pulsations per minute is the average which constitutes a dividing line between the sexes. Below this 68 per cent. are males, 20 per cent. females, 12 per cent. doubtful. In conclusion they say: "It may be generally stated that we find an opinion as to the sex of the child in utero, founded on the rate of the foetal pulse, to be of little more value than a guess."

As a cure for that most intractable of complaints, the *nausea and vomiting of pregnancy*, Drs. Morse and Simmons have both employed rectal injections of chloral hydrate.

Dr. Morse<sup>3</sup> reports one case in which everything was tried without effect until 40 gr. of chloral night and morning, continued for 7 days, effected a cure. Dr. Simmons<sup>4</sup> reports 4 cases. He used 30 gr. doses, by the rectum, in mucilage. He adds: "If I meet with other cases I think I will use larger doses at once to produce a decided impression, as thus it may require fewer doses to put an end to the trouble." Several cases are also reported where the K. Br., given by the rectum, produced excellent results. Dr. Playfair<sup>5</sup> reports a case of *retroversion of the gravid uterus*.

<sup>1</sup> Schlesinger, Allg. Zeit., March 10, 1874.

<sup>2</sup> Med. Examiner, Aug. 15, 1874.

<sup>3</sup> W. H. Morse, N. Y. Med. Rec., Oct. 15, 1874.

<sup>4</sup> D. B. Simmons, Japan, N. Y. Med. Rec., June 1, 1874.

<sup>5</sup> Playfair, Obst. Jour. Gt. Br. and Ire., No. 15, p. 154.



He introduced an air-pessary, distended it, and thus by gradual pressure returned the displaced organ. A Hodge was then introduced, and all went well. Dr. Gervis<sup>1</sup> reports 3 similar cases, 2 of which were fatal. In one complete retention of urine had existed 3 or 4 days. Abortion occurred a few hours after reposition, and death on the second day. In the second case retention had existed much longer, and death occurred on the day following reposition. In the third case retroversion, with more or less retention, had existed three weeks. The uterus was replaced, and the patient did well.

In the discussion which followed in the London Obstetrical Society, many similar cases were reported, and the importance of appreciating the accident, and taking proper means for its relief, was dwelt upon.

Dr. Michel Peter,<sup>2</sup> of Paris, calls our attention to the *accidents* which may happen to *pregnant women suffering from cardiac disease*. In seven of the eight cases observed the lesion was mitral insufficiency, in one aortic stenosis. He holds that such women are subject to accidents which may affect the lungs, impede gestation, and aggravate the state of the heart. Pulmonary accidents and miscarriage happen especially in the fifth month. The production of the lung troubles is due to the augmentation in the total mass of the blood, which increases with the wants of the fœtus. The disease of the heart involves a congestion of the lungs, hence pregnant women with cardiac disease are doubly exposed to pulmonary accidents. The practical consequences of these facts are that women with heart disease should not be mothers. If one, however, becomes pregnant, the physician must attentively observe the respiratory functions and intervene energetically as soon as pulmonary disorder begins.

Reciprocally, when a woman suffers towards the middle of her pregnancy with severe pulmonary disorders, or when she miscarries by them, the physician must examine the heart, and will thus perhaps discover a disease till then unknown or misunderstood.

Dr. L. D. Bulkley has reported some cases of a rare skin disease affecting the pregnant women, which he calls *herpes gestationis*. He has found eight similar cases recorded. He sums up the features of this remarkable disease as follows, in brief :

See AM. JOUR. OBST., vol. vi., p. 594.

At a meeting<sup>3</sup> of the New York Obstetrical Society thirty-four cases of *artificial induction of labor* were reported. All the mothers but one did well, and a large majority of the children were born alive. Dr. Noeggerath said that the results were more favorable in this country than in Europe. He had no doubt that the results were better since the introduction of Barnes' dilators (Dr. N. reported seventeen of the thirty-four cases), and to the instrument could be ascribed in part the favorable results. Dr. Swayne,<sup>4</sup> after relating the history of twenty cases of induction of premature labor, remarks: "On the whole, as regards the maternal mortality, the induction of premature labor has not, in my hands, shown a more favorable result than craniotomy, which has proved fatal in only two of my thirty-four cases. Although the saving of infant life affected by this operation is an advantage far outweighing any of the disadvantages just mentioned, yet the accoucheur should, I think, always bear in mind that it is a proceeding attended with some risk, and involving a great amount of responsibility, such an amount, indeed, that it ought always to be shared with another practitioner.

<sup>1</sup> Dr. Gervis, Obst. Jl. G. B. and Ire., vol. ii., p. 579.

<sup>2</sup> M. Peter, Paris. Obst. Jour. G. B. and Ire., vol. ii., p. 426.

<sup>3</sup> Am. Jl. Obst., May, 1874.

<sup>4</sup> Brit. Med. Jl., Aug. 8, 1874.

Kühne<sup>1</sup> has brought on *premature labor* in fifteen cases. His method is to introduce a male catheter, and then inject water. The procedure is repeated if necessary. This is the so-called Cohen's method. Of his cases all the mothers lived, and twelve of the children.

Berthold reports ten cases; the mothers all lived, and five of the children are still in good health. His method is to introduce a flexible catheter between the membranes and leave it until contraction begins—Krause's method.

Dohrn has had much better results than in the case given by Spiegelberg, who tried to prove that labor at term gave better results than premature labor in contracted pelves.

Dohrn reduced the mortality to seven per cent. for the mothers, and forty-four per cent. for the children, while Spiegelberg had eighteen per cent. of deaths among the mothers, and sixty-six per cent. among the children.

Since the time of Simpson, chloroform has been coming more and more into use as an *anæsthetic during labor*.

Dr. Campbell<sup>2</sup> has employed what he calls "obstetrical anæsthesia" in 942 cases without having to regret the smallest accident.

Dr. Depaul says "that the accidents from obstetrical administration of chloroform are not unknown; he is in possession of cases in which sudden death has been produced by it. He believes it requires great care in its administration, and in ordinary labors can be dispensed with." It has been suggested, that Dr. Depaul would do a great service in publishing the details of the fatal cases.

In the New York Obstetrical Society (1874) Dr. Lusk has reported two cases which came near being fatal from the use of chloroform during labor.

Dr. Playfair<sup>3</sup> objects to its use on other grounds, and proposes *chloral as a substitute*. He says: "The more I use chloroform, in labor, the less I like it, not because it does too little, but because it does too much. In many cases, not all, it unquestionably diminishes the force and frequency of the contraction; it also undoubtedly predisposes to post-partum hemorrhage. The use of chloral as a means of lessening the pain of labor is, I think, by no means as yet fully appreciated. It has the immense advantage over chloroform, that it does not diminish the force of the pains, while it does their painfulness. It can also be given sooner than we would give chloroform, viz., toward the end of the first stage. When judiciously given, the patient falls into a drowsy state, nearly asleep; she is roused by the pains, but suffers comparatively little. I have given it for the last two years, and have seen no bad results, nor have I any grounds for thinking that it diminishes either the force or frequency of the contractions. When the first stage is near completion, I give fifteen grains. This is repeated in twenty minutes, and this generally suffices. It will seldom be necessary to give a 5 dose, and never more than 5 i. The effect can be kept up for many hours."

Mr. Berry<sup>4</sup> reports a case, where he gave ninety grains of chloral, and both mother and child did well. During the whole time she slept soundly between the pains, and there appeared to be great relief to her suffering. There was nothing to lead one to suppose that the chloral had retarded the progress of the case; on the contrary, the character and frequency of the pains was all that could be desired.

In case chloroform is used, and death is threatened as a result, we have

<sup>1</sup> Arch. f. Gyn., B. vi. H. ii.

<sup>2</sup> Dr. Campbell, Paris. Medical Times and Gazette, July, 1874.

<sup>3</sup> W. S. Playfair, Lancet, February 21, 1874, p. 263.

<sup>4</sup> Lancet, April 11, 1874, p. 532.

a new method brought to our notice for *resuscitating* our patient.' Dr. Sims<sup>1</sup> relates a case which illustrates Dr. Nélaton's method of resuscitation after apparently fatal narcosis. The method consists simply in inverting the body so that the anæmic brain will be again supplied with blood, artificial respiration being kept up continuously. In this case the patient was three times inverted, and on both the first and second attempts to restore her to the horizontal position, the pulse and respiration stopped. The third time she was held inverted until consciousness was quite restored, after which there was no more trouble. Other similar cases have been reported.

Prof. Braun,<sup>2</sup> of Vienna, in a lecture on the *course of a natural labor*, says that Wm. Braune, of Leipzig, has illustrated the position of the uterus and foetus at the end of pregnancy by sections of the frozen cadaver. The sections are of great interest, and establish what he (Prof. Braun) has taught for years as to the relations of the cervix during pregnancy and labor. In one case—a suicide in the ninth month of pregnancy—the uterus was folded over the symphysis pubis, forming a kind of sac with the anterior wall, a condition which showed it to be in the flaccid state. The vaginal portion was deep, the cervical canal narrow, and  $4\frac{1}{2}$  cm. long; the internal os was so narrow that only a stiff probe could be forced through it. The external os was on a level with the inferior border of the symphysis.

In the other case the woman died during labor, and the bag of water was found the size of an apple in the vagina. Death took place at the beginning of the expulsive stage. The uterus held the child tightly clasped; and the absence of folds indicated a contracted condition of the uterus. The inner os was somewhat above the entrance to the pelvis, and was marked by a large vein. On the uterus being emptied, the os internum was found 3.5 cm. above the symphysis, and 2 cm. above the promontorium. It resembled a half ring, separating distinctly the widely distended cervix from the uterine body. The completely distended os externum was only recognizable as a little eminence on the vaginal wall. The length of the uterine cavity from the os interior to the fundus was 16.5 cm.; the distance from the os exterior to the os interior in the axes of the pelvis was 11 cm. Dr. I. E. Taylor,<sup>3</sup> on the same subject says:

See AM. JOUR. OBST., May, 1874.

In the lecture already referred to, Prof. Braun gives the *temperature throughout a normal labor* as varying between  $36.6^{\circ}$  and  $38^{\circ}$  C, average  $37.44^{\circ}$  C. That is about  $.2^{\circ}$  to  $.3^{\circ}$  C. higher than in other people, about  $.1^{\circ}$  to  $.2^{\circ}$  higher than during pregnancy.

The temperature in the second stage is a little higher than in the first stage, and immediately after the birth, is a little higher than during the labor. The pulse is very variable; the respiration in general follows the temperature. The respiration of the after-birth is followed by a loss of blood, generally not exceeding 2 to 3 oz. After the uterus has contracted, the os internum presents an oval ring with hard edges, and about 2 fingers in width. The cervix is 9 in. long, and so wide and soft as scarcely to be distinguished from the vagina. These views, as you will see, as to the condition of the cervix after labor, are totally at variance with those held by Taylor.

Dr. Dickinson<sup>4</sup> recommends as a ligature for tying the fœtal cord a "flat

<sup>1</sup> Sims. Brit. Med. Association Trans., 1874.

<sup>2</sup> Wien Med. Woch., No. 23, 1874.

<sup>3</sup> Am. Jour. of Obs., May, 1874.

<sup>4</sup> Obst. JI. of G. B. and Ire., p. 41, No. 13.

silk elastic," which follows down the shrinking cord, and thus prevents any possibility of hemorrhage.

Our ideas in regard to the proper treatment of the puerperal woman have undergone very radical changes during the last half century, but none so radical as those proposed and practised by Dr. Goodell,<sup>1</sup> of Philadelphia. He holds that from the first day the diet should be generous. The canonical purge on the third day should be dispensed with. The milk-fever, for the relief or prevention of which it is given, is only a myth, and has no real existence. Premature exertion must not be allowed, but a recumbent position should not be too rigorously enforced. "I feel persuaded," he says, "that this tradition of the lying-in chamber does more harm than good, for nothing so relaxes muscular fibre as confinement in bed. Since labor is a physiological process, there can be no sound reason why a woman should not sit up in bed, or even slip into a chair, whenever she feels so disposed. Such movements excite the womb to contraction, and empty it and the vagina of putrid lochia. When the lochia are offensive, these upright positions should be insisted upon, as being in fact better deodorants than any detergent vaginal injections. They also tend to lessen those conditions which are a common cause, in my opinion, of posterior displacements. The prolonged use of the obstetric binder is another factor in the production of these complications. It may be useful in the first forty-eight hours, but when kept on simply to preserve the shape, it only defeats this object by paralyzing the abdominal muscles and lessening the retentive power of the abdomen. It also greatly impairs the process of involution." These are the conclusions of a long and well-sifted experience. As regards the milk-fever mentioned by Dr. Goodell, the observations of Dr. Linn<sup>2</sup> are confirmatory. He found in a certain number of *normal puerperal women* that the *temperature* was only 6° higher than normal; the pulse the same as in ordinary conditions; the respirations a little more frequent. He holds, then, that there is no such thing as milk-fever. Dr. Barker<sup>3</sup> also holds similar views. Dr. Linn says further, that a temperature of 100°, or a pulse of 100, is an indication of some pathological process which should be discovered at once, that its development may be properly arrested and the evil removed.

An explanation of the *rigors* which so frequently attack women very soon after delivery has always been difficult. Pfannkuch<sup>4</sup> rejects all the explanations thus far offered, and adds one of his own. Experiment shows that the temperature of the fœtus in utero is at least 9° higher than that of the mother. It follows from this that every pregnant woman has a second centre of warmth in the uterus, but her temperature is not thereby increased; she must therefore produce less warmth than when not pregnant. When the child is born this heat-producing centre is removed, and there is a disproportion between the heat produced and that given off. The effort to restore the equilibrium is what causes the rigor. The intestines are very susceptible to heat and cold, and it is from their neighborhood that the heat-centre is removed. Moreover, the uterus having sunk downwards, the intestines are allowed to touch the abdominal wall, which is also very much thinned, and thus more heat is lost. If the child dies during pregnancy, the mother is often subject to rigors and complains of a feeling of coldness in the abdomen. If, then, Pfannkuch is correct, there should be no rigors after the birth of a dead child; and he reports

<sup>1</sup> Med. and Surgical Reporter, Feb. 21, 1870.

<sup>2</sup> Dr. Linn, Phil. Med. Trans., May, 1874.

<sup>3</sup> Dr. Fordyce Barker, Puerperal Diseases.

<sup>4</sup> Arch. f. Gynäk., B. vi., H. 11.



that in a few cases seen since the adoption of these views the rigors have been absent.

Dr. Fehling,<sup>1</sup> from his observations, considers that an equal temperature in the uterus and vagina may be regarded as quite sure proof of the death of the fœtus. He objects to Pfamkuch's explanation just given, and says it would imply a lowering of the temperature in the interior parts of the body, the fact being that in the febrile chill, as distinguished from one which is purely nervous, there is a rise, and commonly a rapid rise, in the temperature of the internal organs over that of the periphery. He has twice seen a chill, following the birth of a putrid fœtus, not due to septic influences. He agrees with Schroeder's explanation of the rigor, viz., that it is due to a difference between the temperature of the skin and that of the interior of the body. The internal temperature rises in consequence of the activity of the uterus (as we have already seen from Braun), which is now empty, and meets with no resistance on contracting. During the second stage, however, from the greater voluntary efforts taking place at the same time, the lungs and skin are in a state of great functional activity, so the surface of the body is warm and moist. In the repose which follows the third stage the loss of temperature to the skin is considerable, and a chilly feeling is therefore quite natural.

Any new plan of treating *sore nipples* must attract our attention. Dr. E. P. Cutter<sup>2</sup> thinks the crushing in of the nipple by the clothing, causing a deep re-entrant angle at the base where the secretions are retained, and the epidermis softened, is a prolific cause of sore nipples. If the dress be loosened in order to relieve the pressure, the constant friction keeps up the irritation. Now, if we can give the nipple rest and ventilation, it will get well. To accomplish this he fits a cork, with a hole in the centre larger than the nipple. This is worn, and thus the clothes are kept off, and the air allowed access, and the reparative process goes on undisturbed. He has obtained excellent results by this method.

As an incentive to the careful study of the abnormalities of labor and of the puerperal diseases, Dr. Duncan states as his belief that in Great Britain one in every 100 women, delivered at or near full term, die in parturition, or before the puerperal state and its effects have passed over. Dr. Barker gives the proportion for New York as much greater, though an exact estimate is very difficult to form.

Dr. Penrose,<sup>3</sup> in speaking of the delivery of the head in *breach presentation*, says: When the head is transverse, the fingers inserted on the side of the pelvis can easily reach to the superior strait, and can easily be applied to the root of the nose or in the mouth, and the head can thus be quickly and readily conducted, still in the transverse diameter, to the floor of the pelvis. And again: If the head is at the superior strait, and the uterine forces fail, do not think of the forceps, but press directly down on the head with the hands; you can thus apply any amount of *vis a tergo*. In almost all cases of breach presentation the forceps are unnecessary, and the rapid delivery of the head can be easily and quickly secured by the bearing down efforts of the mother, aided or replaced by the pressing down efforts of the attendant.

Dr. Angus McDonald,<sup>4</sup> in a long and very interesting article on *difficult occipito-posterior positions of the head*, concludes as follows: In occipito-posterior positions, if they are persistent, we may safely assume that we

<sup>1</sup> Arch. f. Gynäk., 1874.

<sup>2</sup> Medical Record, April 1, 1874.

<sup>3</sup> Obst. Jl. G. B. and Ire., Am. Sup., p. 187, No. 12.

<sup>4</sup> Edbg. Med. Jl., Oct., 1874.



have some pelvic peculiarity or disproportionately large head to deal with, and as a general rule all attempts at artificial rectification of the position of the head will prove abortive, and are even dangerous if attempted to be effected by means of forceps, levers, etc. The only exception is when temporary delay is occasioned from accidental displacement of a small head, in which case we may wait for normal delivery or aid by the hand.

In cases which threaten to end "face to pubis" and are at the same time decidedly difficult, it is best to pull the head through cautiously, and to abstain from every attempt at rectification of the position. In these cases take great care of the perineum. In cases of obstructed occipito-posterior positions in which rotation takes place at the outlet of the bony pelvis, while the head is in the grasp of the long forceps, there is very great danger in primiparæ of lacerating the soft parts by the instruments. To prevent this accident either, first, the blades should be cautiously removed, the head fixed in position, and the uterus allowed to finish the expulsion of the head, or, second, the curved instruments may be reapplied, adjusted to the altered relation of the parts, or, third, a straight short pair may be applied, and the further advance of the head secured.

*Decollation* has been recommended in certain cases of shoulder presentation as a substitute for evisceration. Dr. F. W. Wright<sup>1</sup> advocates the following method:

Pass a simple blunt hook pierced with an eye carrying a piece of twine or tape, to which are attached half a dozen thin copper wires, over the foetal neck. The presenting foetal arm is meanwhile seized by the right hand of the operator and considerable traction made upon it, until the left fingers are passed over the neck, when the blunt hook is inserted and traction made, so as to enable the operator to withdraw the twine. A leash of copper wire is then attached to this and is drawn over the child's neck, and by a sawing motion the head can be severed in five seconds. The body is then extracted by the presenting arm, and the head is delivered either by seizing the neck, or by forceps, or the crotchet. The advantages of the operation over evisceration are that it is short, quickly done, not dangerous to the mother, and is not so repulsive and bloody. "Perforation and evisceration are not always sufficient to bring relief to the mother. Decollation never fails to bring instant relief and safety to the mother, and is unquestionably simpler, safer, and speedier than evisceration."

Dr. Duncan<sup>2</sup> has experimented to discover the *size of the aperture necessary for the passage of the placenta* and of the accoucheur's hand. A knowledge of this is useful in the treatment of placenta prævia. He finds that an aperture, fully two inches in diameter, is required for the transmission of the uninjured mature placenta; if lacerated, it can pass a smaller opening. The os, supposed to be circular, must be two and a half to three inches in diameter to transmit the hand. He concludes that in the great majority of cases of placenta prævia the hand may be passed into the uterus if the placenta can be extracted from it in a satisfactory manner without disruption.

Dr. Jüdel<sup>3</sup> gives the results obtained in seventy-four cases of *placenta prævia*, which occurred in Breslau under the care of Prof. Spiegelberg. Of the cases eight were primiparæ, and sixty-six multiparæ. Thirteen of the mothers lived, and sixty-one died. Of the children thirty-four lived, and forty were lost. The methods of treatment were various.

Although *prolapse of the funis* is not so frequent a complication of labor

<sup>1</sup> British Med. Jl., Sept. 19, 1874.

<sup>2</sup> Obst. Jl. of G. B. and Ire., No. 14, p. 104.

<sup>3</sup> Arch. f. Gyn., B. 6., H. 3.

in this country as it is abroad, still it occurs sufficiently often to demand a thorough understanding of its treatment on the part of the accoucheurs. Dr. Engelmann<sup>1</sup> has studied the subject very carefully, giving us for the first time a clear scientific discussion of the cause, treatment, etc.

See AM. JOUR. OBST., vols. vi. and vii.

Cohnstein has proposed and practised a new method of performing *craniotomy* in head-last deliveries. It usually happens, he says, that with the considerable narrowing of the pelvis, that the body can be extracted no further than the shoulders, and then the head is reached with great difficulty, or not at all. Cohnstein's method has the advantage of being very easy and applicable as soon as the body can be extracted. It does not necessitate any particular practice or any special instruments. The dead child is not lacerated, and the soft parts of the mother are respected. At the union of the dorsal and cervical vertebræ, the soft parts were divided as deep as the apophyses of the vertebræ. Four or five arches were then detached with a scalpel and removed, and the dura mater of the cord pulled out as far as possible. A metallic catheter was then introduced into the cranium by the way of the spinal canal. The catheter was worked around in all parts of the cavity, and tepid water injected through it. Soon the cervical matter began to run out and as soon as the stream diminished the sound was gradually withdrawn.

An hour after the operation, on opening the skull, it was found quite empty.

Dr. J. L. May, of the French navy, reports a case of *Cæsarean section* for extreme rachitic deformity. The conjugata vera was  $4\frac{1}{2}$  cent. He met with no difficulty in the operation; no sutures were introduced into the uterus. There was no metritis or hemorrhage, and both mother and child did well. M. Silvestre<sup>2</sup> proposes the use of the elastic thread as a ligature in Cæsarean section, as he claims it would follow the shrinking of the uterus. He reports one successful case.

*Puerperal convulsions*, from their frequency and from the distressing character of the disease, always attract a great deal of attention.

Dr. T. M. Madden<sup>3</sup> has given us an elaborate paper on the etiology, prevention, and treatment of this condition. The treatment, he says, must be considered in reference to the state of the patient in each case.

Under the head of preventive treatment, he mentions relieving the kidneys by cupping over the loins, diluents, and mild diuretics, especially colchicum. Also purifying the blood by saline aperients and diaphoretics, and soothing nervous irritability by the bromide of potassium and belladonna. Cold affusion, a remedy recommended by Valescas in 1482, he stated to be one of undoubted efficacy, and chloroform, although perhaps overrated, he considers of unquestionable value in some cases. Extended mention was made of the value of chloral, opium, belladonna, and veratrum viride.

But it was pointed out that it should be the primary object to deliver the woman as soon as is consistent with her safety, and with that of the child. In those rare cases, in which delivery cannot be effected by ordinary means, Dr. M. mentions incision of the os, only, however, as an *ultima spes*. He gave the history of eight cases which he has met with in his practice. Of these four recovered. In closing the debate which followed the paper, Dr. A. Kennedy said he was gratified to find there was unanimity as to the necessity of bleeding, a mode of treatment, as confirmed by expe-

<sup>1</sup> Am. Obst. Jl., 1874.

<sup>2</sup> Archive de Tocologie, March, 1874.

<sup>3</sup> Irish Hosp. Gaz., June, 1874.

rience, as necessary to life. He had never regretted having bled in a single case of convulsion.

*Chloral* has been very extensively used during the past year in the treatment of this affection, with apparently good results. Dr. Fanny,<sup>1</sup> Paris, reports a series of cases, one where the chloral was used after other preparatory treatment, as phlebotomy, purgatives, anæsthetics, etc. There were sixteen cases and fourteen recoveries. The other series were treated by chloral alone. There were twenty cases; nineteen were cured, and one, result unknown. Dr. Fanny concludes, that chloral hydrate is now the best treatment for puerperal convulsions.

Dr. Charnier,<sup>2</sup> of Paris, also reports some cases which are very favorable for the employment of chloral in the disease. A writer in the *Western Lancet*, 1874, says that when he expects convulsions, he commences at once to give chloral, fifteen gr. doses, every twenty minutes. It does not interfere with the labor at all. He keeps up the use of the drug for twenty-four to thirty-six hours after labor. In one case he gave 230 gr. in thirty hours. The albumen in the urine disappeared in twenty-four hours. Dr. Conder-eau<sup>3</sup> relates a case where the combination of morphine with the chloral was found very satisfactory. A 3 of chloral did no good, but three-fourths of a gr. of muriate of morphia, given hypodermically, followed by thirty gr. of chloral, stopped the convulsions. In a second case, a man, he had similar success.

Dr. M. D. Mann<sup>4</sup> published a short paper, in which the use of the *serres-fines* for the immediate treatment of rupture of the perineum, according to the Vienna method, was detailed.

The *serres-fines* are preferable to sutures as causing less pain and anxiety to the patient, and requiring less skill and preparation on the part of the practitioner. Fourteen cases occurring in the General Hospital, Vienna, were given, which tend to show the advantages of the procedure advocated. Dr. Cleaver<sup>5</sup> met with a case where the rupture was completely through the sphincter ani. Spontaneous union occurred. Dr. Cauley<sup>6</sup> operated on a case of complete rupture immediately after delivery, and was rewarded with a complete cure.

No other accident which can occur in the lying-in room causes more anxiety, or more necessity for immediate and unhesitating action, than *post-partum hemorrhage*. The practitioner who meets with a case finds his nerve, energies, and knowledge tested to the last degree.

What wonder then that "when Dr. Barnes proposed the intra-uterine injection of the perchloride of iron, the profession eagerly seized upon it? for a weapon with which death in its most appalling form could be conquered was what every obstetrician and general practitioner wanted." <sup>7</sup> It came then into general use, particularly in England, and it was but natural that on the introduction of the subject in the different societies animated discussions should follow.

The first discussions took place in the year 1873, and are therefore outside of our limits. The discussion in the Dublin Obstetrical Society—a society which represents the best school of British obstetricians—took place, however, in 1874, and the journals for that year contain many of the most

<sup>1</sup> P. Fanny, Thesis, Paris, 1874.

<sup>2</sup> A. Charnier, *Anal. de Gynecol.*, Jan., 1874.

<sup>3</sup> *Bull. Gen. de Thérapeut.*, 1874.

<sup>4</sup> *Am. Jour. Obst.*, November, 1874.

<sup>5</sup> *Am. Pract.*, January, 1874.

<sup>6</sup> Canley, *Lancet*, July 10th, 1874.

<sup>7</sup> *Obst. Jour. of G. B. and Ire.*, p. 225, vol. ii.

important contributions to the subject. It shall be my object, then, to develop the main practical points which were brought out.

The original procedure, as recommended by Barnes, was: "Mix in a deep basin four (4) ounces of the strong liquor ferri perchloridi (Br. P.) with twelve ounces of water. Exhaust the air from the syringe; then pass the delivery tube into the uterus, so that its end touch the fundus; then pump gently and slowly; and the styptic fluid will thus bathe the whole inner surface of the uterus." X

From the cases reported, we see that the iron has been used in three classes of cases: First, as a prophylactic, to prevent hemorrhage, in those cases where, from previous experience, it might be expected. Second, in cases of secondary hemorrhage—represented by Dr. Heywood Smith's case—where bleeding occurred first on the 10th day, and the iron was used by repeated injections, the last being of the pure liquor, on repeated recurrence of the trouble. The hemorrhage in this case probably depended on a piece of retained placenta, and the fatal result showed the danger of the practice.

Both of these classes of cases may be considered, then, as being outside the limits of the discussion. The question then remains, in those cases where the hemorrhage occurs very soon after delivery, and where it depends on an atonic or uncontracted condition of the uterus, is the injection of the perchloride of iron intrauterine justifiable?

Dr. Barnes<sup>1</sup> says we have three stages of hemorrhage to deal with: Hemorrhage with active contractility of the uterine muscle; here excitants to action find their applicability. There is the stage beyond the first, when contractility is seriously impaired, or even lost; here excitants to contraction are useless. Our reliance must be upon the direct application of styptics to the bleeding surface. There is a stage beyond, where no remedy holds out any hope, unless it be transfusion. Again, he says, when the diastaltic system is dead to irritation, when life is almost ebbing away, when no known power can compel the uterus to contract, and thus close the bleeding vessels, styptics, which act chemically, will still seal up the mouths of those vessels, and corrugate the inner surface of the uterus. So long as cold, and we might add pressure, acts, use it, but when cold fails, use iron, which will not fail.

Dr. L. Athill,<sup>2</sup> in a paper on the subject, considers that in this discussion three points are to be decided. 1st, do cases occur which we have reason to believe would terminate fatally, or at least in which life is seemingly endangered, notwithstanding the judicious use of the remedies ordinarily employed for the arrest of hemorrhage? 2d, is the injection of a solution of the perchloride of iron of itself a dangerous remedy? and 3d, if so, are the dangers likely to follow its use such as to outweigh its obvious advantages, as an agent capable of affecting, with almost certainty, the further loss of blood? He then related five cases, four of which recovered, and the other died of peritonitis, on the fifteenth day; hoping that others might be induced to do the same, and thus some practical inferences might be deduced, on which sound rules for practice may be based. He concludes that—

1st. Cases of post-partum hemorrhage do occur in which the injection of the perchloride of iron, or some similar styptic, is alone capable of arresting the bleeding.

2d. That the injection of such styptics does not necessarily increase the

<sup>1</sup> Brit. Med. Jour., November 29th, 1873.

<sup>2</sup> Obst. Jl. G. B. and Ire., No. 14, p. 107.



tendency which exists in such cases to the occurrence of pyæmia, septicæmia, and peritonitis.

3d. That while it never should be had recourse to unnecessarily, it should not on the other hand be delayed too long.

He considers this ordinary means as most uncertain. In the discussion which followed, Dr. Madden said: "In truth, post-partum hemorrhage so severe as to require the use of the iron injection, is, when delivery has been properly conducted, I believe, a very rare accident. In any case, if due attention be paid to the duration of the second stage of labor, which can be neither abridged nor lengthened with safety, if ergot be given early and care be taken to secure and maintain firm contractions of the uterus by continuous pressure, we shall have few opportunities of using the perchloride of iron injection."

The doctor either has a very extensive practice, or, according to his own showing, does not pay due attention to the second stage of labor, for he reports ten cases which seemed to him to warrant the use of the iron. In one of these the iron failed, and in another the patient died on the 21st day.

He says, "I trust the Society will agree with me in considering that these cases are, on the whole, in favor of the employment of the remedy in some cases which resist all other treatment."

Dr. McClintock was strongly in favor of the use of the iron, and thought Barnes had not said too much when he called it a "new power."

Dr. Ringland thought the first question was, were we justifiable in employing the drug? To answer this two other questions must first be answered; was it efficacious for the purpose? and was it comparatively safe? As to efficacy, in the forty-five cases reported to the Society, it failed but once, and then the failure was due to the omission to empty the uterus of the blood, so that the drug was prevented from coming in contact with the uterus. Among the cases there had been eleven deaths, but he thought these cases, with the exception of two, could not be fairly attributed to the use of the drug.

He thought the danger was from the fluid being absorbed by the sinuses or penetrating the fallopian tubes. He proposes painting the inner surface of the uterus with the solid salt, and has done so several times, with one death.

Dr. Darby thought the cause of death in Dr. Ringland's case had been due to inflammation caused by the application of the drug to the inner surface of the uterus. Dr. Atthill thought Dr. Ringland's summary of the fatal cases gave an unfair impression, for he did not state as clearly as he should have done, that of the eleven deaths a considerable number died of diseases in no way attributable to the use of iron. His own view of the action of the iron was that if it did harm, it did so rapidly.

Dr. Snow Beck is the most uncompromising opponent of the perchloride injection. In several articles<sup>1</sup> he makes a vigorous attack on Dr. Barnes and his followers, in which he supports his position with many strong arguments, some cases, and a few post-mortem examinations. Beck's line of argument is that the hemorrhage is from the placental arteries, and not from the veins and sinuses, and that normally the veins and sinuses remain perfectly pervious, not filled at all with clots. In support of this position, he quotes some experiments by Dance (1828), and says it appears a fair deduction, from Dance's experiments, that no hemorrhages took place with the pervious condition of the veins; the blood must come from some other source when flooding takes place. He continues: "It may, I think, be

<sup>1</sup> Obst. J. G. B. and Ire., Nos. 10 and 11.



accepted that there is only one cause for post-partum hemorrhage, want of contraction or relaxation of the uterus, and one means of meeting it, viz., by procuring contraction of the uterus and compression of the arteries. What are the best means by which contraction can be induced and maintained, the bleeding stopped, and the veins or sinuses closed, and thus all absorption of noxious fluids obstructed? I maintain," he says, "that the means in ordinary use, when *efficiently* and *properly* employed, are enough. Dr. Barnes contends that these frequently fail to evoke contraction, and it becomes necessary to have recourse to a new plan of treatment by means of styptics applied direct to the bleeding surface, which acts chemically, and seals up the mouths of the blood-vessels, and corrugates the inner surface of the uterus."

He then tries to prove that the perchloride acts only by its irritant action, causing contraction, and not by virtue of its chemical action closing up the mouths of the vessels, as maintained by Barnes. As to the action, he quotes Dr. Norris, who says: "The injection was followed by instant and permanent uterine contraction, cessation of flow, and recovery. The manner in which the remedy instantaneously contracts and holds permanently contracted the previously flaccid uterus is something little short of marvellous." Dr. Norris also says, in five minutes after the injection, the sphincter vagina, which had previously allowed the passage of the hand, was so contracted by its use as finally to admit of only a single finger.

He adds numerous other quotations to prove the same thing. He then shows, as is always admitted, that the perchloride thrown into the circulation in any quantity must be fatal, and says: "So far as we at present know, no reasonable objection can be urged to the employment of these strong styptics, *provided* the one essential condition be attained—the complete and permanent contraction of the gravid uterus; for it appears a matter of little consequence by what means the contraction is produced. So long as it be complete and permanent so as to close the arteries and obliterate the canals of the veins or sinuses, and prevent any injurious absorption, the woman is safe, and will make a good recovery. But the great danger in the use of the agent, as shown by the fatal cases which have followed its employment, appears to consist in only partial contraction being produced, by which, although the hemorrhage was arrested, yet the veins or sinuses remained open, and permitted the iron to be taken up and conveyed into the general system, ending in the almost inevitable death of the individual. In some cases no susceptible contraction has followed the injection of even a strong solution of the iron, and in others the uterus has again relaxed."

Dr. Beck maintains that a proper use of the ordinary means at our command, among which he enumerates ergot, pressure, cold, galvanism, the introduction of the hand, the removal of all clots, and the injection of cold water, will answer in nearly all cases.

The question then seems to narrow itself down to this: Are the risks attending on the mode of treatment so great as practically to deter us from its use? The question is a clinical one, and cannot well be settled by abstract reasoning. Certainly the practice of some who have used this remedy as a preventive is wholly and entirely unjustifiable. But whether we may with propriety use it when we have fairly and faithfully tried and found wanting the usual methods, must, provided nothing better offers itself, it seems to me, be answered in the affirmative. Perhaps the tincture of iodine injection, lately proposed by Dr. Trask, may prove quite as efficacious and less dangerous. The question is an open one.

We must agree with Dr. Kennedy, when he says, in the discussion already alluded to: "If every man who meets with a dash of hemorrhage, resorts to

the use of the styptic, to the neglect of the trusted and hitherto successful modes of treatment, the consequences must be deplorable."

Other methods of treating this accident have found advocates. Dr. J. G. Miller reports two cases successfully treated by *pressure* of the *abdominal aorta*. Dr. Beck proposes to use vinegar as an injection.

Dr. P. C. Williams<sup>1</sup> reports three cases of post-partum hemorrhage relieved at once by *hypodermic injection* of Squibbs ex. fl. of *ergot*. 3 ss. in the thigh; in cases one and three, other means had failed; in case two it was alone tried, and answered at once.

Dr. P. Müller<sup>2</sup> proposes to use *Esmarch's bandage* when death is imminent as the result of hemorrhage. By driving the blood from the limbs toward the heart and brain, cerebral anæmia is obviated, and the heart continues to receive blood enough to continue its action. He has as yet had no cases.

Dr. W. Highmore,<sup>3</sup> after seeing a fatal case of flooding in child-bed, says: "It has occurred to me since, if I had at once collected the hemorrhagic blood of the woman, and defibrinated and warmed it to the proper temperature, by my clinical thermometer, over hot water, and then injected it with my Higinson's syringe and the transfusion pipe, it would all have been the work of a few minutes, and the only chance of life the poor woman had would have been offered her." This brings us to the subject of *transfusion*—a subject which belongs only in part to the obstetrician. During the last year transfusion has awakened renewed interest, and occupied a large share of attention. To give a synopsis of all that has been done, would require a greater amount of time than remains to us, and would be very unsatisfactory, for no very definite conclusion can be as yet arrived at. I copy the following from a report to the British Medical Association by Dr. Madge.<sup>4</sup> The forms of transfusion which are now most in vogue, and which have been attended with the greatest amount of apparent success, are:

1. Transfusion with defibrinated blood.
2. Mediate transfusion with pure blood.
3. Immediate transfusion from "vein to vein."
4. Immediate transfusion from "artery to vein."

The first method, with defibrinated blood, is the one most generally used in recent cases. It has the advantage of avoiding clots.

The second mode, if it would be done with ease and safety, would no doubt be preferable. The danger is that clots will enter the vein and lead, like carelessly defibrinated blood, to pyæmia and embolism. It has, however, many advocates. Dr. Howe,<sup>5</sup> of this city, has, I believe, proposed and practised the use of the aspirator for this form of transfusion, the blood being drawn into it, and then directly into the patient without being exposed to the air. The danger of clots is thus diminished: he also adds ammonia to the blood.

Third. Immediate transfusion from vein to vein has been revived and practised by Dr. Aveling. The doctor has also invented a very useful instrument for the performance of the operation. Many favorable cases are reported.

Fourth. Immediate transfusion from artery to vein is the oldest form. It is now practised by using the blood of a lower animal, generally a lamb. A large number of successful cases are reported, mostly in Germany.<sup>6</sup>

<sup>1</sup> St. Louis Medical and Surgical Reports, December, 1874.

<sup>2</sup> Wien Med. Presse, Nov. 8, 1874.

<sup>3</sup> Lancet, January 17, 1874.

<sup>4</sup> Obstetrical Journal of Great Britain and Ireland, No. 14, p. 120.

<sup>5</sup> J. W. Howe, N. Y. Medical Journal, October, 1874.

<sup>6</sup> Dr. Oscar Hesse, Al. Wien Med. Zeit., December, 1874, etc.

"If," says Dr. Madge, "the question be now asked which is the best and safest form of transfusion, I think it must be admitted that, in the present state of our knowledge, each of the four principal plans I have mentioned may be employed with almost equal chances of success."

Still another method has been proposed, viz., the hypodermic injection of the blood. Dr. Lendenburger<sup>1</sup> has performed it once, with some improvement to his patient. The subject has also been fully discussed at the N. Y. Obstetrical Society, the N. Y. Academy of Medicine, and by Dr. Barnes, and numerous other writers at home and abroad.

The highest office of the physician is to prevent and not to cure disease, it has been said, and, acting on this principle, Dr. Wm. Goodell, of Philadelphia, has established<sup>2</sup> very stringent regulations for the *prevention of puerperal fever* at the Preston Retreat, an institution under his charge. The statistics of the institution compare favorably with private practice.

Good ventilation is insisted upon. The wards are used in rotation, and, as well as the beds and bedding, are carefully disinfected while idle. All danger of infection by nurses and physicians is guarded against by careful disinfection of person and clothing. Sponges are not used, but wads of soft oakum, which are thrown away after use. Previous to delivery the woman's general health is carefully attended to. When labor begins the patient takes a bath and a rectal injection. The bag is ruptured artificially, and the liq. amnii collected in a grocer's scoop. Any delay in the second stage of labor is met by forceps or vectis. The placenta is delivered by supra-pubic pressure, Credé's method, and the cord stripped and tied, and left to dry up without any dressing. Ergot is given when the head presses the perineum.

The diet is liberal, and the patient gets up on the second day while the bed is being made. On the fourth or fifth day she gets up and is dressed. After-pains are stopped by morphia and quinine, in large doses, till the ears ring. Vaginal injections are avoided as much as possible, as the syringe may be a conveyer of contagion. The thermometer, for similar reasons, is not brought into general use. No bed-pans are used. Should the lochia become offensive, and the upright position fails to correct it, then, and then only, is a solution of permanganate thrown up the vagina. The upright position he considers a good deodorant, better than any injection. They also give a chance to air well the bed and bedding, a great advantage in a hospital. He says: "I will hazard the assertion that there is a form of puerperal septicæmia not necessarily accompanied by putrid lochia, which stubbornly resists treatment until the patient is made to get out of bed."

The women are encouraged to get up for good when they feel so disposed. "In my experience the woman feels better on the fifth day than she does on the ninth if kept in bed." The purge on the third day is omitted, being illogical, and in some cases dangerous. Three instances of puerperal peritonitis have come to his notice which were as plainly as possible referable to the use of a purgative.

Quinia is given without stint, as being above all remedies the one best suited for puerperal disease. The results in the retreat have been, out of 756 cases of delivery, 6 deaths, and two of these were from some puerperal diseases.

In the course of a paper on *Lymphangitis in Pelvic Pathology*, Dr. Tilt, of London, says: 1st. Pelvic cellulitis, benign or septic, originates in the lymph spaces and the capillary lymphatics that have been wounded in some lesion of the utero-vaginal mucous membrane.

<sup>1</sup> Med. Corr. Blatt, 20, 1874.

<sup>2</sup> Obst. Jour. of G. and Ire., No. 16. Am. Suplt., p. 49, etc.

11. Though often overlooked in post-mortem investigations, sporadic puerperal lymphangitis is not uncommon, but is more frequently met with in connection with and eclipsed by phlebitis.

13. Whether women be poisoned by puerperal lymphangitis or phlebitis the general symptoms are the same in nature and intensity, and the local symptoms vary according to the amount and to the variety of its primary and of its secondary lesions.

14. The increase in size and number of the pelvic lymphatics during pregnancy, and their increased functional activity, strongly support the rule not to operate on pregnant women.

15. As the liability to puerperal lymphangitis is in direct proportion to the number and to the gravity of the lesions inflicted on the utero-vaginal mucous membrane by tedious labors, it is advisable to shorten the labor by the use of forceps according to the modern practice.

17. The innocuity of injecting a solution of tr. iod. into the womb immediately after delivery, and of a solution of the perchloride of iron very soon afterwards, warrants the injunction to inject into the womb some strong disinfectant substance whenever the lochial discharge is fetid.<sup>1</sup>

Dr. Duncan,<sup>2</sup> in an address on *puerperal pyæmia*, thinks the time has come when obstetricians should try to leave off the use of the convenient term puerperal fever, because it embodies error. There is nothing essentially puerperal known in it, nor is there anything of the nature of a fever, as the term is generally understood. A new name, already widely used, is to be found in the already comparatively old term *pyæmia*. It is not to be supposed that *pyæmia* is a term to be analyzed into its component parts and held as implying purulent blood.

*Pyæmia*, in several forms, which are characterized each by more or less peculiar symptoms, but most distinctly by their pathological appearances, discovered post-mortem. There is that most widely-known form, where you have septic emboli and scattered abscesses caused by them, and perhaps otherwise also. There is that where you have inflammation of the peritoneum and other serous cavities, including the synovial and endocardial. Then the form when the mucous membranes are chiefly affected—the muco-enteric. And lastly, there is that where the only results found after death are alteration of the blood, enlargement of the spleen and the liver, and degenerations of their most important tissues, with similar degenerations in other organs. As to treatment he remarks: "These results go far to justify the belief that *pyæmia* is a septic disease, and that puerperal *pyæmia* may be almost, if not altogether, prevented by the application to delivery of a practice based on antiseptic principles. . . .

"Such imperfect antiseptic precautions," he says, "as the use of antiseptic gauze and otherwise, I have used with apparent advantage, but we have a long way to go yet in order to recover complete antiseptic delivery and subsequent treatment. To reach the desirable objects, the efforts of several good minds are already, I know, directed both at home and abroad, and from recent unpublished cases of successful antiseptic treatment of wounds of the penis, where periodical discharges of urine have to be permitted, supply a sketch in miniature of plans that might be applied to ordinary confinement.

Dr. J. S. Parry<sup>3</sup> gives an interesting account of a peculiar epidemic of puerperal fever that occurred in the Blockley Hospital, Philadelphia. It has been characterized by diphtheritic deposits about the vulva, vagina,

<sup>1</sup> Ob. Jour. of G. B. and Ire., p. 229, 1874.

<sup>2</sup> Ob. Jl. G. B. and Ire., Sept., 1874.

<sup>3</sup> Am. Jl. Obst., May, 1874, and Am. Jl. Med. Sc., Jan., 1875.



uterus, and sometimes on other portions of the body. It sometimes lines the enteric vagina and uterus. With regard to treatment Dr. P. remarked that he had tried faithfully all the zymotic agents, and he relies upon the administration of opium, in enormous doses if necessary. Intra-uterine injections seemed to increase the local trouble and pain, and have been of no use whatever.

I find a number of cases of *extra-uterine pregnancy* recorded, some of which are so unusual as to be worthy of your attention. Dr. Bandl<sup>1</sup> reports one which occurred in Prof. Braun's wards in Vienna. She was well during the whole pregnancy, except some emaciation and weakness, dating from the second month. She had been able to be about all the time. The abdominal enlargement was equal to that of the ninth month of pregnancy. In the ninth month of gestation some febrile action set in, and she died after three days. The child was removed five minutes after death by the Cæsarean section. It was alive, but lived only five or ten minutes. It weighed eight pounds.

"It is evident from the autopsy," says Dr. B., "that at an early period of pregnancy the ovum had burst, the placenta remaining in its original place, and the fœtus developing in the abdominal cavity. It is most surprising how well both mother and child bore the process. It is probable that the ovum burst in the third month, when the emaciation commenced, and this is also indicated by the post-mortem appearances."

Dr. Hodges<sup>2</sup> met with a case which he considers to be one of tubal pregnancy, coincident with a normal uterine pregnancy. The sac burst about the fifth month without much hemorrhage, and the fœtus died, and a limited peritonitis occurred. The normal pregnancy developed as usual, and the woman was safely delivered and did well.

The extra-uterine dead fœtus was absorbed.

Dr. Atlee<sup>3</sup> reports a case where he performed abdominal section when the patient was in articulo mortis. She improved, but died on the third day.

Dr. Janvrin<sup>4</sup> of this city, has given us a very interesting account of a case, which he considers to be one of *interstitial pregnancy*. Parts of the fœtus were passed by the rectum, and the doctor endeavored to remove the remainder by an operation through the same channel. The patient was, however, too far gone, and did not rally from the operation.

Unfortunately the diagnosis was not confirmed by a post mortem examination, though there seems to be but little doubt of its correctness.

Thus far we have considered only the mother, and now a few words in regard to the fœtus. In the *Obstetrical Journal* we find a case in which Dr. Myles<sup>5</sup> restored a newly born child, *where animation was suspended for two hours*. The labor was easy and natural. Artificial respiration, baths, and friction with whiskey were the means employed. He was induced to continue his efforts by hearing the continuous beating of the fœtal heart.

Dr. Duncan (Br. Med. Asso.) found that the *child's neck separates* at a tension of 120 lbs. The spinal column snaps at 105 lbs. One leg is enough to pull by to accomplish these results. These results are of importance in deciding the old question of forceps versus version in cases of contracted pelvis. The forceps can pull more than this without doing any harm.

"It is now admitted," says Pfannkuch,<sup>6</sup> "that the *umbilical cord* is sometimes broken during labor. The question remains to be solved how great a force

<sup>1</sup> Wien Med. Woch., Aug. 8, 1874.

<sup>2</sup> St. Louis Med. and Sur. Jl., August, 1874.

<sup>3</sup> Br. Ob. Jl., Am. Sup., No. ii.

<sup>4</sup> Am. Jl. Obst., Nov., 1874.

<sup>5</sup> Obst. Jl. Gr. Br. and Ire., vol. 13, p. 12.

<sup>6</sup> Arch. f. Gynäk., B. vi., H. iii.



is necessary to accomplish this result. Cases are recorded (Späth) where a violent expulsive pain, by throwing the child to a distance from the vulva, has broken the cord, the mother being in bed. As a rule, this accident occurs when the mother is erect or semi-erect. It commonly occurs in secret or dishonorable pregnancies, and thus becomes of importance in a medico-legal point of view."

After various experiments the author gives as his conclusions, that "In all labors, where the full weight of the falling child is brought to bear on the cord, there is not only the greatest probability, but almost a certainty, that the cord will be broken."

Dr. Zweifel<sup>1</sup> has investigated the effect of *chloroform* given to the mother during labor *on the child*. He has proved its presence in the fresh placenta, the urine, and breath of the child.

He says, since the use of narcotics in general are contraindicated in infants, it is an important question for obstetricians to decide, to just what degree anæsthesia may be carried in women in labor with impunity to the fœtus.

[THE pressure of original matter compels us to omit the "*Quarterly List of Literature*"; we do not fear that our readers will regret the exchange of this, to some, doubtless, useful department, for more interesting and attractive reading. In view of the extra sixteen pages added to this number, it is scarcely necessary to state, that the Transactions of the New York and Philadelphia Obstetrical Societies have been necessarily and reluctantly abridged, also that we have been compelled to omit all Reviews and Notices of Books. We shall endeavor to remedy all these deficiencies to the satisfaction of all parties in the next Number.

COMMUNICATIONS HAVE BEEN RECEIVED FROM PROFS. WM. T. LUSK, New York, on "An Epidemic of Puerperal Fever in Bellevue Hospital;" WM. GOODELL, Phila., "Clinical Memoir on Turning in Pelves narrowed in the Conjugate Diameter;" A. F. A. KING, Washington, D. C., on "A New Basis for Uterine Pathology;" DRs. J. S. PARRY, Phila., on "Pregnancy and Labor in Epileptic Women;" RICHARD B. MAURY, Memphis, Tenn., "Notes on Perimetritic Inflammation;" LEOPOLD PUTZEL, New York, on "Puerperal Fever," Prize Essay at Bellevue Med. Coll., 1875; F. D. LENTE, Cold Spring, N. Y., "Incomplete Lacerations of the Perineum;" E. NOEGGERATH, New York, on "The Immediate Treatment of Lacerations of the Perineum."—ED.]

<sup>1</sup> Berlin, Klin. Wochensch., 21, 1874.

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ORIGINAL COMMUNICATIONS.

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CLINICAL MEMOIR ON TURNING IN PELVES NARROWED IN  
THE CONJUGATE DIAMETER.

By WILLIAM GOODELL, M.D.,

PHYSICIAN-IN-CHARGE OF THE PRESTON RETREAT; CLINICAL PROFESSOR OF THE DISEASES OF  
WOMEN AND CHILDREN IN THE HOSPITAL OF THE UNIVERSITY OF PENNSYLVANIA, ETC.

(Read before the Philadelphia Obstetrical Society, February 4, 1875.)

ALTHOUGH, on account of the size of her offspring, the daughter of Orchomenos died in travail; and although, as Homer sings, Latona was nine days and nine nights in giving birth to Apollo—

Λήτω δ'εννῆμάρ τε καὶ ἐννέα νυκτας ἀέλπτοις  
'Ωδίνεσαι πέπαρτο;—

it does not appear that their celestial paramour devised any mode or relief for such contingencies. Nor did his ox-eyed consort, who was the tutelary divinity of pregnant women. Nor did other erring gods and goddesses, not even Latona herself, who, in the end, became a powerful deity. To Aspasia, the mistress of Cyrus the Younger, was reserved the honor of suggesting the induction of labor in women unfit to bear; and

to Moschion, a physician of the second century, that of resorting to version whenever the labor is a difficult one.<sup>1</sup>

These golden hints appear, however, to have faded from view; for in the fifth century, we find Aëtius, the first of Christian medical writers, reviving the teachings of Aspasia (*Tetrabib.* 4, *Sermo* iv., *cap.* xviii.); and, in the seventh century, Paulus Aegineta, surnamed Obstetricus from being the first "man-midwife," refuting the aphorism of Hippocrates that breech presentations are generally fatal to both mother and child. But printing was yet an undiscovered art, and these dawning thoughts lay fallow. At intervals of time, men endowed with great originality, such as Paré, Guillemeau, and Mauriceau, throw out hints that in isolated cases of labor they resorted to version; but from these scattered observations no general laws were deduced. According to Brudenell Exton (*New System of Midwifery*, 1751), even the brothers Chamberlayne gained their reputation, not so much from the use of the forceps as from the operation of version—which in their day was little known. Influenced by their example, Sir Fielding Ould (*Treatise on Midwifery*, Dublin, 1742) taught his pupils, that in contracted pelves a delivery by podalic version is easier to the mother and safer to the child. His able cotemporary, Paul De Wind of Holland (Sue's *Essais Historiques des Accouchemens*, vol. ii., p. 394), vehemently proclaimed the inferiority of the forceps to version. Even John Burton, the Dr. Slop of Tristram Shandy, so far forgot his learning and his breeding as to abuse Smellie and his forceps in a tirade, which in bitterness was surpassed only by that of the midwife and Amazon, Nihell. But the partisans of the forceps prevailed, and the manual delivery in faulty pelves appears to have been forgotten until 1847, when the genius and the enthusiasm of Simpson brought about its great revival.

Notwithstanding the weight of authority, at the present day undoubtedly leans toward podalic version in pelves narrowed in their conjugate diameter; yet the question of its utility is still an open one, and this operation has not yet become an undisputed canon of obstetrics. Men of high standing in the profession, such as Byford of Chicago, Martin of Berlin, Radford

<sup>1</sup> Περὶ τῶν Γύναικείων Παθῶν, in I. Spachii Gynæciorum Harmoniâ, Argentinaë, 1597.

of Manchester, Duncan of Edinburgh, and Depaul of Paris, look with disfavor upon it; while the great mass of the profession on this continent, influenced by the teachings of Hodge and of Meigs, and by their own unrivalled skill in the use of the forceps, regard a safe delivery of the child to be impracticable, whenever that instrument fails to extract it. In view of this perplexing discrepancy of opinion, the object of this paper will be to give my own personal experience, and to discuss the question as soberly and dispassionately as a new convert and a warm partisan can.

The advocates for the use of the forceps in narrow pelves contend—

1st. That the neck of the child cannot sustain much traction, and that, consequently, the degree of tractive force which can be safely exerted by the forceps far surpasses any extractive power which can be brought to bear upon the after-coming head.

2d. That during traction by this instrument, moulding of the head is a gradual process, and may go on in safety for an indefinite length of time. But, after version, moulding cannot take place, because death ensues if the delivery is not very rapidly effected.

3d. That when version fails to deliver, the subsequent operation of craniotomy or of cephalotripsy is a more difficult one than when the forceps fails.

These are the stock arguments against version. But the main reason which, I think, influences the profession at large against this mode of delivery, is their habitual ill-luck in ordinary breech-cases, and also their fear lest the neck should part and the head be stalled in the uterine cavity.

The arguments in favor of version may be summed up as follows:—

1st. In cephalic presentations the head usually lies in the transverse diameter of the brim, and does not tend to dip. It presents by its vault, which is, therefore, flattened out and made broader by the resistance at the brim.

2d. The forceps as generally applied catches the unflexed head usually in the occipito-frontal diameter, exceptionally in the fronto-mastoid diameter. In either case the compression made by it causes the head to bulge out laterally, hence to

overcome this mechanical disadvantage very great brute force is needed.

3d. When the promontory of the sacrum is laterally displaced and the brim unsymmetrical, should the vertex present in the contracted side of the pelvis, it may, by traction on the proper foot, be turned around and brought into relation with the more roomy side.

4th. By version the small end of the cranial wedge—viz., the unyielding base—is the one which first enters the brim. From this mechanical advantage there is gained a maximum of lateral compression of the upper, thinner, and wider portions of the skull; in other words, the bones bend in and overlap better.

5th. Clinical observation conclusively proves that the unflexed after-coming head is nipped, not in its biparietal diameter, but in the small end of the fronto-parietal cone or wedge, viz., the bitemporal diameter, which is from half an inch to three-quarters of an inch shorter. The biparietal diameter passes through to one or the other side of the conjugate diameter. From this mechanism of descent less extractive force is needed, and the time necessary for delivery is thereby much abridged.

6th. The bilateral compression gained by version is the least dangerous to the life of the child. On the other hand, the occipital blade of the forceps may destroy life by pinching the cord when coiled around the neck, or by too great and too prolonged a pressure on the medulla oblongata.

7th. The jutting promontory so protects the cord from pressure that after version the necessity for immediate delivery is not so urgent as in ordinary breech cases.

8th. The child's neck can sustain with impunity a very great strain, and if to this be conjoined, by the hands of an assistant, a propelling supra-pubic pressure upon the vault, fully as great an extractive power can be brought to bear upon the after-coming head as by the forceps in head-first labors.

9th. Provided the brim is wide enough to admit the hard base of the skull, the operation of craniotomy or of cephalotripsy is quite as easy in the one as in the other mode of delivery. The brain can be reached by perforating the roof of the mouth, the skull behind the ear, or, what is the point of



election, the suboccipital region. Moreover, the base can be more readily grasped and crushed by the cephalotribe. When the brim will not admit the cranial base the difficulties attending craniotomy and cephalotripsy are perhaps greater, but by no means insuperable.

10th. Since by version the narrow conjugate is traversed more quickly, the passage of the child's head is attended with less risk of injury to the mother's tissues.

11th. After craniotomy turning is often a very efficient way of completing the delivery.

12th. By the induction of premature labor conjoined with version, living children can be delivered through pelves so narrow as otherwise to demand craniotomy.

From arguments so conflicting truth can be sifted out, not by fireside theories and closet speculations, but by an appeal to bedside experience. Let us study them in that light. The following illustrative cases are not all that I can offer, but they are perhaps the most salient ones. From them I shall also exclude all cases of original breech-presentations in narrow pelves, of which I have seen several.

CASE 1.—My first case of turning in a narrow pelvis was forced upon me by a prolapse of the cord. Had not this accident happened I should have resorted to the forceps. So far as the life of the child was concerned it was a failure, but in other respects it afforded me much food for reflection. A. K., æt. 25, bore her first child in 1863. She was, as she alleged, four days in labor, and had in attendance two physicians of experience. After many hours of hard tugging at the forceps they finally delivered her by craniotomy. Since that time fruitful intercourse had for six years been prevented by some one of those vile measures so much in vogue at the present day. Through very unwelcome carelessness she again became pregnant, and on March 27, 1870, fully up to term, she fell into labor and into my hands. I found the brim kidney-shaped, the promontory easily reached by my short index finger, and the unruptured bag of waters filled with a large coil of the cord. To replace this, the postural and other methods were in vain tried both before and after the membranes broke. Half an hour after the water had drained off the child was turned by the bimanual method. Much force was needed to extract the head,

but it was not made as promptly and as efficiently as I have since learned to make it. The head passed the brim with a jerk, and was thereafter easily delivered, but the child was still. It weighed eight pounds five ounces. Here was a pelvis whose conjugate diameter measured less than 3.5 inches, and yet the delivery was comparatively easy and short. From a pretty large experience with flat pelvis I was satisfied that had the forceps been applied, one hour or more would have elapsed before so large a head could have been made to pass the narrow brim—perhaps not without craniotomy.

CASE II.—On this occasion your President and I were called in by the attending physician to a desperate and ultimately fatal case of puerperal eclampsia. Vain attempts to deliver were made with the forceps. With extreme difficulty version was performed, but after its completion the delivery was comparatively easy. The child was dead, and weighed exactly thirteen pounds, being the largest one that I have ever seen. Strictly speaking, this was not a case of narrow pelvis; but it was one of disproportion between the pelvic diameters and those of the head, and as such I report it to prove the superiority of version over the forceps.

CASE III.—This case was also one of compulsion and not of choice. M. McC., æt. 27, had in her previous labor been prematurely delivered at the end of the seventh month of gestation. The child weighed less than five pounds, and yet her labor lasted over forty-eight hours, six of them being attended with great suffering and violent expulsive pains. Her physician recognized the conjugate contraction, and in her second pregnancy urged upon her the induction of premature labor. Upon her refusal to submit to this operation he declined to attend her, and she sought admission into the Preston Retreat. At 8 o'clock A.M., June 8th, 1870, after labor had lasted ten hours, I found the waters dribbling away from an unexpanded os. The head, presenting the occiput to the left ilium, could not bear upon the cervix, but rolled about upon the shelf formed by the promontory. By careful measurements the conjugate diameter of the brim was estimated at a trifle over three inches in length. At 3 o'clock P.M., finding her condition unchanged, I decided to dilate the os with the water-bags and apply the forceps to the sides of the head. While preparing to

do so, the woman suddenly began to twitch in her muscles, to start convulsively, and to complain of blindness and intense headache. Fearing an attack of eclampsia, I decided to turn. The woman was accordingly etherized, brought down to the edge of the bed, and each hip supported by a nurse. Two fingers of the left hand were then squeezed into the os, and ultimately a third. By these, in conjunction with the external use of the right hand, version was made with so little effort that I did what I have never before nor since done; I turned the child more than half a circle and had to reverse the process. One foot having been dragged down, time was given for the os to dilate before the body and arms were brought down. By then following a method which I had previously found of advantage, and which will be hereafter described, I quickly delivered the child. It was asphyxiated, but promptly recovered. It had a deep furrow on the right side of its head, and weighed seven pounds and twelve ounces.

CASE IV. was the first one in which I turned from choice. Late in the evening of January 15th, 1871, F. K., an undersized primipara, aged 31, was admitted, having already been in labor twenty-nine hours. Her last catamenia ended April 15th, so that she was fully up to term. I found the waters drained off, the conjugate diameter somewhere about three inches and a half, and the occiput looking directly towards the left ilium, but not bearing at all upon the cervix. Having lately had a series of tedious forceps cases, I felt much inclined to try version. Wishing, however, some backing, I sent for my friend, Dr. E. L. Duer, who verified my measurements and sustained me in my decision. I accordingly turned by the bipolar method, and at once proceeded to deliver. Dr. Duer pushed down the child's head from above, while I, by three short movements of traction, extracted it without the slightest delay. The child weighed five pounds and fourteen ounces, was born lusty and did well, although bearing on its right temple a slight depression, such as that great military surgeon, Ambrose Paré, aptly compared to the bruises which a silver kettle-drum receives when thrown on the ground. Nine days after, the mother accidentally discovered a dislocation of the acromial end of the left clavicle, which I should like to believe was a congenital deformity.

CASE V.—On December 16th, 1871, E. R., a dwarf in size and in wretchedly bad health from phthisis, applied for admission to the Retreat on account of the severity of her three former labors. In her first labor the late Prof. David Gilbert, whose skill with the forceps is well known, tugged away at that instrument for five hours and delivered her of a badly marked but living child. In her second pregnancy she refused to have labor induced, and went to term. This time he kept the forceps on for nine hours, and after lashing the handles, delivered her of a dead child with its head badly crushed in. He now told her that hers were the two most difficult labors he had ever seen, and vowed never to attend her in another. Again she became pregnant, but kept this fact concealed from him until labor set in. Upon great entreaty he very reluctantly consented to attend her; had the forceps on for three hours, and delivered her of a living but small and badly marked child. Following each one of these labors a smart attack of peritonitis kept her in bed for four weeks after the first two, and for three weeks after the last one. After each one also, the catheter was needed for several days. Her husband now died, and she remained a widow for several years; then married again, and soon found herself pregnant for the fourth time.

Upon her admission I carefully examined the pelvis. My index finger easily touched the sacro-vertebral angle, and gave a length of four inches to the diagonal conjugate, and therefore one of 3.32 inches to the true conjugate. Labor was not induced, because she was very near her time. Four days later she went into a slow labor, with the occiput to the left ilium. After nine hours the membranes broke prematurely. The expulsive pains then increased in severity until in three hours more they became, as my note-book states, "terrific." Yet the os did not dilate, and the head remained movable above the brim; I therefore gave her ether, turned the child, and delivered it alive in less than three minutes after strong traction and propulsion had been begun. It was a well-nourished girl, weighed seven pounds ten ounces, and bore a deep pressure-mark in front and above the ear.

So impressed was this woman with the advantages of turning over those of the forceps, that on March 10th, 1874, she struggled against great difficulties in order to put herself again into my

hands. She was then bed-ridden, and desperately ill with pulmonary hemorrhages and hectic fever. Her friends daily expected her death; but by tears and entreaties she prevailed upon her husband to carry her to a hack, in which, lying on his lap, she was conveyed to the Retreat. On entering the building she went off into an eclamptic fit. This time I turned through the membranes by two fingers, then ruptured them and as before forcibly delivered her of a plump and lively boy. It weighed six pounds fourteen ounces, and bore the same kind of a pressure-mark as its sister. From the former labor as well as from this one, her recovery was much retarded by her constitutional disease, but in nowise from the effects of the labor. I thought her dead and buried, when, to my astonishment, in January last she feebly walked into my room at the University and sought my advice for an abdominal tumor. My assistant, Dr. D. Bray, and myself carefully examined her. We both easily touched the promontory with the index finger, and found the tumor to be a gravid womb. She at once got an order of admission to the Retreat, and I am daily expecting her to be brought in.<sup>1</sup>

CASE VI.—Some time in 1873, the month and day I cannot recall, my friend Dr. R. B. Cruice asked me to see one of his patients, whom he had delivered some three or four times, but always of large children, and, on each occasion, after a tedious use of the forceps. She was again in labor, and he had several times applied and reapplied the Hodge forceps; but after powerful traction the blades invariably slipped. I found a very fat woman in hard labor. The pelvis was unusually deep and somewhat narrowed. The head was perched above the brim, with the occiput looking towards the left ilium. Failing to get the blades over the sides of the head, I had to content myself with their oblique application; but the head being a large one, they did not hold well, and we therefore decided to turn. The operation of version was by no means as easy as the subsequent delivery. The child was alive and unusually large. The mother's convalescence was retarded by a sharp attack of peritonitis—then prevailing as an epidemic—but she eventually did well.

<sup>1</sup> On May 5th I delivered this woman for the third time, by version. The child was alive and weighed eight pounds and four ounces.



CASE VII.—Not long ago I exhibited at a meeting of this Society the body of a child which I by version had a few hours previously delivered through a very narrow pelvis. The remarks which I made on that occasion were not reported in the *Transactions*, because due notice of the intended presentation of the specimen could not be conveyed to the members in advance of the meeting. This case I shall therefore now report at length.

On March 13th, 1874, M. S., a native of Germany, sought admission in the Retreat on account of her previous very difficult labors. She was forty years old, and had been married twice in eighteen years. Both she and her twin sister were reared on the bottle; they cut their teeth very late, and neither could stand on her feet until after she was two years old. Both married, and both became pregnant; but the sister could not give birth to her child, and died after being delivered by the Cæsarean section. M. S.'s first labor took place in Germany, the child presenting by the head. After many hours of hard labor, the midwife in attendance foreseeing trouble sent for a physician, who broke up the vault, but failed to extract the child. A second physician was called in, and finally three more before she was delivered. So much difficulty was experienced that one arm was torn off with the blunt hook.

Her second labor took place also in Germany. The fœtus could not have been much older than the fifth month of uterogestation; for, according to her description, "it had no nails on its fingers or on its toes," yet the labor was a very tedious one. Her husband now dying, she remained a widow for a number of years, then married again, and emigrated to this country. Her third labor happened in a small hamlet in the interior of Pennsylvania. The attending physician being too far removed from any other to call in help without much delay, pluckily took charge of the case single-handed. After breaking up the head and working over the case for the better part of a day and a night, he succeeded finally in delivering the woman of a fœtus so mutilated as, to borrow the language of her husband, "no longer to look like a child." In 1872 her fourth labor occurred; this time in one of the suburbs of this city. The arm presented; it was torn off, and so was a leg, before the child could be turned and delivered. According to

the husband's statement, no instruments were used, yet the child's head was crushed in.

She told me that her catamenia were last seen in the middle of June, and that she was daily expecting her confinement. Owing to this statement, and also to the prevalence in Philadelphia of an epidemic of puerperal fever, labor was not induced, as it should have been. On March 31st, at 10 A.M., she fell into labor, and I now for the first time examined her pelvis. The head presented transversely, with the occiput looking towards the left ilium. With my index finger, Earle's pelvimeter, a carpenter's rule, and a pair of calipers, I obtained the following measurements, to which, for the sake of comparison, Schroeder's measurements of the standard pelvis are appended:—

	<i>Pelvis of M. S.</i>	<i>Standard Pelvis.</i>
Between crests of ilia	9.5 inches	10.75 inches.
External conjugate	6. “	7.8 “
Diagonal conjugate	3.5 “	5.01 “
True conjugate <sup>1</sup>	2.82 “	4.33 “

Having satisfied myself that the child was rather under than over the average size, I decided to turn. After ether was administered, I passed in my left hand and made a more careful examination. The transverse diameter of the brim seemed sufficiently ample, but, to my surprise, I could not pass the breadth of my hand through the conjugate without painful squeezing. Now I happened to know that my hand, with some compression, measures just 2.5 inches across the knuckles. To explain the discrepancy between my outward measurements and this internal one, I made further exploration, and found it to be owing to two sharp ridges of bone projecting inwardly along each edge of the pubic symphysis. This somewhat staggered me in my intention to turn; but, upon reflecting that in her last labor, that of an arm presentation, the head was not perforated; and also, that from the shallowness of the pelvis, the difficulties of craniotomy would not be much enhanced, I decided to go on. I turned through the bag of waters, then broke it, and seized both feet; but found there was not room enough to bring them down. I therefore let go the right foot

<sup>1</sup> The true conjugate is found by subtracting Schroeder's correction of .68 of an inch from the diagonal conjugate.

and coaxed down the left one by hooking two fingers over the instep. Very powerful traction was needed to pull the half-breech and the chest through the brim. Finding the child still alive, I at once brought down the arms, and proceeded to extract the head. By a propelling power of about fifty pounds on the vault of the head, and a tractive weight of not less than one hundred and twenty pounds on the child's neck, aided finally by a pump-handle movement of traction on its legs, a dead child was born in exactly twenty minutes after the rupture of the membranes. Its neck was unbroken, but there was a vertical groove on the pubic side of the head, and on the opposite side a deep depression and fracture of the parietal and frontal bones. The bottom of this indentation lay above the ear, but in front of it and in a line with its tragus. The bitemporal diameter at the seat of fracture measured 2.5 inches. The child weighed six pounds twelve ounces. The woman did not need the catheter, was up and dressed on the fifth day, and left for home on the twelfth. Had the cephalic presentation been retained, and craniotomy been resorted to in this case, the operation would, without doubt, have been a tedious one and perhaps a dangerous one.

CASE VIII.—Early in July, 1874, E. B., a very short woman, aged 18, and two years married, applied in her second pregnancy for admission to the Retreat on account of her previous difficult labor, which had lasted three days. Three physicians were in attendance; two of them men of large experience. After repeated and prolonged trials with the forceps, they finally delivered her by craniotomy; but not soon enough to prevent the mishap of a large vesico-vaginal fistula. The excoriation of her person from the constant dribbling of urine, her nervous apprehension and dejected appearance, made her a very pitiable object. Her catamenia ended on November 28th, 1873, and she was therefore urged to come in at once, in order, if needful, to have premature labor induced. This advice, as I afterwards learned, frightened her, and she went home with the intention of staying out until labor set in. Meantime I daily expected her to enter the building, and on that account kept putting off my summer vacation. On the afternoon of August 13th, my friend Dr. Jacob Roberts, who had kindly undertaken to look after my patients during my absence, called

to ask why I did not go away. While I was explaining to him the cause of my stay in town, the door-bell rang and the woman herself came in. Finding her in labor and the os well dilated, I at once impressed Dr. Roberts into service and put her under ether. The vagina was somewhat narrowed by a cicatricial band. The conjugate was over three inches in length, but under three and a half. A closer measurement than this I cannot give, because I unfortunately neglected to verify my finger measurement by a carpenter's rule. The pelvis was uniformly contracted; yet there seemed room enough in the bisiliac diameter to admit the occipito-frontal diameter of an average child's head, and I accordingly decided to turn. As soon as the arms were brought down, Dr. R. made suprapubic pressure with all his might, while I threw on the child's neck all of my weight possible in a bent posture. A distinct snap of some cervical structure was felt and heard. In less than it has taken me to describe the process, the head bounced out of the grip of the brim. After a short but anxious detention at the cicatricial band, a living and lusty child was born. It weighed five pounds six ounces, and, apart from a slight depression on the side of its head, appeared none the worse for the rough treatment it had received. As soon as the mother had recovered her senses, and had heard the cries of her child, she seized my hand in both of hers and covered it with kisses. I mentioned this incident, not from any lurking feeling of sentiment, but from a wish to show how vivid was the memory of her former labor, and how keen was her sense of gratitude. The extractive power applied to this case, I rate at not less than 200 pounds. Of this amount Dr. Roberts exerted about 90 and I not under 110 pounds. The necessity for this great force was owing, not so much to the narrow conjugate, as to the general contraction of the pelvis. The mother's convalescence was so prompt that on the fifth day she was dressed, and on the twelfth she went out as a wet nurse.

CASE IX.—Since this paper was read before the Society, the following case has happened to me. Late in the summer of 1873, E. O'N. æt. 32, fell into labor with her first child. Dr. James F. Wilson, being in attendance, put the forceps accurately on the sides of the child's head, and, for an hour, pulled away in vain. He then sent for Dr. J. S. Parry, who arrived

at 5 o'clock P.M. After trying for several hours to deliver her with the forceps, they succeeded finally at midnight, after a tedious craniotomy operation. I give the names of these gentlemen as a sufficient voucher to the members of this Society that the fullest toil of service was exacted from the forceps. The woman's water had to be drawn off twice daily for two weeks thereafter, and her convalescence was a slow one.

On the 10th of February, 1875, she again fell into labor, and Dr. Wilson asked me to see her. We found the expulsive pains very severe, the water drained off, the os dilatable, the head movable, and the vertex to the left ilium. A very careful examination gave a diagonal conjugate of 3.5 inches, making the true conjugate 2.82 inches in length. At 10.30 o'clock P.M. the forceps was very accurately applied by Dr. Wilson to the sides of the head. For precisely one hour he and I, by turns, tugged faithfully at the handles without gaining the slightest descent. In order to get the full benefit of the woman's expulsive efforts, ether was withheld. At 11.30 o'clock the forceps was removed, ether now given, and the child turned. Very unfortunately the feet came down astride of the cord. This mishap made me hurry up matters, and in my haste to deliver the pubic (right) arm I broke the clavicle. Dr. Wilson, who was supporting one of the woman's knees, made strong supra-pubic pressure with one hand, while I put on the neck a few pounds more than half the weight of my body. Within two minutes after the arms had been brought down, and before the clock struck twelve, the child was born. It was asphyxiated, but soon began to cry, and weighed eight pounds six ounces. Adjacent portions of the frontal and parietal bones above and in front of the left ear were greatly flattened. In the centre of this area was a spoon-shaped depression with probable fracture into which I could lay my thumb. Fourteen hours later Drs. Wilson and R. G. Curtin met me at the house of the patient. She was doing well and had passed her water. In spite of its concavo-convex head, the child seemed lively enough. At that visit I measured the diameter of its head at the site of the fracture. Although the bottom of this lesion had now become very nearly flush with the flattened surface, and the latter had very appreciably rounded out, the calipers gave a measurement of 3.15 inches. A smaller



measurement than this could have been got, for the bones were very yielding, but the child winced at the gentle contact and I did not dare to use the slightest compression. The clavicle was dressed by a pad in the axilla, and by three adhesive straps. By the evening of the 27th inst., the bones were so well knit that all the dressings were removed. At this date the left side of the head was still more flat than its fellow, but the difference was not so marked. With the exception of a strain of the right hip-joint, caused from spreading the thighs too widely while the forceps were in use, the mother did well.

Honesty demands that I should now relate a case of failure to deliver by version. It is thus far my only one. C. K. was for the first time delivered in December, 1873, by a skilful physician, after an extremely difficult forceps labor. The child was dead; its face and skull so crushed in that the husband would not permit his wife to see the body. Becoming again pregnant, she fell into my hands. At 10 o'clock p.m., December 19th, 1874, labor began. At 7 a.m. of the next day, the waters gushed away from a well-dilated os. Yet after an hour and a half of strong pains the head did not offer to engage, but remained perched so high up that, by introducing two fingers, I could just touch it. The woman begged so hard not to be anæsthetized, that I was weak enough to yield to her wish. The parts were consequently too sensitive to permit accurate measurements. Misled by finding the conjugate fully 3.5 inches in length, I overlooked a uniform contraction of the whole pelvis, and did not discover my mistake until it was too late to mend it. The head lay in the first position of the vertex, with the posterior fontanelle so high that the introduction of four fingers was needed to reach it, and yet it was lower than the anterior fontanelle. By burying the lock in the vagina, the forceps was readily applied obliquely, but the depth of the pelvis and the woman's resistance prevented an exact biparietal application. Strong traction for half an hour doing no good, the child was turned. The half-breech was dragged through the pelvis with so much resistance, that it augured badly for the safe extraction of the head. For half an hour before the brim was passed, I hung on the neck all the weight I dared, fully 130 pounds. The trouble lay in the want of the room in the bisiliac diameter. The head neither flexed nor rotated, al-

though the child being now dead, due moulding time was given in the intervals of traction. After the lapse of another half hour I felt the spinal column give way, and, therefore, opened the skull behind the right ear. As soon as the brain was broken up, one finger was hooked into the opening and the head turned out with so little trouble, that it was by far the easiest case of craniotomy I ever had. The child, a boy, less a few drachms of brain, weighed eight pounds eight ounces; its head was large and firm.

It is worthy of note, that in this unsuccessful case, as well as in the ten preceding successful ones, and also in others which I have not reported, all the women, excepting the one with eclampsia and the one attacked by peritonitis, recovered as promptly from the effects of the labor as if it had been a natural one. Further, in no single instance that I can recall was the catheter needed to empty the bladder.

From the foregoing cases it will also be seen that not only were infants very speedily born through narrow pelvises; but that some of them were born alive after very powerful traction upon their necks, and indeed, after the forceps had failed. Five questions at once suggest themselves: What amount of traction on a child's neck is compatible with life? What amount, without decollation? What amount of extractive power can be brought to bear upon the child as a whole? What is the mode of traction? What is the limit of conjugate narrowing through which an average-sized head can be made to pass?

In a paper on the "Management of Head-Last Labors," which was read before the Philadelphia County Medical Society (*Philadelphia Medical Times*, March 20, 1875, p. 385), and of which this one is a pendant, I stated that a strong opponent to version, Matthews Duncan, had found the neck of a mature infant capable of sustaining an average weight of 105 pounds before the spinal column yields, and one of 120 pounds before the body parts from the head. That these averages seemed to me to be underrated, for, from a series of experiments, I found that, while I rarely exerted a force of over 100 pounds, I had on several occasions delivered living children after throwing on their necks a weight of 130 pounds. I further showed that Joulin, also a warm partisan of the forceps, after putting on the feet of three dead children a steady traction power, re-

spectively of 125, 145, and 148 pounds, had dragged their heads through an artificial pelvis without breaking their necks. In that paper I cited a remarkable instance, in which a well-known physician of this city, by bracing his feet against the woman's person, had exerted his utmost strength on the neck of a child without any lesion whatever.

Twice have I delivered living children, and one of them actually lusty, after using so much force as to cause a very audible snap of some cervical fibre—an experience which tallies with that of Braxton Hicks, Steele, and of others who took part in the discussion on Dr. Duncan's paper (*British Medical Journal*, September 19, 1874, p. 384). Although exerting all the manual strength at my command, I have never seen the body part from the head. Nor in three instances, including one of breech-presentation, was there the slightest appreciable injury to the neck, although the sacral side of the head had been broken in. On one occasion only had the perforator to be resorted to, and, as before stated, it was by far the easiest operation of the kind that I ever performed. Further: it must not be overlooked that the neck of a living child is presumptively stronger than that of a dead one. For aside from the vital contractility of the muscles and the ligaments, there is on the part of the child, as my friend Dr. Isaac S. Eshleman has called to my attention, an invariable resistance to traction, whereby a very marked shrinkage takes place in the muscles of the neck.

By a series of experiments with his dynameter, Joulin<sup>1</sup> found that, without any brace for the feet and by pulling with the muscles of the arms alone, a robust (*vigoureux*) man can exert on the forceps a force of 113 pounds. When the feet take a purchase on the floor, a force of 150 pounds may be reached. When the physician braces his feet on the edge of the bedstead, a force of 225 pounds is attainable. Including even this last mode of traction, which, if prolonged, is hardly ever compatible with the life of the child or with the integrity of the mother's tissues, the conclusion is inevitable, that, by the conjoint use of two very nearly equal forces, viz., that of suprapubic pressure by the hands of an assistant, and that of traction

<sup>1</sup> *Traité Complet d'Accouchements*, p. 1063.

on the body of the child by the physician, there can be safely brought to bear upon the hind-coming head an extractive force fully as great as that by the forceps on the fore-coming head. Thus, in Case VIII. Dr. Roberts and myself together exerted a force of certainly not less than 200 pounds. In Case IX. Dr. J. F. Wilson's *vis a tergo* and my *vis a fronte* must, unitedly, have equalled fully 150 pounds. For want of a better place, let me here say that the supra-pubic pressure possesses another helpful property besides that of propulsion. If directed downward and backward, as it should be, it flattens the head bilaterally against the sharp edge of the promontory, and aids in the process of moulding.

If now, to an extractive force fully equal to that of the forceps be added the great mechanical advantage gained by getting the small end of the wedge, viz., the cranial base, to enter the inlet; and also that gained by the engagement in the conjugate of the small and very compressible bitemporal diameter; and if to these very substantial advantages be superadded the telling mechanism of delivery in version, by which only one small portion of the skull is compressed, and that where most needed, viz., in the offending diameter itself of the child's head, while by the forceps the compression is diffused over large surfaces, on unimplicated diameters, and consequently in more or less direct antagonism to the line of compression made by the symphysis and the promontory—it follows that the weight of argument in favor of the manual over the instrumental operation is overwhelming.

Notwithstanding the foregoing facts show the wonderful tensile strength of the foetal neck, and the great amount of force that can safely be used to extricate the after-coming head from the grasp of the brim, it is of importance to exert the power to the best mechanical advantage, and to grade it to the resistance. This brings me to the mode of making traction. But since the question of version in narrow pelvis hinges on that of the management of head-last labors, I shall take the liberty of quoting somewhat freely from my paper on that subject.

In the first place, the woman's hips should be brought slightly over the edge of the bed, and each leg supported by an assistant.

My reasons for this position in preference to the lateral one are : That supra-pubic pressure is then better made by the hands of a third assistant, or by the free hands of the two assistants. That very few physicians, while bending forward in front of the woman thus placed, can exert a steady force of one hundred pounds upon the neck of the child ; and, finally, that the upper hand of the physician can then force the neck into the hollow of the sacrum, and thus make the line of traction somewhat behind the axis of the superior strait.

In a brim narrowed in its conjugate, the sacral side of the after-coming head is bent in and fixed by the jutting promontory. Hence, as Barnes has shown, the extrication of the head as a whole can take place only when its pubic side revolves around the promontory and descends over the smooth undersurface of the symphysis of the pubes ; in other words, the head must be warped around the promontory. Bearing this fact in mind, it is important after version, that the sacral side of the head should be caught at a point as near as possible to its vault. To gain this end, the physician, after grasping the nape of the neck with one hand and the ankles with the other, should make his first movement of traction in the axis of the outlet. For then the pubic side of the head will be tilted away from the inlet, while the sacral side will proportionally descend over the promontory and affront the brim. This canting of the head can be very materially aided by the free hands of the assistants, who will make very firm downward and backward pressure upon the vault of the head through the now flaccid abdominal walls. By these manœuvres the sharp promontory is made to indent the sacral side of the head at a point still higher up and nearer to the vault. Hence, the arm of the lever, measured by a line drawn from the base of the skull to this fixed point, will be correspondingly lengthened—a mechanical advantage not to be overlooked. If now, *without for a moment remitting but rather increasing the traction*, its direction be reversed, and the child's body be swept backward upon the coccyx—the neck being also forced downward and backward into the hollow of the sacrum with all one's power—the sacral side of the head becomes bent in, and the pubic side is made to revolve around the promontory, and descend with the least expenditure of power. Apart from the leverage thus gained, and the shorter are thus



described around the promontory as the centre of motion, I am not sure but this manœuvre will, in very narrow pelves, so cant the cranial base as to get it below the sharp edge of the promontory. This much I can affirm: that by this method less power is needed to deliver average heads in narrow pelves than large heads in average pelves. For, in the former, the resistance is limited to a single osseous point; in the latter, diffused over the whole bony brim.

Whenever this mode of traction fails at once to release the head from the grip of the brim, or the difficulty lies rather in the size of the head than in the narrowness of the pelvis, I have on several occasions, especially in original head-last labors, found a pump-handle movement very efficient. Made with steady and unremitting traction, and aided by supra-pubic propulsion, it will cause each side of the wedge-shaped head to descend alternately. The range of oscillation should extend from the axis of the outlet anteriorly, to very firm pressure on the coccyx posteriorly. With a very sharply-jutting promontory this up-and-down movement does not usually succeed, unless the parietal bone has been depressed as a whole, or broken in, and not simply indented. Otherwise the sacral side of the head is held fast, and the pubic side will then librate around the indented, and therefore fixed point, merely rising and falling without any onward progress whatever.

As soon as the unflexed head has passed the brim, which it usually does with a well-marked jerk, it is brought into relation with new pelvic diameters. Flexion and rotation now spontaneously take place, and the line of traction must be changed to that of the outlet. And when, finally, the head is about to clear the bony canal, the body of the child should be raised up in front of the pubes, and traction made in a line at right angles to the mother's body.

That the spoon-shaped depressions or even fractures of the sacral side of the child's head are not very dangerous lesions, not only do my own cases show, but also those reported by Simpson, Schroeder, Danyau, Depaul, Blot,<sup>1</sup> and by many others, the enumeration of whose names would make too long a bead-roll. They are by no means so fatal to life as a lack of promptness on the part of the physician in making efforts at delivery.

<sup>1</sup> Archives Générales de Médecine, July, 1863, p. 25.

I have, however, taken for granted that he does not err in this respect. So long as the cord beats, or other signs of life are present, there must be no remission whatever in the extractive efforts of the physician. But if, after the head has become well jammed into the brim, the child is found to be dead and its delivery proves to be a difficult one, due moulding time may be given.

The limit of conjugate narrowing through which the after-coming head of a mature infant may be dragged cannot be easily determined. Here theory must give place to bedside experience. Numerous are the cases on record in which turning succeeded after the forceps had failed. Simpson<sup>1</sup> turned and delivered dead children through pelves measuring 2.5 inches and upward in their conjugate diameters. Others have been equally lucky. Schroeder<sup>2</sup> extracted living children through conjugates of 2.8 inches; I myself through one of 2.82 inches; and Blot<sup>3</sup> through one of three inches, in which Dubois had previously been driven to the cephalotribe. On one of the museum shelves of the Bellevue Hospital Medical College I saw the fractured skull of an infant which Professor Isaac E. Taylor had dragged through a pelvis measuring 2.75 inches. To the above I would add Madame Lachapelle's<sup>4</sup> cases in conjugates respectively measuring 2.75, 2.5, 2.25, and 2.2 inches, were it not that I distrust the accuracy of that celebrated midwife's statements.

Now, so far as I can judge from the history of these cases, none of the operators, excepting myself, invoked the very substantial help of a propelling force; hence the inference is logical that the conjunction of traction and propulsion offers better results. Thus, in Case VII. these two forces delivered a head through a strait narrowed to 2.5 inches, and bounded at each end by bony projections. From theoretical and clinical deductions two broad rules for guidance in turning may, I think, be laid down: first, that the brim must be narrowed mainly in its conjugate; and, second, that a brim which can admit the unyielding base of the skull is traversable by the crown. With

<sup>1</sup> *Obstetrical Works*, vol. i., p. 450.

<sup>2</sup> *Manual of Midwifery*, Am. ed., 1873, p. 262.

<sup>3</sup> *Archives Générales de Médecine*, July, 1853, p. 19.

<sup>4</sup> *Pratique des Accouchements*, vol. iii., 9th Memoir.

regard to the first rule, it is self-evident that there must be room enough in the bisiliac diameter of the pelvis for the occipito-frontal diameter of the unflexed head to pass. This diameter is a very hard one to measure; but transverse narrowing may be inferred whenever a strongly flexed head lies obliquely, and yet does not engage or descend. In conjugate narrowing, on the other hand, the head lies transversely, and so extended that the anterior fontanelle sinks lower than the posterior one. The second rule is based on the presumption that the lateral portions of the skull lying above the hard base, being flexible, are reducible to the width of the base. The portion of the base which, according to my observation, first enters the conjugate, and impinges on the promontory, is the hollow lying directly in front of the external meatus of the ear. The diameter of the base at this point ranges in length from 2.50 to three inches; and to 2.75, their mean, as a minimum, would I, therefore, restrict the operation of version when the infant is mature; provided always that the head is presumptively an average-sized one. Should the head prove to be a small one, a living child may possibly be delivered. If the head be so large that its base cannot enter the brim, then the worst that could happen would be a final resort to craniotomy or to cephalotripsy—measures which would have been initial had the cephalic presentation been retained.

Whether, in these transverse cranial positions, turning has advantages over the application of the forceps to the sides of the child's head, is a question which I have thus far purposely avoided, but on which I now invite discussion. The cases of mature children delivered by the forceps through conjugate diameters not over three inches in length may be counted on the fingers. In straits narrower than this no one has, to my knowledge, ever succeeded. Its effective range, as ordinarily applied, is from a diameter of 3.50 inches to that of a standard pelvis. A diameter of even 3.50 may compel a resort to the perforator. But, while numerous facts seem to prove that in very narrow pelves the forceps cannot compete with version, I am sure that its range of usefulness can be very materially widened by that cephalic application of the blades, viz., to the sides of the head, which is practised by the best obstetricians of this city. Such an application in these transverse cranial positions, by compel-

ling the greater equitation of the parietal bones, and by shortening the offending lateral diameter of the head, lessens the risk to the mother's tissues, and the power needed for extraction. Also, by compressing the least vulnerable portions of the head, it is less likely to cause fatal brain lesions. Yet, even then, the pressure on the mother's tissues is a prolonged one. At best it usually fails to deliver living children through pelves narrowed down to 3.25 inches; while at three inches labor must very generally be ended by craniotomy. Again, in so far as the question of version is concerned, the majority of the arguments urged against the pelvic application of the forceps, holds good against its cephalic application.

Let me not be misunderstood. By no means do I cast off such an old and tried friend as the forceps. It has served me too many good turns to be so illy treated. Cases there are in which turning cannot or should not be performed. Again, the urgency after that operation for immediate delivery is a strong argument against its indiscriminate use. To put these thoughts into practical shape, I offer the following general propositions:—

1. Turning should generally be preferred to the lashing of the forceps handles.

2. In pelves uniformly contracted the forceps is the better means of delivery.

3. In pelves narrowed in the conjugate diameter, turning should be resorted to whenever a half-hour's faithful trial with the forceps fails to make the head engage.

4. In pelves whose conjugates range from 2.75 to 3.25 inches, turning should be the initial step.

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THE RELATION BETWEEN ALIMENTATION AND THE GASTRO-  
INTESTINAL DISORDERS OF INFANTS AND YOUNG  
CHILDREN.

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With not a little hesitation I have ventured to add another to the already numerous papers that have been written on a class of disorders we are all so frequently called upon to attend. My excuse, and, I trust, my justification, also, for so doing, lie in the conviction that much is yet to be learned in regard to the gastric and intestinal diseases peculiar to infancy, and that the presentation of views and conclusions resulting from experience and study may, perhaps, either by their own merit or in the resulting discussions of them, be the means of enabling us to more successfully prevent and alleviate a class of disorders that yearly reaps so large a harvest for death.

As the object of this paper is to furnish, if possible, information which, it is hoped, may be instrumental in saving infant life, a brief introductory reference to the subject of mortality in infancy will not be out of place.

It is admitted by those who have looked into the subject that evidence exists to prove that at least one-fourth of all children born end their existence before their fifth year.<sup>1</sup> According to the returns of the Board of Health of Philadelphia,<sup>2</sup> there were born in that city during the five years ending Dec. 31, 1870, 85,957 living infants, of whom 25,636 died before they were two years old, and a total of 31,662 before their fifth year—a mortality of a little more than 36.83 per cent.

<sup>1</sup> Practical Treatise on the Diseases of Infancy and Childhood. By Thomas Hawks Tanner, M.D., London, 1871.

<sup>2</sup> Infant Mortality, etc. By J. S. Parry, M.D., Philadelphia. Paper read May 5, 1871, before the Social Science Association.



In England, according to a high authority,<sup>1</sup> statistics show that fifteen infants in every hundred die in the first year of life; and the statistics of the continent give the same results as to the period of greatest mortality. In this city 53 per cent. of the total number of deaths occur under the age of five years, and 26 per cent. under the age of one year. In fact, it may be safely stated that one-fourth of the children born in this city die before the age of five years.<sup>2</sup> As to the causes of this great mortality, the purpose of this paper does not admit of my elaborating all of them. It is considered by some that the natural fragility of the infant is sufficient to account for a great portion of the premature mortality; but we will dismiss that idea at once. "It certainly cannot be supposed that the Creator intended that all these little ones should suffer and die. There is every proof to the contrary. Has He not wisely and beneficently adapted all their physical wants to the circumstances in which they are supposed to be naturally placed? Has He not surrounded them with every condition and requirement necessary for the maintenance of life and health? For the very preservation of life, has He not ordained certain natural laws to be observed, the neglect of which necessarily brings disease and premature death? When, however, we reflect upon the amount of ignorance and neglect to which infants are subjected, our surprise must be that the death-rate is not higher."

Allowing that some of the causes of infant mortality are and always will be more or less unavoidable, yet it will be conceded that the greater number of deaths must be due to causes largely within our power to remove. If we look into some of the statistics as to the causes of death in infancy, we will find that affections of the respiratory and digestive organs destroy the greater portion. Of 9,873 children who died in Massachusetts in 1870 under five years of age, more than one-half of the deaths were due to affections of the digestive organs.<sup>3</sup> Of 11,382 children under twelve years of age treated at the Free Dispensary for Sick Children in this city during the past three

<sup>1</sup> A Treatise on the Diseases of Infancy and Childhood, by J. Lewis Smith, M.D., etc., 1872, p. 23.

<sup>2</sup> Op. cit., p. 24

<sup>3</sup> The Proper Treatment of Children. Discourse before Mass. Med. Society, 1873. By Charles E. Buckingham, M.D., etc., Boston.

years, 3,243 were suffering from affections of the organs of digestion. It will not be supposed for one moment, even by those ignorant of medical knowledge, that very many of the above cases were due to congenital or unavoidable causes; but, on the contrary, such figures most convincingly prove that the great prevalence of digestive disorders in infancy and childhood must be due to causes arising from ignorance and carelessness in their management, especially as to their alimentation.

In support of this statement let me offer a few figures. In 1870 Dr. E. Ballard, of England, published a very thorough article on Infant Mortality.<sup>1</sup> He took as the standard of natural mortality the statistics of the Society of Friends. During five years (from 1863 to '68) there occurred in the families of that society only 147 deaths of infants under one year of age, against 1,323 births during the same period, giving a death ratio of 11.1 per cent. during the first year of life. In this society domestic training requires care being bestowed upon the young. "If," Dr. Ballard says, "we may accept this ratio as a normal death-rate under favorable conditions, we arrive at this conclusion: that the infant death-rate of England is nearly half as large again as it ought to be; and have next to inquire how it is that, in the same five years referred to, 164,190 deaths happened more than there was due and proper cause for happening. Were it possible to make a strict inquiry into the circumstances of the death of every infant, it is scarcely to be doubted that, among those whose deaths we are now going to enumerate, would be found the items which go to make up the excess.

Deaths from causes not specified.....	12,458
Sudden deaths, causes not ascertained.....	3,879
Deaths from violence.....	8,398
Want of breast-milk.....	6,595
Diarrhœa.....	54,274
Thrush.....	5,060
Atrophy and debility.....	107,825
Teething.....	10,459
Convulsions.....	105,294
Total.....	314,242

<sup>1</sup> British and Foreign Medico-Chirurgical Review, April, 1870.

Considering what we know concerning the causes of thrush, atrophy, teething and convulsions, we will not be considered extravagant in asserting that half of these 314,242 deaths ought not to have occurred at all; that they represent roughly, for the five years referred to, the amount of unnecessary destruction of infant life due to active or passive *criminality, ignorance and carelessness.*"

Even if we take from the above figures those deaths due to disorders of the digestive organs and insufficient alimentation, and accrediting, as, in my opinion, it is but right to do, only one-half of the deaths from thrush, atrophy, teething and convulsions to the same class, we still have, out of the 314,242 deaths, more than one-half, or 175,208 of the infants destroyed by gastro-intestinal disorders.

In a discussion upon the mortality of infants before the Académie de Médecine of Paris,<sup>1</sup> in 1857, M. Guérin stated that there were two great classes of facts upon which the excessive mortality of children depended—*improper* and *deficient* nourishment. After twenty-five years of observation, M. Guérin stated that he had arrived at the conclusion that the chief of these causes was premature feeding. By this he means a diet which is too substantial, and not suited to the age of the infant—an age when the digestive organs are not qualified for receiving any other kind of nourishment than milk. The effects of such feeding, he claims, influences the development of *tabes mesenterica* and rickets, and produces a state of constitutional debility which renders the child liable to disease. All such children do not die; some escape all its dangers; but experience proves the lamentable results of such defective management of children, not only in the decrease in numbers, but also in a degeneration of the race.

At the session of the Imperial Academy of Medicine held in Paris on October 16, 1867, a memorial was submitted by Dr. Monot on the increased mortality of children in France.<sup>2</sup> The chief cause, he found, lay in deprivation of the infants of their natural food, on account of so many mothers going out as wet-

<sup>1</sup> Archives Générales de Médecine. Février, 1857.

<sup>2</sup> Archiv für pathologische Anatomie und Physiologie und für klinische Medizin, 1867.

nurses, leaving their children to be artificially fed. At the same meeting Dr. Bondet also stated that in a large part of the empire three-fourths of the new-born children died in their first year, in consequence of the improper and bad manner of bringing them up.

The foregoing brief remarks seem to me to be sufficiently convincing that gastro-intestinal disorders are chief amongst the causes of the enormous death-rate occurring during infancy, and that the origin of such diseases must be looked for in a faulty and vicious alimentation, arising from the ignorance of parents as to the requirements of their offspring, and a deficient or faulty knowledge of the physiology of infant digestion and assimilation, on the part of people in general.

During the past five years, in my connection for the greater portions of that time with the children's classes at the New York, the Demilt, and Children's Dispensaries, as attending physician, and as assistant to the Clinique for Diseases of Children in the College of Physicians and Surgeons, I have seen a very large number of cases of gastro-intestinal disorders; and, looking back upon such experience, as well as that obtained in my private practice, I cannot avoid stating that, in by far the greater number, faulty alimentation lay at the "root of all the evil," and those cases were very few indeed in which no such cause existed. The report of a committee appointed in 1869 by the Obstetrical Society of London<sup>1</sup> supports this statement. It was found by that committee that errors of diet, even in the nursling, were general and most flagrant; and the report concludes: "Under these circumstances it is little to be wondered at that bowel and intestine disease, followed by general wasting, is so alarmingly prevalent. Indeed, this question of improper feeding must be held accountable for a very large proportion of the excessive infant mortality which prevails."

So forcibly, indeed, is this experience impressed upon me, that I have but little hesitation in saying that much rather would I undertake the treatment of the majority of cases of gastric or intestinal disorder in infants without being allowed the use of a single article in the materia medica, than be withheld the entire control of the alimentation of the infant. This

<sup>1</sup> Transactions of the Obstetrical Society of London, for the year 1869 Vol. xi., p. 137.

may, perhaps, seem a somewhat hazardous assertion to those who are accustomed to place their greatest reliance in drugs, but to the latter I wish to say that I do not deny that with drugs alone we can accomplish much, but that by attention to alimentation we will accomplish more.

If, therefore, it is a fact that the majority of the cases of gastro-intestinal diseases are the immediate or subsequent results of faulty alimentation, chiefly through a prevailing ignorance as to the proper requirements of the infant's stomach, we cannot be too zealous in our endeavors to combat the misfortune that the young of the human species are not "endowed from birth with the power of distinguishing good from injurious feeding. There the young of the lower animal has the advantage. He is fed on natural food only, from the beginning, because his father and mother don't know enough to kill him. Man, the reasoning being, is defeated by the animals, who possess instinct only."<sup>1</sup>

In refutation of the preceding statements, the argument may be used that, as the majority of infants are fed during the first months of their life upon the food intended for them by nature—breast-milk—how can faulty alimentation be the cause of gastro-intestinal disease in such? In answer to this question, and as initiating the proper subject of this paper, I will take up the consideration of the common gastro-intestinal disorders of the infant at the breast; and, in the ensuing remarks, I wish to be understood as excluding the few cases due to congenital diseases, my remarks having reference only to that larger class in which functional disease cannot be accused of the above evils.

From birth to the eruption of the teeth, Nature supplies the infant with material for its sustenance that is in perfect harmony with the needs of its economy, namely, its mother's milk.

According to Vogel<sup>2</sup> the composition of healthy woman's milk is as follows:

Specific gravity.....	1,032.
In 1000 parts it has of	
Water.....	\$89.08
Sugar.....	43.64

<sup>1</sup> The Proper Treatment of Children. Discourse before Mass. Medical Society, 1873. By Charles E. Buckingham, M.D., etc, Boston.

<sup>2</sup> Practical Treatise on Diseases of Children, American edition, 1871, p. 32.



Casein.....	39.24
Butter.....	26.66
Salts.....	1.38

According to Bouchut:

	At 1st month.	At 24 months.
Specific gravity.....	1033.11	1030.81
Water.....	872.99	876.55
Solid constituents.....	127.61	123.45
Sugar....	43.13	41.33
Butter.....	34.05	41.47
Casein.....	48.26	37.32
Salts.....	1.57	1.33

Simon found by analyses made every ten days of healthy nurses for six months, that, at the commencement of nursing, casein is at its minimum, then rises to nearly a fixed proportion, the salts increasing with it. On the other hand, sugar, at the commencement of nursing, is at its maximum, and then gradually diminishes. Butter he found variable. These analyses agree with those made by Vernois, Becquerel and others. Peligot and Reiset found more fat generally towards the close of lactation.

Accepting the foregoing as giving us the exact composition of the proper food of the nursing, we will find that the infant digestive organs and its power of assimilating food of such a character are in perfect accord with the latter.

Dr. Routh, quoting Burdach and others, in his excellent work on Infant Feeding, says: "It is remarkable that suction is the only faculty for the prehension of food which the child possesses at birth. There is, moreover, no saliva secreted for the first two months, so that no species of preparatory change can take place in the month, as, for example, in the conversion of starchy matters into sugar, through the agency of the saliva. The stomach in infants is a small, tube-shaped membrane, dilated in the centre, one extremity ending in the œsophagus and the other in the pylorus, resembling in this character that found in carnivora through life. In position, also, it lies more parallel to the trunk; the large and small curvatures and muscular structures being but very little developed. The liver at birth is unusually large; the pancreas, perhaps, not more developed

than the salivary glands ; the intestinal tube is shorter, and the large intestine approaches more nearly in its length to the small. The cæcum is very small ; the peristaltic motion is more rapid. All these are evidence that food taken will be kept for a shorter time in the canal, and, therefore, should be in the condition most favorable for digestion. Lastly, in no other of the mammalia is there, in the first periods of life, such complete absence of teeth. In man they appear latest and are longest in obtaining their full development." If we compare these facts with what we find in herbivorous animals, we will see that they are provided with more fully developed and perfect organs, fitted for a long period of digestion. All this, therefore, goes to prove that animal, not vegetable, that fluid, not solid food, is the proper nourishment for an infant, and that the liquid food received into the stomach must be in such quantity and possess such qualities and received at such intervals as to admit of its easy digestion and rapid assimilation. If, therefore, these requirements are not fulfilled, it necessarily follows that gastro-intestinal disorders of some kind will probably occur, which, if the evil be not corrected, from a slight ejection of undigested milk, will lead to chronic vomiting, enteralgia, constipation, intestinal catarrh, etc., which, in their turn, will lead to wasting, convulsions and death.

The quantity of breast-milk required by the infant, it is perhaps somewhat difficult to decide, for it is true that both the capacity and demands of nurslings vary. Routh<sup>1</sup> estimates the amount taken by a healthy child at three months to be about half a pint, while a younger child will consume not more than three ounces every meal. It, however, is an exceedingly easy matter to judge, from the manner of the child's sucking, when its appetite is appeased, and so soon as this is recognized the nipple should be withdrawn. As Eustace Smith most truly says : "The great principle at the bottom of all successful feeding, viz., that an infant is nourished in proportion to his power of digesting the food with which he is supplied, and not in proportion to the quantity of nutritious material which he may be induced to swallow, is so obviously true that an apology might almost seem to be required for stating so self-evident a proposition ;

<sup>1</sup> On Infant Feeding, page 87.

but experience shows that this simple truth is one which in practice is constantly lost sight of. That that infant thrives best who is most largely fed, is an article of faith so firmly settled in the minds of many persons that it is very difficult indeed to persuade them to the contrary. To them wasting in an infant merely suggests a larger supply of food: every cry means hunger, and must be quieted by additional food.”<sup>1</sup>

We must be careful therefore that the milk is not given in too large quantities, or that the meals are not repeated too often. If the stomach is kept constantly overloaded, the effect is almost as bad as if the infant was fed upon indigestible food. Most infants, however, cease nursing the moment their appetite is appeased, only those taking more than they require who are exceedingly greedy naturally, or suffer from dyspepsia. There is one positive guide, however, as to the quantity of milk requisite for an individual infant, which, from my experience, is generally seen in over-nursed infants. It is ejection and vomiting of milk after nursing, and which is ignorantly considered by very many to be the sign of a healthy child, and therefore should not be interfered with, when it in fact denotes over-feeding, and is the effect of reflex action, excited by an over-distended stomach; Nature thus protesting in language most plain against the greed of the hearty infant and the ignorance of its mother.

As my experience has taught me, most infants who thus throw up after nursing suffer also, sooner or later, from enteralgia and constipation, and other symptoms of indigestion, which latter are only permanently relieved when the greed of the child is restricted. Such vomiting, it is true, may be due also to some fault in the mother's milk, but this is so very rarely the case in women who are in good health, that it hardly need be mentioned, for the observing physician will always see that the mother's health is up to the standard required for good nursing. In regard to this matter, Simon says: “However much the nutriment of the mother may vary, no great influence is thereby exerted on the relative quantities of casein and sugar. The changes consist in a greater or less degree of saturation, in the rich yellowish-white or bluish color, in the quantity of the

<sup>1</sup> Hand-Feeding of Infants, by Eustace Smith, M.D., etc., London Sanitary Record. Also The Sanitarian, New York, January, 1875.

milk and in the solid constituents; with the exception of the variation in quantity, all the other changes are dependent on an increase or diminution of the butter. The former occurs under the use of a copious and nutritious diet, the latter when the diet is poor and scanty.”<sup>1</sup>

It may therefore be safe to say that a healthy woman secretes healthy milk, and that the most variable ingredients in breast-milk is the butter, the least likely to disagree with the infant.

The vomiting of milk by a healthy infant, nursed by a healthy woman, is, my experience has taught me, in most all instances due solely to an excessive quantity of food, and when a limitation is placed upon the amount of the latter, will invariably at once cease. When I state that I have traced most of the cases of chronic vomiting, wasting, and simple intestinal disorders in nurslings directly to over-nursing, the importance of paying attention to apparently so slight a disorder may be appreciated; and by thus at once putting a stop to what ignorance considers a healthy sign, we will ward off the greater evils that are in waiting to follow.

The frequency of nursing the infant, also, has very much to do with gastric and intestinal troubles. On this subject, however, there is much diversity of opinion amongst mothers, nurses and physicians. By those who have studied the subject, and have had large opportunities for so doing, it is considered sufficient for the new-born infant to take the breast every two or three hours during the day, and then only if it is hungry, for if remaining quiet and apparently not hungry, I cannot see why it should be put to the breast “because it is time.” Once during the night is quite often enough, for it must be remembered that during sleep, digestion is not performed as rapidly as during wakefulness. Consequently, if the infant’s stomach is filled during the night several times, the fresh milk entering at each meal comes in contact with the semi-digested curds of the preceding meal, and thus must necessarily be curdled itself, and, on the infant falling asleep, remains longer in the stomach undigested than requisite, at last passes into the intestines, and thus gives rise to colic, anti-peristaltic action, morning vomiting and constipation, and, if the evil habit is persisted in for days

<sup>1</sup> Transactions Sydenham Society, vol. ii.

and weeks, will at last cause a gastric and intestinal catarrh of more or less severity.

I am quite sure I am not alone in my experience of having seen very many infants in whom the cause of a gastro-intestinal disorder was due solely to too much and too frequent nursing, and who were restored to health by remedying these abuses. And my experience in consequence has led me to believe that the majority of cases of gastro-intestinal disorders of even the severest types, occurring in the naturally healthy infant, fed by a healthy breast, are due to *too much* and *too frequent* nursing; and with perfectly well children I never lose the opportunity to impress upon the mother the importance of avoiding falling into these errors.

But supposing we are called in to attend a case which, from the above causes, is suffering from either vomiting, constipation, wasting or diarrhœa, or two or three of these disorders together. What are we to do? Are we to remove the child from the breast for certain intervals? It may be said if we deprive the already wasted body of nourishment for any length of time, will we not run the risk of losing our little patient? To these questions I reply, *Starve the stomach! give it rest!* Does not the vomiting or the diarrhœa tell us in the plainest language that the digestive organs need rest? Will we gain any benefit, I ask, from ejected or undigested food, even if it cause no severer disturbance? Certainly not; therefore, is it not better by far that not even so small an amount as only a teaspoonful of milk be taken, if not retained and assimilated? Put such children, therefore, according to the urgency of the symptoms, upon nourishment diminished both in quantity and frequency; and if pains be taken to carry out such practice, we will accomplish a cure more surely and much more quickly than can be accomplished alone by any article or combination of articles in the materia medica. In the one case we will permanently remove the cause of the disorder; in the other we will obtain but temporary relief.

The cases are not few that have come under my observation in which, for weeks and even months, various medicines had been administered to control a gastric or intestinal disorder in a nursing infant, but without success, not because the drugs were unsuitable, but because the overtaxed and exhausted digestive



organs were not allowed rest, on account of persistent efforts to make them do work they were unable to perform. Stopping all medicines in such cases, I allowed the digestive organs perfect rest for six, ten, or twelve hours, according to the urgency of the case, generally giving nothing but a teaspoonful of cold water every fifteen or twenty minutes, and when nursing was resumed allowing the child to suck for a very few minutes only; again nothing for four or more hours, and so going on alternately for a couple of days, when the normal amount of food was very gradually to be allowed. So successful has this method been with me, that, when strictly carried out, all nausea and vomiting will be found to cease at once, the small amount of nourishment taken will be retained and digested, and gradually the digestive organs will return to their proper duties, and the child be restored to health.

The constipation so common in healthy infants is also frequently due to excessive and too frequent nursing, and is the common accompaniment to the vomiting already mentioned. The explanation of this constipation is quite simple. The stomach being overburdened with food, and consequently overtaxed with work, each supply of milk, instead of being coagulated into fine and soft coagula, which are readily acted upon by the secreted pepsine, comes into contact with the semi-digested acid coagula of the preceding meal, and in consequence it is coagulated more rapidly than it should be normally, the coagula being larger and harder. Such masses, if not ejected by the stomach, pass into the intestinal canal but little or not at all changed by the digestive process, will impact together on contact, and from their size and dryness are with difficulty passed along the bowels, thus giving rise to constipation, colic, etc. The unusual volume and accumulation of such curds will not unfrequently also cause such an irritation of the alimentary canal as to give rise to an intestinal catarrh of more or less severity, and which may ultimately run into the severest forms of intestinal disease.

Many I know will ask, Do not these disorders depend as much upon too acid a condition of the secretion of the infant's stomach and the presence of too much casein in the breast-milk, as upon errors in the amount and frequency of nursing? That such may be the case in some instances I am aware, but my experience has led me to make such the exception. Is it

not far less likely that a healthy infant should have an abnormally acid gastric secretion, than that it should be rendered so by faulty alimentation, and that hard, undigested coagula are less the sign of an excess of casein in the breast-milk, than of impaired gastric power in the infant? The fact that such disorders have, in the majority of cases coming under my care, been permanently relieved by simply diminishing the frequency and quantity of the nursing according to the urgency of the case, certainly strengthens me in my conclusions as to their cause.

So much then as to the influence of excessive and too frequent nursing in developing gastro-intestinal disorders in healthy infants, who take their nourishment from healthy breasts. I propose next to say a few words on similar disorders occurring in the infant who, deprived for some reason of its natural nourishment, is nourished on artificial food, and also in children beyond the nursing age.

"The disastrous results of attempts to supply a substitute for human milk have brought the whole practice of hand-feeding into disrepute; but if a food be judiciously selected, with a correct appreciation of infant wants and an accurate estimate of infant powers of digestion, there is no reason why a child fed artificially, with judgment, should not thrive as well as one suckled naturally at its mother's breast. The food we select for the diet of an infant should be nutritious in itself, but it should also be given in a form in which the child is capable of digesting it, otherwise we may fill it with food without in any way contributing to his nutrition, and can actually starve the body while we load the stomach to repletion."<sup>1</sup>

No food can be capable of nourishing an infant unless it possesses heat-giving, fat-producing properties, and material for supplying a waste of the nitrogenous tissues.

As is well known, and as I have previously remarked, the infant digestive organs are meant to receive only liquid animal food that can be easily digested and readily assimilated. Any nourishment, therefore, which does not come up to these requirements will most probably cause some digestive disturbance. The great frequency, therefore, of gastro-intestinal disorders in

<sup>1</sup> On the Hand-Feeding of Infants. By Eustace Smith, M.D., etc. The Sanitary Record, London, Eng. Also The Sanitarian, New York, Jan., 1875.

hand-fed infants and weaned children is, according to my experience, to be attributed to neglect and ignorance of the requirements for digestion at such periods of life.

There is perhaps no question upon which the profession, as well as mothers and nurses, are more at variance than the character of the food to be given in place of breast-milk. One physician, mother or nurse will recommend cow's milk, pure; another, skimmed milk; another, the same diluted with a little water; another, water with a little milk; another, cream and water; others, milk with corn-starch, arrow-root, crackers, rice, barley, oatmeal, and so on "*ad infinitum*." And yet with all these varieties of nourishment infants suffer and die from wasting and gastro-intestinal disorders, and mother remains in ignorance as to the cause. Her child, weakly perhaps to begin with, is filled with a quantity of such food, which it is unable to digest. Its stomach and bowels, revolting against the burden imposed upon them, endeavor to get rid of the offending matter by vomiting and diarrhœa; a gastro-intestinal catarrh is set up, which still further reduces the strength; every meal causes a return of the sickness; the bowels are filled with fermenting matter, which excites violent griping pains, so that the child rests neither night nor day; after a longer or shorter time it sinks, worn out by pain and exhaustion, and is then said to have died of "consumption of the bowels." Such cases are very common in the experience of every physician who has seen much of the diseases of children, and when seen early enough may readily yield to appropriate treatment; but unfortunately it is too often the case that, instead of recognizing and removing the cause—the bad feeding—the child is dosed the harder and fed the more the weaker it gets, and at last dies, in spite of all that was done.

If, then, the improper character of the nourishment generally given as a substitute for breast-milk is the frequent cause of gastro-intestinal disorders, what nourishment are we to give to the hand-fed infant? Is there any one kind of nourishment that, properly prepared, can and does uniformly and perfectly take the place of breast-milk? These questions I will endeavor to answer.

"The most perfect food for children, the only one, indeed, which can be trusted to supply, in itself, all the necessary ele-

ments of nutrition in the most digestible form, is milk. In it are contained nitrogenous matter in the curd, fat in the cream, besides sugar and the salts which are so essential to perfect nutrition. The milk of different animals varies to a certain extent in the proportion of the several constituents, some containing more curd, others more cream and sugar; but the milk of the cow is the one to which recourse is usually had, and, when properly prepared, is perfectly efficient for the purpose required.”<sup>1</sup> The important difference between woman’s and cow’s milk is to be found in this, that the casein of woman’s milk, of which there is about thirty-eight parts, curdles in the stomach into small, light flakes, forming a readily digested substance, while cow’s milk, containing sixty-eight parts of casein, the latter coagulates into large, compact lumps, which the infant gastric juice is incapable of dissolving.

In consequence of this peculiarity of cow’s milk, it is unsuitable in its natural state as an article of diet for infants, but efforts have been continually made to render it suitable by the addition of such articles as I have already mentioned. The chief amongst these, or the one most generally used, is water. The object of adding which to the milk is to dilute the excess of casein, and thus render it more readily digestible, and on this sort of slops many infants do manage to get along, but by far the greater number suffer in consequence. If we look into subjects of this dilution of cow’s milk, we will find that the addition of water only does not improve the digestibility of the casein, in fact, that this dilution, even with the addition of sugar, does not render it more suitable for digestion and assimilation. In 1874 Dr. A. S. Von Mansfeld wrote a very interesting paper on Infant Food and its Digestion,<sup>2</sup> and in it undertakes to prove, and I think does so with much force, that the dilution of cow’s milk with water and sugar does not reduce the percentage of casein, nor render the latter more digestible. Because, he says:

“1st. By the addition of water no dilution of the casein takes place.

“2d. It is not physiological to give food of whatever description in a highly diluted form.

<sup>1</sup> Hand-Feeding of Infants. By Eustace Smith. Op. cit.

<sup>2</sup> The Chicago Medical Journal. March, 1874. Vol. xxxi.

“3d. The digestion and assimilation of casein is not assisted by the addition of sugar.

“4th. It is the addition of fat that will make the artificial food a healthy substitute for the mother’s milk.” . . . . .

“There are found in the milk of woman about 38 parts of casein to a thousand; whereas, in that of the cow, no less than 68 parts are contained (Simon). Now take 100 scruples of fresh cow’s milk, about as much as a healthy child takes at a time, and mix this with 100 scruples of water, does this addition *lessen the amount of casein* in the fluid? It is simply a dilution of all the solid ingredients of the milk. In order to have the child get the benefit of the 100 scruples of milk, it has to be given in a bulk of 200 scruples, rather too much for one meal, and if it is given, it gets all the casein contained in the 100 scruples of milk, and if only half is given, it gets the casein of 50 scruples of milk, but existing in this milk in the ratio of  $3\frac{1}{2}$  parts to a hundred, in spite of 50 or 100 scruples of water superadded. *This water separate, as it exists out of the body, is separated at once in the stomach, leaving the casein intact.*

“As to the second assertion, we are inclined to believe little is necessary to be said. For it is not only known to physiologists that an unnatural dilution of food retards its digestion in the stomach, and as this organ in the human family is by no means created to contain fluids in quantities, *these rapidly egress* into the intestinal canal, where they are acted upon by the admixture of bile, pancreatic fluid and intestinal juice to be further prepared for absorption and assimilation; but also the pathologist and practitioner know the dangers which the partaker of food incurs, when protein compounds are not acted upon sufficiently by the gastric juice to insure their absorption by the intestinal gland apparatus. One more reason, and we are satisfied a very tenable one, is the fact, sustained by physical laws, that the larger the dilution of certain bodies, the heavier the resulting precipitate will be; and that the texture of the coagulum is the index to the facility with which this is changed by the gastric juice into peptones. Now, suppose that we are sustained by the above-mentioned scientists, what good does the additional water do, which quickly passes into the intestinal canal? *It induces a closer union of the casein mole-*



*cules*, which is already the case in cow's milk, making its digestion more difficult.

"In order to do justice to the third assertion, we have to consider the digestion of casein and of sugar. . . . .

"As to the use of casein, no doubt can be entertained. It is the desideratum for the growth of the young animal, which not only derives the pabulum for its soft structures from it, but also the material for its bony frame, through lime salts, which are abundantly present in the casein, precipitate.

"Milk enters the mouth of the infant at a temperature of bodily warmth (98° Fahr.) Here it is mixed with the secretions of the buccal cavity, called saliva. Has this any influence upon the digestion of casein? We say, most positively, no! It may exert a relaxing influence upon albuminoid food, like that of warm water; this is all, and surely not needed, when the highly diluted casein is eaten.

"After having entered the stomach, the milk loses its identity, the casein is precipitated from the whey, which latter takes with it the sugar, some of the salts, and traces of oil. By the peristaltic motion of the stomach the coagulum is reduced to a pulpy mass, being at the same time mixed with the gastric juice; *but this division does not take place until the greater part of the whey has found its way into the duodenum.* The rapidity of division is of necessity also regulated by the texture of the precipitate, as above stated. After the solution of the coagulum, it does not present the same chemical character as that of the casein solution, milk. It is not precipitated by acids and metallic oxides, as the latter; it is no more *casein* in solution, but *casein peptone* in solution. The latter owes its formation either to a catalytic influence of the gastric juice, *i.e.*, its pepsins; that is, it has changed itself, not losing its chemical equivalents, but a different grouping of its atoms has taken place, or it has formed a pepto-casein salt with the earths that are largely present in casein; the former having been acted upon by the free acid always present shortly after digestion commences in the stomach. Thus prepared, the casein compound slowly enters the intestinal canal, and in this locality its absorption is further facilitated by the action of the intestinal juices upon it, whose power upon it closely resembles that of the gastric secretion.

. . . . .

"Sugar is not acted upon by the buccal secretion, excepting perhaps some of the more complicated glucosides, but even these can only be changed by long standing, with the saliva at a high temperature; could not, therefore, be acted upon by this medium in the short space of time allotted to their presence in the mouth of the infant, since the action of the saliva is positively interfered with by healthy gastric juice. Sugar lies actually inert in the stomach, excepting its solution and transference to the duodenal canal, from whence the fluids regurgitate, as observers have shown, to favor this transmission.

*"It is the intestinal tract that favors the digestion and absorption of sugar;* soon after taking it, it finds its way into the intestinal canal, which it quickly traverses as far as the first half of the large intestine, from whence it is absorbed, or is changed in the small intestine to lactic, and in the large intestine to butyric acid; either of which, combining with the alkalies, the salts of which are found in the secretions of this locality, are also absorbed and carried into the economy to answer their purpose.

. . . . .

"Are we justified to assert, that the addition of sugar to the diluted milk is not only of no help to the digestion and assimilation of casein, or any other protein compound, but is even of harmful tendency, when taken in any quantity above the requirements of the organism?

"Who will tell the limits of its healthy or abnormal action? Who, of any practice, has not observed the ill consequences or uncomfortable condition of infants who have been fed with over-doses of sugar, or, what is worse, with candies of unhealthy composition."

Cow's milk, therefore, diluted with water and sugar, does not supply the place of mother's milk; nor will any of the other preparations generally used, excepting one or two, which I will presently mention.

Skimming the cream from the milk is not only of no benefit, but, on the contrary, despoils the milk of one of its most nutritious ingredients, fat being one of the most important ingredients in breast-milk. According to Liebig fat is the fuel, the burning of which assists largely in the maintenance of bodily

temperature; and according to Lehmann,<sup>1</sup> "fat takes a highly important share in the most important and, at the same time, the most mysterious processes in the formation of cells and tissues. . . We cannot believe that fat is a mere incidental agent in all these processes; but we must rather regard it as of essential aid in the process of converting nitrogenous nutrient substances into cells and masses of fibres, in like manner *as it co-operates in the processes of lactic fermentation and digestion.* . . . Indeed, it is impossible to believe that, in the vital activity of cellular action, fat should be without influence on the metamorphosis of the substances which it accompanies." Now, if we analyze pure cow's milk we will find that it contains about ten per cent. less fat in proportion to the casein than is the fact with woman's milk;<sup>2</sup> and, as it has been demonstrated that the casein of woman's milk coagulates more finely and softer than that of cow's milk, may this not justly be attributed to the influence of the larger amount of fat mixed with the casein of the former? If such is the fact, then in place of removing the cream from cow's milk, we should more properly take the milk from the cream and use the latter in preference to the former, especially in using the well-skimmed and watered city milk. And such has been the results of my experience in infants hand-fed and suffering from mal-nutrition, the greatest improvement following the use of rich, creamy milk, in place of the skimmed, watery mixture; and I am led to believe in consequence that the practice of using the latter results, in a large number of cases, in the class of disorders under consideration.

The addition of the starchy matters enumerated, to cow's milk is also a practice injurious to infant digestion, and one that cannot be condemned too strongly. It has already been stated that during the first two or three months of infant life both the salivary and pancreatic glands are in an undeveloped functional condition, as has been proved by the experiments of Korowin, of St. Petersburg, Dalton, and others. Now, as the assimilation of starchy matter depends upon the conversion of the starch into sugar by the saliva and pancreatic juice, the conclusion is apparent that in the absence of the latter no such conversion of the starch occurs, but that it must pass through the alimentary canal

<sup>1</sup> *Physiological Chemistry*, vol. i., page 239.

<sup>2</sup> A. S. Von Mansfeld, M.D., *op. cit.*

unchanged, and as such it will ferment and cause irritation of the stomach and bowels.

Dr. Prospero Sonsino,<sup>1</sup> of Pisa, in an article entitled, "Researches on Infant Digestion," proves by a number of experiments that there is a physiological or normal dyspepsia to starchy food in the first portion of infant life, and Dr. Routh, already quoted, also protests against the use of all starchy matters for infants before they have teeth.

The admixture, therefore, of farinaceous articles with milk to be given to infants cannot be too strongly condemned. When used to any extent such food is certain to lead to the most disastrous consequences. "In fact, the deaths of many infants can often be attributed to no other cause than a purely functional abdominal derangement, excited and maintained by farinaceous food." There is, however, one article which contains so little starchy matter and that in a state of very fine division, that it is even digested in small quantities by infants, in combination with good, rich cow's milk; it is barley-water.<sup>2</sup> The addition of the latter to cow's milk acts mechanically in preventing the coagulation of the casein into large, hard masses by the action of the gastric juice, and thus enables it to be quickly and easily digested. Good, rich cow's milk, diluted one-third or one-half with thick barley-water, to which is added a little sugar and salt, makes, in fact, the most perfect nourishment for the hand-fed infant, provided, of course, that it is not given too abundantly and too frequently. I have yet, indeed, to see the infant that will not thrive well on this food, when it is well prepared and judiciously given. Occasionally, if the bowels are a little too confined, the addition of more cream, or the substitution of oatmeal-water for the barley-water, will insure regular action often better than purgative medicines.

Leaving here the consideration of the influence of faulty alimentation in the infant in developing gastro-intestinal diseases, I will make a few remarks upon the same in infants during dentition, which physiological process really terminates the period of infancy, it being the commencement of childhood.

<sup>1</sup> London Practitioner, Jan., 1873.

<sup>2</sup> For having my attention first called to barley-water, I am indebted chiefly to Prof. A. Jacobi, of the College of Physicians and Surgeons of New York. Isinglass is also highly extolled by some, especially Meigs and Pepper and Lewis Smith, in their excellent treatises on Diseases of Children.

It is the popular idea that when an infant begins to teethe it is time to begin giving him more substantial food, and this is certainly correct in principle ; yet this very period of infant life is also popularly considered the most critical for the reason that the process of cutting teeth renders the child liable to fevers, convulsions, diarrhœa, etc., and when these disorders occur at this period of infant life, the cause is at once said to lie in the fact that "the baby is teething," and that consequently it is an unavoidable misfortune that it is sick. If there ever was an absurd fallacy fastened upon the popular mind, it is, in my opinion, this bugbear of "teething sickness." I have never seen such a case myself, and it is beyond my comprehension why the Creator should afflict only the young of man with a normal physiological process dangerous to health and life.

But nevertheless multitudes of infants are taken sick and die just at this period with gastro-intestinal disorders, and some cause there must be for it. The latter is not difficult to find : *it is faulty alimentation* either prior, or, as is most generally the case, during the cutting of the teeth.

The infant, whose digestive organs have been accustomed to easily digested milk, is at this time expected to digest "everything on the table." While it would not be well to deprive it of its infant food, yet it is deemed quite proper, even advisable, to let it enjoy adult meals, and what is equally bad, cakes and candies. Many instances I can recall in which teething infants had been given such articles even as pork, fried fish, cabbage, ham, fried potatoes, and such like trash, and yet the teeth were blamed as the cause of the ensuing gastro-intestinal disorders.

But I need not elaborate upon these facts. Time and your patience bids me to bring these remarks to a speedy conclusion. Let me, therefore, express my conviction that faulty alimentation, that is, excessive and too frequent feeding, and the inappropriate quality of the diet, is one of the chief, if not the chief cause of the gastro-intestinal disorders in infancy ; and that if children, gradually and carefully, in proportion to their growth and general development, were changed from the food of infancy to that of adults, we should hear less of the dangers of the teething period, and see comparatively few cases of gastro-intestinal disorders. Give the infant, when it begins to get its teeth, more substantial food than milk, but let milk still be



the basis of all its nourishment. Milk and a certain portion of the farinaceous foods make the best of diet, for by the period of dentition the salivary and pancreatic glands secrete, and starch in moderate quantities is easily digested.

Some of the lighter of the animal broths are also advantageous once in a while, and as the infant grows older food suitable for its development can be given, such as the easily digested meats and vegetables.

In the foregoing remarks I do not wish to be understood as denying that very many cases of gastro-intestinal disorders are unavoidable, even with the utmost care, especially as premonitory or concomitant signs of other diseases; but I do wish to be understood as of the opinion, the result of experience, that the majority of infants and young children suffer and die from gastro-intestinal disorders because of the prevailing ignorance as to the requirements of the infant as to the quantity and quality of the nourishment requisite to maintain perfect health; and that, when such diseases do occur, it would save many an infant suffering and death if the stomach and bowels were given that rest for which they so vainly appeal, in a language too often unheeded—vomiting and diarrhoea.

## A NEW BASIS FOR UTERINE PATHOLOGY.

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DR. THOMAS<sup>1</sup> begins his chapter devoted to "*General Considerations upon Uterine Pathology and Treatment*" with these words: "Nothing more decidedly retards the progress of gynecology, lowers it as a special study in the eyes of the sister departments, and fans the dying flame of prejudice with which it has been able successfully to contend only during the past half century, than the unsettled state of uterine pathology. In general medicine, in surgery, and in all other special departments, the study of pathology is made the keystone of the

<sup>1</sup> Diseases of Women, p. 216, edit. of 1874.

arch which supports them; and observers seem willing to agree as to fixed principles supporting it. In gynecology this whole subject presents the melancholy aspect of uncertainty and dissension.

This acknowledgment on the part of so high an authority may be some palliation for my presenting a view altogether different from his own, and at variance also, I believe, with every other acknowledged authority on the subject. And while Dr. Thomas, in the sentence next to that I have quoted, goes on to say that owing to the "pathological confusion" referred to "every newly-fledged specialist feels warranted in elaborating and maintaining a theory of his own;" I must plead innocent of taking this liberty, inasmuch as the ideas upon which my theory is based were in part arranged and presented in a paper before the Medical Society of the District of Columbia nearly ten years ago (May 31st, 1865), before I had given any particular attention to obstetrics or gynecology.

Intentionally concealing for a while the main idea of the present paper, I must beg to ask the reader's attention and assent to the following propositions, or facts, which I think are so generally acknowledged as to be almost axiomatic.<sup>1</sup> They are these:—

1st. Assuming an organism to have completed its physiological development without a fault, that it always has been and still is surrounded by environing conditions that exact from it just those functions which its matured structures are perfectly adapted to perform, the result could be nothing else than *perfect health*.

Instances of such *absolutely* typical perfection, sad to relate, in the civilized races of mankind are almost wholly imaginary, but departures from the typical standard may be in any degree—ever so slight, or very extreme. *Approximate* perfection is not rare.

2d. This absolute or approximate perfection of structure and function *must* continue so long as the environing conditions continue to be absolutely or approximately natural. Hence,

3d. The *cause* of any existing deviation from the typical normal standard of structure or function must therefore consist

<sup>1</sup> For a more elaborate presentation of these propositions see the No. of this Journal for November, 1872, pp. 436-437, etc.

in some differentiation of the environing conditions, and such changes of conditions would first entail modification of function, to be followed, in time, by modification of structure.

4th. Modifications of structure thus set up, while they are real abnormalities, ought to be recognized as processes of *pathological evolution*. They are strikingly analogous with processes of physiological evolution, especially in the several particulars of being (provided the change of surrounding conditions has been gradual and moderate instead of sudden and violent<sup>1</sup>) slow in their evolution; symptomatically latent; affected injuriously, and in the same manner, by the same disturbing causes, and by serving the same useful purpose of adapting the modified organs to modified functions which the modifications of surrounding media have called into play. Furthermore, structures thus undergoing modification tend to follow a typical standard as do organs and organisms when developing physiologically.

5th. Organs that have undergone structural modification adapting them to the modified functions entailed by modifications in the environing conditions, it is evident, can no longer be *perfectly* adapted to perform the original functions that belonged only to the original environing conditions. But when the new environing conditions are withdrawn, and the old ones replaced, the organism will attempt to reconstruct itself back again to its original functional and structural state. Each modification and re-modification, however, naturally goes on slowly, and hence requires time for its completion.

6th. While *gradual* modifications of structure consequent upon *gradual* changes of function and environment may be almost or quite latent, like physiological growths, *sudden* changes of a similar kind cannot be latent, but are accompanied by unpleasant symptoms, both local and general, and come more properly under the commonly accepted notion of "disease."

7th. Organisms undergoing modification of the kind now indicated, when exposed to cold, are prone to suffer from all grades of congestion and inflammation of the modifying structures; just as are the physiologically developing organs of young animals under similar circumstances. The same liability is

<sup>1</sup> Just as the changes of conditions that attend physiological development, from infancy to maturity, are *naturally* gradual and moderate also.

manifested when the modifying organs are subjected to chemical or mechanical irritation, or to unusual function excitement, or when the general circulation is disturbed by physical or emotional causes. This liability to inflammation or congestion will also bear a direct relation to the suddenness and violence of environing changes.

*8th.* The degree of local suffering, constitutional disturbance and life-shortening, attending any more or less suddenly evoked functional and structural modification, will generally bear a direct relation to the intrinsic importance of the affected organ, and to the extent and intimacy of its relations with other organs, especially with the organs of general nutrition.

In now applying these principles to the pathology of the uterus, and in attempting to search out by them the starting-point of uterine disease—the *fons et origo mali*—from which branch off the wider departures from typical normality, it is necessary, *first*, to define the natural environment of the womb, the persistent continuance of which would secure perfect (or approximate) functional and structural health; *second*, to point out the changes of environment from which follow the modifications of function and structure that constitute the beginnings of disease; *third*, to explain how these first beginnings of evil lead in time to more decided, serious, and complicated functional and structural changes.

*First*, as to the *natural environment of the womb*. This can be ascertained, and with considerable exactness, by searching out the precise functions which the organ is naturally adapted by its anatomical construction and physiological power to perform. The environing conditions that elicit from the uterus these, and only these, particular functions, will, it is evident, constitute its *natural* environment.

Before the age of puberty, before the reproductive system has completed its physiological development, the uterus performs no function. It rests. It receives comparatively little blood. It is not the seat (as it is in later years) of vascular excitement, orgasmic congestion or erection. The conditions, therefore, that at this time constitute its natural environment are those that allow it perfect repose, that do not in any way tend to produce vascular excitement, orgasm, etc., and as one of the most important constituents among these conditions must

be mentioned total absence of all *sexual* excitants. However reluctant we may be to acknowledge depravity in our own race, and however exceptional may be the instances in which precocious excitement of the sexual organs has led to any material incipient change in the functional career and structural integrity of the uterus, still, as scientists, we are compelled to acknowledge that such excitation is a *possible* disturber of uterine equanimity during the ante-pubertic age. With what frequency it occurs is not in our power to determine; and I shall not dwell upon the manner in, or the circumstances under which such precocious excitement of the sexual system in the female has been known to be produced. Apart from actual vice, however learned, a vitiated stage, and a vitiated press have, doubtless, contributed in some degree to direct the minds of the young into such channels of thought as develop ideas, which, in the natural course of things, ought not to occur until later years, and the reactionary tendency of which upon the physical organism is to produce excitement of the sexual system. Here it concerns us most to know that females who have been subjected to the influence of excitement of the kind referred to are apt to be *precipitated into the transformation of the pubertic epoch before their time*. Such a misfortune cannot fail to leave its impress upon the entire future reproductive life of the individual.

But allowing excitement before puberty to be only an insignificant or infrequent factor (though it *may* be otherwise) in the production of uterine disease, let us go on to the age of puberty itself. And in asking what is *now* the womb's natural environment, let us assume that the reproductive system, together with the entire organism, has completed its normal development without a fault; that the individual has inherited no tendency to disease, and that from birth the organism has been surrounded by those conditions only that are perfectly natural to it. The uterus in such a case would be perfectly healthy.

What external conditions would *now* constitute its natural environment?

There is but one answer, viz.: those conditions that elicit from it the special function which its anatomical construction and functional power adapt it to perform.

What *is* this function?

There can be but one reply, viz.: it is *reproduction*.



What special condition in the natural environment is it that serves to elicit this function of the uterus?

Again, there is only a solitary answer available, viz.: the condition that secures the performance of the preliminary process of fecundation—in point of fact, *un mâle*.

Let this condition of the natural environment be changed, and there must necessarily occur a corresponding modification of the natural function of the uterus, and in time of its natural structure, the modification of function and structure serving to adapt the uterus to the environing modification, but at the same time necessarily unfitting it, in some degree, for the perfect execution of the functions that belonged only to its originally natural environment.

The modification of function here referred to, as we all know, is *menstruation*. In the performance of the menstrual office the uterus is made to deviate from the true line of typical normality. In the strictest sense of the word, therefore, menstruation is a disease; it is not a purely physiological process, but a departure from nature; and in a recently developed uterus, otherwise perfectly healthy, menstruation constitutes the first obliquity from the direct line of health, and from it, as a beginning, the wider and more complicated divergences branch out. By considering the function (?) of menstruation as an abnormal process, we, I think, hold the end of the thread in the tangled skein, by following which the mystery of uterine pathology may be unravelled, and its knots of confusion orderly arranged. And here we strike the central idea of the present essay.

So deeply grounded in the medical mind is the conception of the process of menstruation being purely physiological in character, that I may here present, though only in brief, the considerations that ought to lead any unbiassed thinker to a different conclusion. They are as follows:

1st. Menstruation is the result of an interference with nature, of a thwarting of her designs, of a violation of her laws, and is preventable by obedience to those laws.

2d. In the great majority of cases it is not latent, as are other purely physiological processes, but is accompanied by unpleasant symptoms.

3d. To preserve comfort and cleanliness, it requires during

its continuance the application of an artificial appendage to the person. This requirement belongs to no *natural* emmenatory.

4th. Menstruation is a hemorrhage, it is attended with the rupture of blood-vessels. Blood-vessels were not made to be ruptured. No hemorrhage is natural.

5th. Although menstruation is desirable and necessary in celibate females to relieve congestion of the uterus, it still ranks second best to reproduction which prevents abnormal congestion; and it ought no more to be considered physiological, on account of its salutary effect, than epistaxis, which relieves congestion of the brain, or bleeding from hemorrhoids, which lessens portal congestion, nor indeed than "vicarious menstruation" from the nose, skin, breast, stomach, lungs, etc., which are equally beneficial in depleting an over-full vascular system.

6th. The menstrual *periods* in women are analogous with the *periods* of œstruation ("heat" or "rut") and ovulation in other animals. In both women and animals these *epochs* are the *periods* naturally designed for *coitu* and successful impregnation, as evidenced (first) by the coincident discharge of ovules, and (second) by the well-known greater certainty of conception taking place when coition occurs during the epoch.<sup>1</sup> Now the menstrual discharge, except during the first few ovulatory periods of puberty, prevents coition, or if sexual union is admitted without precautionary measures, it may produce gonorrhœa in the male. The menstrual *discharge of blood* has no analogue in other animals.

7th. Evidence is wanting to prove that menstruation is common in women belonging to the savage races of mankind, who live more strictly in accordance with nature, untrammelled in their reproductive function by the usages of civilization. The Hindoo women, as a rule, do not menstruate; with them menstruation is considered a crime.

8th. History does not furnish *unequivocal* evidence that menstruation was common in ancient times. True, we are told (Genesis, chap. xxxi., v. 35) that when Laban was searching for the images Rachel had stolen, and which she had concealed by sitting upon them, she said to him: "Let it not displease my lord that I cannot rise up before thee, for the *custom* of women

<sup>1</sup> Henry the Second of France was in this way, by the advice of Fernel, his physician, enabled, after former repeated failures, to secure an heir to his possessions. (Bedford's Obstet., pp. 107, 307-8. 3d Edition.)

is upon me." But the case of Rachel was altogether an anomalous one. She was twice disappointed in marriage; twice were her expected nuptials postponed for a period of seven years (14 in all) by the intrigues of her father Laban. For some time after marriage, unlike her sister Leah, she was barren (Genesis, chap. xxix., v. 31). Envyng Leah, she said to Jacob: "Give me children, or else I die." And Jacob answered: "Am I in God's stead who hath withheld from thee the fruit of the womb" (Genesis, chap. xxx., v. 1 and 2). Although afterwards the mother of two children, she is the first woman recorded as having died in childbed, as she did at the birth of Benjamin. (Genesis, chap. xxxv., v. 18). Her sister Leah bore seven children.

The term "custom," in the text referred to, would seem to be more or less conclusive (admitting no errors of translation) were it not for other references in which we read (Leviticus, chap. xv., v. 19): "If a woman have an issue, and the issue of her flesh be blood, she shall be put apart seven days, and whosoever toucheth her shall be unclean," etc. And again (v. 29, 30): "She shall take two turtles and two pigeons, which the priest shall offer as an offering to *make an atonement for her*, for the issue of her uncleanness." Thus the "issue" seems to have been regarded as sinful. If it had been common, or "customary," how numerous must have been the priests! how constant their duties! and what countless thousands of turtles and pigeons must have been required for the sacrificial offerings. Of these there is no sufficient record.

9th. Women have been known to bear large families and enjoy good health without ever menstruating at all. Can it be said that such women are sick? Must it not rather be admitted that they are enjoying a higher grade of health? that their reproductive systems are following more strictly a natural course, than belongs to sterile and menstruating females?

10th. Since procreation is natural to women only during a part of their life, the child-bearing period must have a beginning. If puberty, when the organs are fully developed and prepared to fulfil the procreative office, is not the natural period for reproduction to begin, when else *is* the beginning of the child-bearing period? To postpone the reproductive act beyond its natural time is abnormal; menstruation would not occur without such postponement.

11th. It is a quite familiar fact that organs which are the seat of structural disease, or which are suffering derangement of function, are peculiarly liable to abnormal congestion and inflammation, when the individual is exposed to cold. No truly physiological function has this liability entailed upon it. Normal digestion does not prevent the individual from tolerating ordinary exposure, but the dyspeptic with deranged digestion cannot enjoy the same freedom. So exactly normal reproduction does not prevent the female from tolerating ordinary exposure, but she who is the subject of menstruation cannot enjoy the same liberty. The menstrual act therefore exhibits here again the qualities of a pathological process.

12th. One other consideration on this head I cannot consent to omit. Though usually ignored, it ought not to be overlooked. I refer to the *physiological office* of personal beauty in the female. This is a most important factor in the promotion of procreation. Without it, in all probability, the race would become extinct; certainly if the human female in the young age of her just completed development, and for some time afterwards, were no more attractive than the woman of old age, such a result could elicit no surprise. The office of beauty then is to attract the male, and it does. We observe the same thing throughout the animated world. Birds, insects, reptiles, in fact all animals and many plants, become more beautiful in their exterior during the age of ovulation and the seasonal periods of æstivation. It is for this that the lily blooms and the rose dispels its fragrance; they attract the pollen-laden bee.<sup>1</sup> The "livelier iris on the burnished dove," the beautiful tints of the butterfly's wing, the brighter gold on the serpent's scales, the phosphorescent gleam of the glow-worm's light, etc., during the season of copulation, are all unmistakable illustrations of the same harmonious arrangement.

Now, at what period of her life is woman *most* beautiful, and *most* attractive to the opposite sex? It is at the age of puberty. Writers on Obstetrics have often enough exhausted all their eloquence in vainly attempting to describe the remarkable changes—not only as to personal appearance, but also

<sup>1</sup> When a flower is fertilized by the wind it never has a gaily-colored corolla, as in maize, docks, grasses, and the oak, ash, fir, etc. (See Darwin's "Origin of Species," page 161, 7th edition.)

beauty of character and emotional susceptibility—that occur at this significant epoch of woman's life. The female becomes transformed, both in her *morale* and her *physique*, writes Dr. Meigs, “as if by the touch of a magician's wand.” And noteworthy among these transmutations, must be mentioned the development of a selective taste by which she is led to admire those characteristics in the other sex which follow the transition from youth to manhood.

The coincident occurrence of personal beauty with the ripening and discharge of ovules, and with the complete development of the reproductive organs, tells us plainly enough that the performance of the procreative office is now the most natural and only *strictly* natural course for the female economy to pursue. This is the common-sense truth. Think as we will, it defies denial. The catamenial “function” is a conservative alternative, but it is second-best to reproduction and a primitive departure from typical normality.

Other considerations in support of the idea that menstruation is pathological might be added, did the intended limits of this essay permit.<sup>1</sup>

I do not assert that *woman as she is* ought not to suffer the menstrual hemorrhage; nor do I affirm that *every* female on attaining the age of puberty ought to marry and perform the reproductive office. Suce a construction of my remarks would be crude, unjust, and puerile in the extreme. What I do affirm, and without hesitation, is, that assuming the female to have attained an approximate or perfect development, that she has inherited no tendency to disease, and been subjected to no such abnormal agencies as would affect injuriously the reproductive organs (and excluding, of course, as every physiologist must, the social, educational, and financial considerations which are allowed to influence the time of matrimonial alliances)—*under these circumstances there can be no doubt that impregnation during one of the several ovulatory periods that usually precede the establishment of menstruation at the puberic age is strictly in accordance with nature, and the surest means of*

<sup>1</sup> I have never been able to meet with Roussel's work—“*Système Physique et morale de la Femme*.” In it, however, he regards the menstrual discharge as pathological—as an unnatural habit acquired in past ages, and now become a settled and regular attribute. (See Meigs' “*Woman and her Diseases*,” 4th edit., p. 451.)



*maintaining typical perfection, both functional and structural, of the reproductive organs.*

Having thus shown how modification of environment leads to modification of function, we have next to elaborate the indisputable truth, that the produced modification of *function* (*i.e.*, menstruation) is in time accompanied by a corresponding modification of *structure*. The menstruating uterus cannot retain the structural integrity of one that has been exercised only in the performance of its perfectly natural office; nor when the womb has undergone the structural changes incident to menstruation can it possibly be *perfectly* fitted to perform its part in the normal process of procreation, unless, indeed, it should undergo retrogressive modification, changing it back again to its primitive (*i.e.*, puberic, but ante-menstruating) structural state, which it cannot so readily do, however, inasmuch as the restoration of the natural environment imposes pregnancy; consequently the retrogressive change referred to would be blended with the progressive evolutionary changes of gestation; and each would embarrass the other.

The general statement that persistent modification of function leads in time to corresponding modification of structure, is, as I have said, axiomatic.

If we now inquire into the nature and order of development of the structural changes in the uterus, resulting from a long continuance of its so-called menstrual "function," we find the starting point to be hyperæmia—*abnormal congestion*. It is an universal law in physiology that blood-supply and blood-demand always increase with increase of functional activity; and in organs whose function is more or less periodical the supply of blood increases during the *periods* of functional activity, as we see, *ex. gr.*, in the stomach during the periods of active digestion. All such periodical augmentations of blood-supply, *when the blood is appropriated to the increase of function for which it was designed*, are purely physiological; but when the periodical increase of blood is thrown upon an organ which is *prevented from performing the extra function for the maintenance of which the increase of blood was a designed provision*, there then follows a pathological accumulation in the vessels—an abnormal congestion. It is thus with the womb at the ovulatory periods. The blood is sent to the uterus for no other

purpose than to supply it with materials out of which, after fecundation, an embryo is to be nourished, and the uterine structures themselves undergo the exalted development that begins with conception and continues during gestation. And while Nature, in the absence of impregnation, enables the uterus to purge itself of the accumulated blood by the conservative hemorrhage of menstruation, yet there is a period of hours, or days, generally the latter, at each monthly epoch, immediately preceding the actual hemorrhage, and remaining some time after it, during which the condition of congestion certainly exists. This is evident from the aches and pains about the pelvis, by the mucous discharge that precedes and follows the sanguineous flow, as well as by post-mortem examinations upon women, otherwise healthy, who have been accidentally killed, just before, or after the hemorrhage was poured out.

Taking this congestion as the starting point (but admitting, since it is only of temporary duration, that consequent structural changes may be slow in their evolution, and thus apt to escape detection; at the same time, however, bearing in mind, that during *nearly* a whole week (Scanzoni says nearly two), out of every four, the congestion referred to does in some degree persist, and which would give us, if not one-fourth, at least a full one-fifth of every month, and consequently one-fifth of every year of celibacy as the duration of the congestion)—I say, taking this congestion as the starting point, we should expect to find developed in the uterus the same abnormal tissue that is known to result from the persistent continuance of congestion in other organs. This is emphatically fibrous or connective tissue, variously called “fibrotic,” “fibroid,” and “cirrhotic” tissue, the latest and final forms of which in very old cases, and attained by steady gradation, are represented in “fibro-cartilaginous,” “cartilaginous,” and “osseous” tissue.<sup>1</sup> Familiar illustrations of these hyperplasias of connective tissue,

<sup>1</sup> It is a beautiful illustration of the conservative *economy* of the body that the highly-organized, constantly-changing, and hyper-vascular.—I had almost said aristocratic—tissues composing organs of great functional activity, and which require much blood for the maintenance of their nutrition, should, when the organ becomes idle, or performs less than its full function, be supplanted by the less expensive, less vascular, and almost functionless fibrous or hyperplased connective tissue, which requires for its nutrition comparatively little blood.

following and resulting from protracted congestion, are presented in the stomach, liver, pleura, lung, and emphatically in the prostate gland of the male, the latter organ being perhaps more nearly allied in its physiological and pathological relations to the uterus than the other instances mentioned.

If we now inquire whether such connective (fibroid) tissue hyperplasias are or not common in the womb, we at once stumble on the broad fact that such kinds of uterine growths are exceedingly common. According to Bayle, of all women dying beyond thirty-five years of age, twenty per cent. are affected with fibrous tumors of the uterus. Klob states that the frequency of fibroid growths of the uterus is such that "at the climacteric period undoubtedly 40 per cent. of the uteri of females who die after the fiftieth year contain fibroid tumors." McClintock asserts that "without question the most frequent organic disease of the uterus, if we except inflammation<sup>1</sup> and its effects, is fibrous tumor." Dr. Barnes admits himself unable to oppose these statements numerically.

If we now add to these larger and late-appearing growths, which have reached the dignity of tumors, those other, lesser, more diffused and far more frequent cases in which the uterus is indurated, increased in weight and slightly enlarged from a more regular and moderate development of fibrous tissue (cases of "chronic parenchymatous metritis," according to older authors), the uteri will indeed be few in which more or less amount of the morbid growth does not exist. In truth, no organ in the body is more prone to exhibit a superfluous growth of connective tissue than the uterus, and while modern authors are content to acknowledge abnormal hyperæmia as the initial

<sup>1</sup> In thus using the term inflammation, Dr. McClintock still clings to the once prevalent, but now almost obsolete idea, that the diffuse growth of connective tissue—the "uterine induration" of older authors—is always of inflammatory origin. We now know it to be rather a slow, latent, chronic, and evolutionary growth, the result of prolonged abnormal hyperæmia, however produced. Klob, Andral, and J. Hughes Bennet advise a disuse of the term "inflammation" altogether, and Dr. Thomas, in coinciding with them, quotes the words of "an accomplished writer" of New York as follows: "The entity inflammation, fallen from its high and palmy state, is hanging by its eyelids as a pathogenic factor in most of the organs of the body; its last resting place seems to be the womb, and here still it has a good foothold. Why should uterine pathology alone be cumbered by an outworn theory?" (See Thomas' "Diseases of Women," 4th ed., pp. 276, 277.)

change leading to this superfluous growth, I merely design to go one step farther back, and cite as (not the only, perhaps, but) the most common cause of this underlying hyperæmia the abnormal condition of protracted celibacy—of ovulation without impregnation—of, in fact, *menstruation and its inevitable sequelæ*.

The fact that “areolar hyperplasia,” as described by Dr. Thomas, should be less frequent in virgins, and that its chief cause, as he asserts, should be subinvolution of the uterus (necessarily following pregnancy), would at first sight seem to conflict with the views I have herein proposed. It, however, does not. I am perfectly willing to admit, as an undoubted fact, that the “areolar hyperplasia” of Dr. Thomas—by which we understand a more than usually *rapid* (and consequently *non-latent*) and *exaggerated* evolution of connective tissue, due to coexisting and *exaggerated* hyperæmia—is more common during subinvolution than in nulliparous women, but I fearlessly add that the subinvolution itself is caused by the less marked, latent, but inevitable tissue changes that have occurred prior to pregnancy *as the result of the menstrual habit*. I have already announced, as one of my preliminary propositions, that “organisms, which have undergone structural modification adapting them to modified functions entailed by modification of environing conditions, can no longer be perfectly adapted to the original functions demanded by the original environing conditions.” Applying this principle to the case in point, we may assert that the uterus, which has undergone the adaptive structural changes consequent upon a protracted performance of the menstrual function, is no longer *perfectly* adapted to perform the various offices that make up the reproductive function. One of these offices—and a perfectly natural one—is involution<sup>1</sup> of the womb after delivery; hence, for the reason stated, this is imperfectly accomplished, and, as a consequence, hyperæmia and areolar hyperplasia follow.

As a sequence to this argument, and as a proof of its validity, we ought to find that young females who become pregnant early in life, before the menstrual habit has been established

<sup>1</sup> Since involution is a *structural change*, rather than a functional act, it may perhaps be a breach of cognominal propriety to call it a “function.” My meaning in the text, will, however, I trust, be understood.

long enough to have caused much structural modification, are less liable to subinvolution and areolar hyperplasia than females who have menstruated ten or twenty years before becoming pregnant, or than those who, though they have borne children early, have passed through years of celibacy, and then resumed child-bearing.

Unfortunately writers give no definite information on this point. Thomas only states that "the virgin uterus is unfavorable to the development of areolar hyperplasia," and that a "uterus once affected by gestation offers a more propitious field for its development." But this expresses nothing as to the greater or less liability to subinvolution in women who have *menstruated many years before becoming pregnant*. In the absence of proof, I am, however, willing to leave it to the future experience of gynecologists, and for the present must adhere to the opinion that subinvolution is less likely to occur in women who have *not* been long accustomed to the menstrual habit.

In further proof of the want of adaption of uteri that have been long menstruating to the office of reproduction, the fact may be mentioned, in which, I think, all will agree, that the unpleasant symptoms of pregnancy, as a rule, admitting exceptions, and other things being equal, are less liable to occur, or when they do occur are less exaggerated in young females than in those who have been long menstruating.

Next, what *anatomical evidence* have we that the womb, long habituated to menstruation, undergoes a corresponding structural change? On this point we have already mentioned the general facts that prolonged hyperæmia in any organ leads to proliferation of connective tissue; that, in the case of the uterus, menstruation entails hyperæmia, and that hyperplasia of connective tissue in the womb is exceedingly common.

We have now to add that during menstruation there occurs on the internal surface of the uterus a rupture of capillary blood-vessels, and, in all probability, of vessels larger than capillaries. When the flow is over, these ruptured vessels heal. How do they heal? By the formation, in their walls, of fibroplastic cicatricial tissue. Actual cicatrices *must* be formed in some instances, microscopic in size perhaps, but making up for



this in number and in the frequency of their recurrence.<sup>1</sup> Now let the bursting and healing continue month after month, and year after year, and can it be otherwise than that the internal lining of the womb shall have widely departed from its original and normal state? Shall we not rather find in it adventitious, abnormal tissue—the fibrous or fibro-plastic tissue—such as occurs in other parts of the body where analogous congestions, hemorrhages, and cicatrizations have occurred? *A priori*, it may be answered, we shall; *à posteriori*, we do. The microscopists have already given us information on this point. Dr. Charles Robin affirms that the mucous membrane of the uterus contains fibro-plastic tissue, which is the ordinary element of fibrous tumors. And Dr. G. S. Bedford (*Dis. of Wom. and Chil.*, p. 415) calls particular attention to a fibro-plastic tissue found in the internal lining of the womb, which he states “is properly so named not only from its microscopic characters, but because it belongs exclusively to abnormal structures: the presence, therefore, of this fibro-plastic material in the mucous investment of the uterus is worthy of recollection, as being *the only example in the economy of this character of tissue in any normal structure.*” “No satisfactory explanation,” he continues, “has yet been given of its presence, and it remains for some future observer to elucidate the question.” “Dubois” (Dr. Bedford tells us) “simply suggests that it may be due to the numerous changes which the mucous membrane of the organ is more or less constantly undergoing.” *I maintain that it is due to the abnormal changes of structure that result from the abnormal “function” of menstruation.*

Besides the proliferation of connective tissue, and the obliteration and cicatrization of blood-vessels that result from the menstrual process, there is still another mode in which consequent abnormal tissue-production may be accounted for. I mean that of blood extravasation—ecchymosis. In a woman who had died just as the menstrual flow had begun, Coste found ecchymoses under the mucous membrane. And when we remember that

<sup>1</sup> If it should be said the vessels do *not* rupture, but admit the migration of corpuscles through the stomata in their walls, I answer that the flow is too sudden to be thus accounted for. Furthermore, if, as some authors aver, the entire mucous membrane is shed at each monthly period (of which we find no analogy in other mucous cavities, nor in the uteri of other animals) what becomes of the vascular twigs that ramified in the mucous membrane itself. Are they not broken off?

thrombi occur in the erectile tissue of the vulva, and hæmatocele in the pelvic cavity, as the result of congestion attending prolonged and exaggerated sexual orgasm, it is in no way a matter of surprise that smaller ecchymoses occur in the tissues of the womb during menstruation. And though the extravasated blood be again reabsorbed, yet some remnant of the clot, microscopic in size perhaps, must remain, nobody knows how long, forming a deposit of organized fibrous tissue which serves as a centre around which new accretions of connective tissue are from time to time added. Ecchymoses of this kind occurring under the peritoneum, or in the interstices of the uterine walls, may, in the manner just explained, constitute the beginnings of what in after-life turn out to be veritable fibroid tumors, and which would thus display a strict analogy with the little fibrous tumors that remain about the anus after extravasation from a hemorrhoidal vessel.

These primary changes in the internal lining of the womb, resulting from menstruation, must have a direct bearing upon the etiology of uterine catarrh<sup>1</sup> (endometritis) both cervical and corporeal, and these latter, as is well known, often precede, and are indeed admitted to lay the foundation for subsequent areolar hyperplasia.

There is still another way in which areolar hyperplasia, when it results from subinvolution, may be explained to arise from the protracted habit of menstruation. I refer to the inevitable dilatation, thickening, loss of elasticity and of normal adaptive modificability, of the uterine vessels. During the congestion of menstruation, when the vessels are distended to bursting, there is dilatation of capillaries, retardation of the blood-current, and consequently distention in some degree of the secondary-sized vessels, and also, though in a less degree, of the more remote and larger arterial trunks of the uterine and ovarian arteries. Under these circumstances, the walls even of the larger vessels become in time permanently thickened, dilated, less elastic, and cannot so readily undergo the physiological hypertrophy natural to pregnancy, *nor the physiological atrophy natural to involution after delivery*. Consequently after parturition these vessels do not regulate the blood supply as they ought; they ad-

<sup>1</sup> Their influence, later in life, upon the causation of adherent placenta, is not unworthy of consideration.

mit too much blood to the recently delivered uterus, and as a result subinvolution, hyperæmia, and hyperplasia necessarily follow.

Before concluding this paper I must add that I have here only presented the disconnected outline of a widely extending subject which I think is justly deserving of serious consideration.

At present perhaps positive experimental data are wanting, either to prove or disprove the ideas I have presented. With the myriad pathological changes that occur in the uterus, their numerous shades, and frequency of occurrence, who of us can know what particular womb represents the type of perfect health, I mean as to histological structure? The main point now required of the histologist is to give us an account of the histological elements entering into the composition of a womb that has reached its full development, and is prepared for procreation, but which has never yet been the seat of menstruation. In a woman otherwise healthy this would be the nearest approach to the normal type of a perfect womb obtainable. The result of such an examination ought then to be compared with the histological elements exhibited in a womb that has for some years—say ten or twenty—been performing the menstrual office, and that has been exempt from any other known abnormality, and which has not been subjected to the changes incident to impregnation. This is the only true means by which the influence of menstruation in producing anomalous and abnormal changes of structure can be ascertained. I affirm that the structure of the organ in the two cases will be found very dissimilar.

Another not unimportant and as yet unascertained point in the physiology of the uterus, and which has a direct bearing upon its pathology, and which, so far as I am aware, has scarcely been touched upon by writers of physiology and gynecology, is this: Assuming a female to have attained a perfect development in all of her organs, that she has escaped any serious disease, and has become impregnated soon after puberty before the occurrence of menstruation, and exacting further that she is in every way robust and strong, *how often ought she to bear children?* As in the structure, so in the function of all organs and organisms there is a normal type or standard of perfection, if we can only discern what that is; and one element in the type of all functions that are periodical is their frequency. How frequently, then, with the most propitious surroundings, and in

a condition of perfect health, ought the female to bear a child ? As a general principle, applicable to all organs alike, I think it may be stated that the most perfect type of function is *the highest degree of normal functional activity of which the organ is capable*, and this is the surest means of maintaining a perfect nutrition of every organ.

Now, applying this principle to the uterus we find that it occasionally occurs, and not so *very* rarely either, that a female will bear a child once a year for several years in succession (perhaps eight or ten) and not suffer from it any material inconvenience, so far as her general health is concerned. Becoming pregnant in a month or two after delivery, she nurses the child already born during her pregnancy, and even after the birth of the succeeding one, suckles them both. I have now a patient in whom this has occurred three successive years, and during the past month she has had a fourth delivery (of twins), the pregnancy beginning three months after the delivery of her third child. She enjoys excellent health. On inquiry, I am informed by several medical gentlemen of this city that they have met with similar cases.

Judging from what occurs in other animals, the repetition of child-birth in the human female once a year ought not to surprise us. The mare, whose period of gestation is eleven months instead of nine, bears naturally once a year, and suckles the colt of one year during the pregnancy of the next. The period of estruation (of ovulation, or "heat") when she will again admit the stallion, comes on nine days after delivery. To breeders of stock this is well known. The domestic rabbit, usually delivered at night, admits the male and becomes impregnated on the following day, as I have myself often observed in the breeding of these animals. And although Dr. Barnes states that the rabbit "at liberty" only has one or two litters a year ; I do not think his knowledge of comparative physiology on this point absolutely reliable, as he immediately afterwards makes the erroneous assertion, that "hens whose eggs are taken away from them to prevent their sitting will lay almost every day for eight months in the year."

The recognized possibility of the referred-to quick succession of pregnancy in the human female must materially influence our ideas as to what, in a state of perfect health, constitutes the

normal period of uterine involution. Since the female *can* become pregnant one month after delivery, and if this (since it is certainly the highest grade of functional activity of which the uterus is capable) is the normal type of function as regards periodical pregnancy, it would seem to follow that normally involution is, or ought to be, complete in one month after parturition. If so, the alleged normal period of six, eight, or more weeks, given by different authors, is a deviation from the normal type; either this or the succeeding pregnancy begins while involution is still in progress. But this matter is at present altogether *sub judice*.

I might refer to other questions relative to the physiology of the reproductive system, in which, figuratively speaking, we are at present entirely "at sea." Until they are more definitely settled our knowledge of uterine pathology must be very confused and uncertain; but in no point, I believe, are we more in error than in recognizing menstruation as a purely physiological phenomenon.

When I look over the armory of instruments that are daily used by the gynecologist, and regard the masses of medical lore crammed into our voluminous text-books on gynecology, and when in the examination of women I so seldom find a perfectly healthy womb, I am often led to ask: Why is it that this unfortunate organ should be the subject of so many ills? And just as often am I disposed to give the same answer that the Divine does, when, looking into my face on a Sunday morning, he explains the reason of my spiritual deterioration. As with the transgressor metaphysically, so with the womb physiologically: it has left undone the things which it ought to have done, and has done those things which it ought not to have done; and (consequently) *there is no health in it*.

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# PREGNANCY AND LABOR IN EPILEPTIC WOMEN.

By JOHN S. PARRY, M.D.,

Accoucheur to the Philadelphia Hospital; Vice-President of the Obstetrical Society, Philadelphia,  
etc., etc.

(Read before the Obstetrical Society of Philadelphia, June, 1874.)

ACCOCHEURS occasionally meet with epileptics who have become pregnant, and who pass through the period of pregnancy and labor without any alarming symptoms, the fits occurring as usual before labor sets in, but being discontinued after the pains come on. The writer can call to mind several confirmed epileptics who have been under his care, who required no additional attention during the time when they were *enceinte*, and in whom the process of parturition was as favorable as in perfectly healthy women. So far as he can now remember without having notes of those cases, no paroxysms of the disease occurred during the conventional ten days succeeding delivery, when the women are under professional supervision.

That this happy termination of labor without epileptic seizures is not without exceptions the succeeding history shows conclusively.

The patient is an unmarried girl, æt. 23, who came to the Philadelphia Hospital to be confined. She is a short, stout woman, and when admitted she had the appearance of a confirmed epileptic. The disease had existed for years, and had resisted all efforts at treatment. The fits are preceded by a distinct aura, commencing in the leg. They occur very frequently, and have all the characteristics of the convulsions of this disease. In April last, shortly after admission to the hospital, she had no less than twenty-five convulsions in five hours. After that time the frequency of the seizures diminished under the use of bromide of calcium prescribed by my colleague, Dr. Girvin, and for some time before labor commenced her condition was very fair, but as the time for her travail approached there was a more decided tendency to convulsions.

Labor began about 5 P.M., on June 30th, and almost immediately afterwards epileptic convulsions set in. Dr. John M. Keating, the resident accoucheur, at once sent me a hurried message, stating that it was probable that immediate delivery was indicated. When I reached her I found that she had reached full term. Labor pains of a severe character were occurring at regular intervals. Her abdomen was very large. This had attracted attention before the commencement of her labor. The distention was increased by tympany. Vaginal examination showed that the os was dilating. At this time, two hours and a half after the commencement of labor, it would more than admit two fingers, and was not at all dilatable. The child's head could be felt presenting, and could be moved with great ease in the cavity of the uterus. It was evident that the womb contained a large quantity of liquor amnii. The pelvic passages were perfectly normal.

Her pulse was 96 per minute, full and round. Epileptic fits occurred at short intervals. Between five and half-past seven o'clock she had sixteen of these, remaining perfectly unconscious for some time afterward. Dr. Keating had noticed from the time that labor commenced the convulsions were preceded by violent slapping of the belly with her hands, while when the pains would come on she would clap her hands strongly and rapidly. We were thus enabled to tell whether a fit or a pain was approaching when sitting anywhere in the room. The convulsions were very violent, lasted several minutes and presented all the ordinary phenomena of these epileptic seizures. They were remarkable in one particular, being characterized by extreme opisthotonos. The heels and occiput were almost in contact, while the chest and protuberant abdomen made a very strong curve. During the paroxysm the legs were so strongly flexed that the heels were brought in contact with the posterior surface of the thighs and retained there until the force of the paroxysm was entirely spent. During the intervals between the convulsions she laid with her legs fully extended, but during the fits only the occiput and knees touched the bed.

Shortly after 7.30 P.M. she was etherized. The ether arrested the convulsions, but it likewise completely suspended the strong labor pains which had been occurring at intervals of ten minutes. As the convulsions were so severe, and as it seemed

probable that they would recur when she rallied from the influence of the ether, I determined to hurry dilatation of the os and rupture the bag of waters with the hope of mitigating or arresting the convulsions. At 8.15 Dr. Keating introduced the middle-sized Barnes' dilator with great ease. It was slowly distended, and the largest size soon substituted for it. At 9.15 P.M. this dilator, which was not as large as it should have been, was removed and the dilatation of the os was almost complete. The head was now found in the left occipito-anterior position. The child was very movable in the cavity of the uterus. The membranes were very strong and bulged freely from the os uteri. About 10.35 P.M. they were ruptured and a large quantity of liquor amnii drained off.

Simpson's forceps were now applied with ease, but upon making traction the os was almost, but not quite sufficiently dilated to allow the head to pass. They were therefore removed, and as the uterus was considerably diminished by the discharge of the waters, it was decided to suspend the use of the ether, and if possible to allow nature to finish the nearly complete dilatation.

When the effects of the anæsthetic disappeared she was semi-conscious, and complained of exhaustion and pain in her head. The uterine contraction recurred at regular intervals, and in a little more than an hour dilatation was complete. Dr. Keating again applied the forceps, and at midnight delivered her of a healthy living boy, who weighed seven pounds seven ounces. After delivery she remained absolutely unconscious for some time. After consciousness was restored she was in fair condition, and had no more convulsions. Her urine was not albuminous at any time.

There are several points of interest in connection with this case. Standing by the poor woman's bed-side during the violent convulsions which she had in April last, or during those which she had while in labor, one could hardly believe that she could give birth to a living child. Bent almost double, with only the bases of the arch touching the bed, it seemed almost impossible that the child could escape injury. Especially when we remember that death of the fœtus—though by no means universal, is the common result in puerperal convulsions. In this particular that disease appears to differ from epilepsy.

The second point of interest is the management of the case. She would have borne bleeding, but there was no obvious indication for it. Her arm is already sufficiently scarred by the lancet, though according to her own statements venesection has always injured her.

The administration of the anæsthetic is probably the first therapeutic agent that would suggest itself to any accoucheur, and it certainly acted most happily so far as the convulsions were concerned, but it completely arrested the pains. Then came up the important question, shall we trust to the anæsthetic to ward off the convulsions, and wait until nature has had an opportunity to complete dilatation? There was evidently a necessity to empty the uterus as quickly as possible, if irritation produced by the extreme distention, had anything to do with the production of the epileptic paroxysms. Therefore I decided to complete dilatation as rapidly as possible, and to deliver with the forceps if necessary, though I freely acknowledge that had the case occurred in private practice I would not have been so hasty. It is to elicit discussion upon the method of managing these cases that I have dwelt thus long upon this subject.

Another interesting case of epilepsy in a pregnant woman came under my care a few years since. The lady, who was then about twenty-four years old, engaged me to attend her in an approaching confinement. She was at that time a handsome woman, with good blood-making powers, dark hair, fair complexion, and thin, delicate skin. She had every appearance of a woman in good health. She had no venal disease. There was no hereditary tendency to nervous affections, and no member of the family had ever had epilepsy. My patient herself remained perfectly free from the disease until she was between three and four months gone in her first pregnancy, when an epileptic convulsion occurred. The fits were repeated at intervals of a few days, or one or two weeks, until she was about seven months gone, when she miscarried with a dead and decomposing fœtus. She had no convulsions during labor. Her recovery was natural, and after delivery there was no tendency to epilepsy.

In her second and last she came under my care too late for me to hope to do anything to prevent the epilepsy. The fits

had set in when she was about three months gone. They recurred several times a week; came on without warning, so that she would fall unconscious wherever she might be at the time of the paroxysm. The convulsive movements were not remarkably severe, but the general characters of an epileptic seizure were fairly developed. The symptoms continued until the end of the eighth month of gestation, when I delivered her of a dead decomposing fœtus, of seven months' development. The labor differed in no respect from ordinary labors, in which dead children are born. She had no convulsions during labor.

Since this confinement, about four years ago, this lady has never been pregnant. In summer of 1872 she began to show signs of commencing phthisis. During the succeeding fall she had a number of severe pulmonary hemorrhages, which prostrated her very much, but she rallied again after several weeks. During the next year, besides many other cares, she was much occupied in nursing her mother and sister, both of whom died in 1873. Notwithstanding these drawbacks, her pulmonary disease is at least quiescent, if she is not recovering from it. During this time she has not had an epileptic fit, nor has she manifested a tendency to any other nervous disease.

The influence of gestation upon epilepsy has been noticed by numerous writers. These observations, however, refer to women who were epileptic before becoming pregnant, and in whom the severity of the disease was either increased or diminished (generally the latter) during the existence of pregnancy. Cases of epilepsy, produced by pregnancy, are comparatively rare. Andrée gives the history of a woman who became epileptic in her two pregnancies. In both instances—like the woman whose history I have related to-night—she aborted.

Tissot met with two cases of this form of disease. In one the fits occurred almost every week until the motion of the child was felt, while the other woman had a fit every month during the first two pregnancies. Echeverria states a single case in which the patient was epileptic during seven pregnancies, though she had no paroxysms during labor. Nor was there any difficulty in the labor of Griffin's patient, who was pregnant for the first time, and who had fits every six weeks during the early part of her gestation, though their frequency was greatly increased towards the end of her time.



La Motte has recorded a curious case of epilepsy during pregnancy, in which the sex of the child in utero appeared to have an influence in producing the disease. She was pregnant eight times, and only had fits when carrying a male child. Van Sweeten mentions a similar case.

In none of these cases, so far as we are able to determine by access to their records, did the epilepsy become permanent ; but Malgaigne has published the account of a patient who had epileptic fits for the first time during pregnancy, and who had them ever afterwards.

In regard to the relations existing between pregnancy, parturition, and epilepsy, experience and the study of the literature of the subject appear to warrant us in making the following statements :

1. Epileptics rarely have convulsions during labor. They are not more liable to puerperal convulsions than healthy women. Labor in them is, as a rule, not more unfavorable than in healthy women.

2. In the exceptional cases, in which violent epileptic convulsions occur during labor, it is not decided whether it is best to hasten delivery or to trust to nature.

3. Pregnancy may be the immediate cause of epilepsy. In these cases fits rarely occur during labor, and the disease is immediately arrested by parturition, but it will almost always re-appear whenever the woman becomes pregnant.

4. Either form of epilepsy may result in the death of the fœtus, but convulsions of this kind are not as likely to destroy the child as are those which may be correctly designated puerperal.

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NOTES ON PERIMETRIC INFLAMMATION.

BY RICHARD B. MAURY, M.D., Memphis, Tenn.

(Read before the Medical and Surgical Society of Memphis.)

THE great authority of Virchow has recognized in the dead body evidences of the existence of two distinct forms of perimetric inflammation. To these he has given the names of Perimetritis and Parametritis. The former he applied to inflammation confined to the serous or peritoneal investment of the uterus, its appendages, and the adjacent organs in the true pelvis. The latter, to inflammation of the cellular tissue in its various ramifications through the true pelvis.

The question in which we as practitioners are interested is, Can these diseases be distinguished at the bedside? or are there any known symptoms and signs by which inflammation of the pelvic peritoneum can be distinguished from inflammation of the pelvic cellular tissue?

In reference to this question gynecologists are not agreed. My appreciation of the importance of this whole subject may be conveyed by the statement that I believe the treatment of no case of uterine disease can be safely approached without a careful and deliberate consideration of the question, Does there now exist, or has there existed at any previous time in the patient's history, either form of perimetric inflammation, and especially peritonitis?

With a view of illustrating some of the forms which pelvic peritonitis may assume, and of bringing into prominence some of its diagnostic features, the following cases are selected from a number which have come under my observation.

CASE I. *Pelvic peritonitis resulting from the use of a sea-tangle tent in a patient suffering with endometritis. Large tumor filling the pelvis. Suppuration, with discharge of pus through the bladder. Recovery.*

In July, 1872, Mrs. —, æt. 35 years, married eight years,

but never pregnant, consulted me for the relief of backache, hypogastric pain, and pain under the left breast, with irritable stomach and meteorism. Menstruation occurred profusely every three weeks, and in the intervals there was an abundant muco-purulent leucorrhœa. The uterus was three inches in length, very tender on touch by the probe carried to the fundus, and readily disposed to bleed. The endometrium was very sensitive, and topical medication always painful. The endocervical membrane was inflamed, and there was extensive granular erosion of the cervix. The position of the womb was normal, its mobility was unimpaired, and there were no signs of present or previous inflammation in the peri-uterine tissues.

The usual topical and constitutional treatment was adopted, and, though her general condition was much improved and the leucorrhœa greatly lessened, the local inflammation underwent but little change.

On 9th Jan., '73, at the patient's residence, a sea-tangle tent of moderate size was easily introduced and secured by a wad of cotton. She was then put to bed, and instructed to use laudanum by enema if there was much pain. As the pain was considerable the laudanum was used, and the next morning the tent was removed by the patient after having been retained eighteen hours. At midday she was entirely comfortable, and a saturated chromic acid solution was applied with great care to the canal of the cervix only. She was again put to bed.

On the 11th a hemorrhagic discharge from the womb appeared, and on the 12th she had pain and tenderness across the hypogastrium with moderate fever. Five grains of Dover's powder every 3 hours was ordered, with hot fomentations to the abdomen and absolute rest in bed. On the 14th there was marked increase of fever. Pulse 112, small and rather feeble. There was nausea and repeated vomiting, and much complaint of desire to go to stool, accompanied with pain and rectal tenesmus. The patient was now a little delirious, and imagined she was surrounded by children. There was pain across the hypogastrium and great tenderness on pressure, but no dulness on percussion.

On the 15th she lay upon her back with the limbs flexed, or else upon her right side, nearly over on the face. The latter position being assumed because of the relief it afforded to the

feeling of pressure upon the rectum. There was frequent desire to empty the bladder, and severe pain through the hypogastric region, coming on in paroxysms. Under the influence of opium she would rest quietly for hours together, and then would have severe paroxysms of pain. The abdomen was moderately swollen and tympanitic below the navel. Vaginal examination revealed a hard and exquisitely sensitive tumor as large as a hen's egg, encroaching upon the walls of the vagina. It occupied chiefly the posterior cul-de-sac and extended thence continuously into the left lateral cul-de-sac. This swelling was separated from the cervix by a distinct groove. The uterus was immovable, and the cervix pushed forward and to the right side of the pelvis.

Jan. 18th. The abdomen is very tender and greatly swollen over its entire extent. Pulse 114. Temp. 102.5°. There is much rectal tenesmus accompanying frequent discharges of bloody mucus. Urine drawn night and morning by catheter. The vaginal tumor now occupies the anterior as well as the posterior and left lateral cul-de-sac. On the posterior wall of the vagina it comes down almost to the perinaeum and nearly fills the pelvis. The cervix is high, and can scarcely be

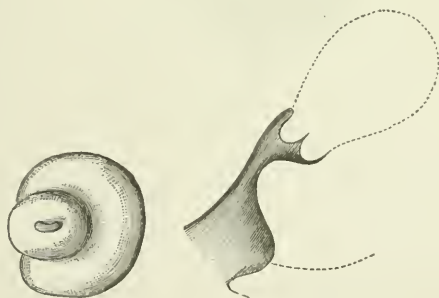


FIG. 1.

FIG. 2.

reached by the finger. Figs. 1 and 2 show the relations of the tumor to the cervix and vaginal walls. A similar condition is discoverable upon rectal examination, the tumor as felt there being hard and sensitive, and most prominent on the anterior and left lateral walls.

It now occupies nearly the whole pelvic cavity.

Jan. 21st. The abdomen is much less swollen. Across the hypogastric and iliac regions there are observable upon palpation, tenderness, fulness and resistance, but nothing whatever like a tumor.

Jan. 27. Much complaint of vesical and rectal tenesmus with pain. No abatement of either the vaginal or rectal swelling. There is no soft point about the tumor. Pulse 102. Temp. 99°. No decided sweating. No rigors. No hectic. Fig. 3 represents the state of affairs in the pelvis.



FIG. 3.

Feb. 1st. During the night pus of a decidedly fecal odor appeared in the urine. This continues to be discharged, and at the close of micturition a sharp pain shoots through the bladder. The tumor as felt through the vagina and rectum is decidedly smaller, and the abdomen less tense.

Feb. 7th. Discharge of pus continues from the bladder. Patient is rapidly improving. Tumor has greatly diminished. Cervix is now easily reached by the finger. The womb is abso-



lately immovable and the vaginal roof as hard as a board. On moderately firm pressure there is no tenderness over any portion of the abdomen except a small spot just above the pubis and to left of the median line. Percussion is resonant, but there is abnormal resistance upon palpation over the hypogastrium.

March 14th. Menstruation reappeared to-day with severe pain in hips and thighs, but without fever. Discharge of pus irregularly, and in small quantities, continues.

May 1st. The patient having entirely recovered, an examination of the pelvic organs was made. There was no tenderness over the lower portion of the abdomen. The position of the uterus was normal, but it was absolutely immovable, and the cervix was tender to the touch. The pelvic roof was as hard and resistant as if made of some solid material. The vaginal culs-de-sac were not limited by flaccid walls as in health, but the upper portion of the vagina was patulous as if forces were at work on the outside and above its roof drawing its walls apart, the contraction, no doubt, of inflammatory products effused in the pelvic cavity. The tissues at the neck of the bladder were drawn up closely under the pubic bone. The functions of the bladder and rectum were properly performed, and all discharge of pus had ceased.

April 10th, 1874. The patient being about to leave Memphis, an examination was asked for as a matter of interest with a view to seeing what changes a year had made in the physical signs. The result was as follows: Anteversion. Cervix high and posteriorly situated, and of soft velvety feel. Uterine mobility decidedly impaired. Appreciable hardness and resistance in the pelvic roof, especially in the posterior cul-de-sac. No tenderness anywhere upon firm pressure. There is extensive epithelial abrasion of the cervix with moderate leucorrhœa. General health robust.

*Remarks.*—This is regarded as a typical case of pelvic peritonitis ensuing upon and caused by the use of a sea-tangle tent in a woman suffering from corporeal and cervical endometritis. Bernutz declared that pelvic peritonitis in most cases takes its origin in disease of the uterus, and extends through the tubes to the peritoneum. In this case, the tent by its presence so irritated the already sensitive and diseased mucous membrane

as to set up an acute inflammation which travelled by continuity of structure until it invaded the peritoneum.

CASE II. *Atresia of cervix from inflammation and the use of strong caustics. Peritonitis from imprudence two weeks after the operation of hysterotomy.*

Mrs. R., æt. 37, had suffered for nearly 10 years with uterine disease, for which the cervix had frequently been cauterized. In August, '74, she applied to me for relief of dysmenorrhœa and sterility. She had recently married her second husband, and having had no children by her first marriage was anxious to have them by this. The womb was found to be in a state of moderate retroflexion, with almost complete atresia of the cervix. On the 15th of August the cervix was divided bilaterally by a Sims's knife, and the internal os was included in the incisions. The usual treatment was adopted; the patient did well; and on the 10th day after the operation was allowed to sit up.

Her improvement continued until the 14th day, when she arose soon after daylight, and walked out to the gate of the front yard in her slippers. After standing there awhile she became chilled and had to go to the kitchen fire to get warm. In a few hours she began to complain of rigors with severe pain in the womb, and of nausea. At this time the pulse was depressed, but in the evening it was 95 per minute. The nausea was incessant and obstinate, continuing for several days with vomiting. The hypogastric pain was paroxysmal in character, and at times was very severe. The countenance was pale and pinched. On the second day metrorrhagia appeared and continued for five or six days. The uterus was tender to the touch externally and per vaginam. Its position was not materially disturbed. For some days no physical signs were discoverable except exquisite tenderness to the touch in the posterior cul-de-sac.

As retroflexion was known to exist, and the patient experienced so much pain upon the gentlest pressure in this locality, I could not determine whether a swelling of very moderate size in the posterior cul-de-sac was the fundus of the womb or an inflammatory effusion into Douglas's pouch.

On the tenth day of her illness a vaginal examination revealed

satisfactory physical signs of pelvic peritonitis. The effusion into the peritoneum was small, and probably of fibrinous rather than serous character, and a perfect outline of Douglas's pouch was mapped out upon the posterior vaginal wall, the recto-vaginal cul-de-sac being distended by inflammatory products. This outline upon the vaginal wall descended to more than one inch below the level of the cervix, and was rounded at the sides, just as the lower margin of the peritoneum is in its attachment to the wall of the vagina.

This attack was of moderate severity, the pulse never going above 96, and falling in a few days to 85 per minute. Tympanites was decided but slight. Uterine mobility was impaired very early, and in a few days entirely lost.

The patient was under observation until recovery was complete. The physical signs gradually lessened, and on 10th of Oct. no evidence of peritonitis remained except the impairment of uterine mobility. She then left the city.

On 5th Jan. 1875, she was re-examined; was in fine health; the uterine canal was open and dysmenorrhœa entirely removed.

*Remark.*—The exposure which this patient underwent, the rational symptoms, and the physical signs, uterine immobility and effusion into the cul-de-sac of Douglas, clearly establish the diagnosis.

For some reason, many of the British writers on gynecology practically ignore the existence of pelvic peritonitis as a distinct disease. In the recently published excellent works of Lombe Atthill and Graily Hewitt it is not accorded recognition; and in the great work of Dr. Barnes, although its existence is admitted, no attempt is made to diagnose it, or to draw the line of distinction clinically between it and pelvic cellulitis. Indeed this author thinks that the subject of inflammation of the pelvic tissues has been worked out of late years "with superfluous critical acumen." He regards the terms "pelvic cellulitis" and "pelvic peritonitis" as being fallacious, because "they represent imperfectly, and therefore inaccurately, what we find at the bedside." The whole subject is discussed by him under the title of "perimetric inflammation."

Prior to 1857, the first of the cases just related would have been diagnosed as peri-uterine phlegmon or cellulitis; and in Dr. Simpson's Clinical Lectures on Diseases of Women there

is a diagram illustrating the manner of puncturing just such a swelling as is represented in Case I., Fig. 3, the swelling being considered by him as the result of cellulitis which had gone on to suppuration.

About the period mentioned, Bernutz published the results of his investigations, and demonstrated the fact that many cases of disease which had hitherto been regarded as cellulitis were in reality pelvic peritonitis. In a memoir, remarkable for its completeness and scientific accuracy, he presents the histories of six cases (Nos. 1, 2, 3, 6, 7 and 9) under his own care, in which a most minute and thorough account is given of the symptoms and physical signs during life, and of the anatomical appearances after death.

In all of these cases the fact was clearly established, not only that the symptoms were due to pelvic peritonitis, but also that there was no disease whatever of the pelvic cellular tissue. Through these investigations then, pelvic peritonitis was raised beyond a doubt to the dignity of a separate and distinct disease, and the fact established, not only by the cases of Bernutz himself, but also by those of other observers quoted in his memoir, that we are liable to meet with it under a great variety of circumstances, and that in many instances it may readily be distinguished from cellulitis.

The value of these investigations has been fully appreciated by some American and German gynecologists; and Prof. Thomas, in his work on Diseases of Women, has given a full and clear statement of all that is at present known concerning the clinical features of the two diseases, being enabled to do so from his own careful and extended observation of the subject, which fully confirms the views of Bernutz.

Moreover, there is no record of any autopsies (so far as I know) controverting Bernutz' results, by those whom I have mentioned as unwilling to accept them.

In studying this subject at the bedside, I was soon impressed by the fact that very wide clinical differences exist between the different forms of perimetric inflammation; and, whatever views one may entertain in regard to their pathology, that it is impossible to classify them under one head.

It will be remembered that in the very interesting investigations of Dr. Murchison into the nature of fever, a certain num-

ber of cases are put down as Typhoid, and others as Typhus, while 200 are left out of consideration altogether, because they were "doubtful cases," and could not be classed either as Typhoid or Typhus. Still they were fevers.

Now it would seem, that in the study of perimetric inflammations, some cases can be diagnosed with certainty as pelvic peritonitis, and some as pelvic cellulitis, while others cannot be classified with either, because our present knowledge does not furnish us the means of an accurate diagnosis.

And yet because these "doubtful cases" do occur, we should not be less anxious, or consider it less important to make an accurate diagnosis in the others.

Many years of labor have been expended by the best medical talent in Europe and in this country in drawing exact lines of difference between Typhoid and Typhus, and yet the practical importance of a differential diagnosis in its bearing upon prognosis and treatment is not a whit less in perimetric inflammations than it is in the different forms of fever.

Admitting the very great importance of the rational symptoms, and a thorough knowledge of the antecedent history, in any given case of perimetric inflammation, it may be assumed that an accurate diagnosis is impossible without a physical exploration of the pelvic organs.

Dr. Thomas has aptly drawn an analogy between the serous and parenchymatous inflammations of the thorax and those which invade the pelvic tissues.

Following out this analogy, it may be added that just as our knowledge of the diagnosis of pulmonary diseases was vague and unsatisfactory until developed by auscultation and percussion, so any further advance now in our ability to diagnose perimetric inflammations, is to be accomplished only through the careful comparison of the physical signs during life with the appearances of the diseased tissues after death.

But the value of physical signs cannot be appreciated without a correct knowledge of two subjects: first, the relations of the peritoneum to the pelvic viscera; second, the distribution of the pelvic cellular tissue.

In order to apply the above remarks, let us examine one feature of the first case whose history has been presented. At an early period a tumor began to form in the posterior end-



sac and extended to the left lateral and anterior culs-de-sac. It was separated from the cervix by a groove or furrow. Being slight in front and at the side, it remained nearly stationary in those localities, but enlarged very rapidly behind, and soon occupied the greater portion of the pelvic cavity. Its position was between the rectum and vagina. It encroached largely upon both these canals, and extended downwards to within two inches of the perinæum. Moreover, it did not extend upwards, and was not appreciable above the pelvic brim.

In the six cases of Bernutz, already alluded to, there was marked uniformity in the physical signs. There was a tumor of variable size presenting in several of the culs-de-sac, always in the posterior, separated from the cervix by an appreciable furrow, and extending principally on the posterior wall of the vagina downwards toward the perinæum. Uterine displacement and immobility were constant. Above the pelvic brim no tumor was appreciable. The whole disease was confined within the walls of the true pelvis.

The tumor in my case, represented in Fig. 3, could not by any anatomical possibility have been formed at the expense of the pelvic cellular tissue, and the autopsy in all of Bernutz's cases showed that it was the result of an encapsulated effusion of serum in the recto-vaginal cul-de-sac.

We cannot suppose for a moment that the thin layer of cellular tissue which intervenes between the posterior wall of the vagina and the peritoneum, or that between the anterior rectal wall and the peritoneum, or the small mass of that tissue on the posterior wall of the cervix, is susceptible of such enlargement from inflammation as in a few days to nearly fill the pelvis.

But a glance at the anatomy of the recto-vaginal cul-de-sac, and the relations of the peritoneum to the rectum and vagina at that point, show at once the importance and meaning of the tumor as a physical sign.

In the discussion of the physical signs of perimetric inflammation I think that due attention has not been given to this anatomical fact, viz.: that the peritoneum, after covering the posterior uterine wall, descends between the vagina and rectum at least as far as the level of the lower margin of the cervix, and often much farther. On the contrary, most of our gynecological text-books either do not dwell upon this point at all, or else

give an incorrect representation of it. For example, out of twenty-seven beautiful illustrations of the pelvic organs in Dr. Graily Hewitt's excellent work on Diseases of Women, there is not one in which the pelvic peritoneum, in this most important particular, is represented accurately, or in a degree approaching accuracy. With such notions of the anatomy, as it is herein given, how is it possible that a proper construction could be placed upon the tumor as a physical sign?

In a valuable contribution to the Etiology of Uterine Flexions, by Dr. Ludwig Joseph, a translation of which was published in the *AMERICAN JOURNAL OF OBSTETRICS* for November, 1873, there are to be found the following remarks upon the anatomy of Douglas's cul-de-sac. "Although Virchow has repeatedly for the last twenty years given the most definite accounts of the course of the peritoneum, still even in the best text-books entirely incorrect views are to be found. All at present admit that the peritoneum passes in front from the internal os to the bladder. Widely different are the accounts of the passage of peritoneum on to the rectum."

After quoting the accounts of Luschka and Henle, Klob, Hyrtl and Hüter, he says, "All but Henle, therefore, concur in saying that the peritoneum does not extend to the rectum from the uterus, but from the vagina, wherefore the excavation should be called recto-vaginal, or, as Rokitansky occasionally does, recto-vagino-uterine. According to my numerous investigations the peritoneum extends to the vaginal wall at a point corresponding to the lower border of the posterior lip of the cervix, generally lower still, as far as below the external os, and even below a horizontal line drawn from the anterior laquear."

A correct drawing of the peritoneum is to be found in Gray's Anatomy, in Savage's Plates on Female Pelvic Organs, and in the Atlas of Bourguery and Jacob.

But all question in reference to this anatomical point is at once settled by a pathological specimen presented by Dr. Sims in reporting the results of his experiments with drainage tubes in ovariectomy. In the case alluded to, the drainage tube, for a special reason, was not passed as in his other cases, at the lowest point of Douglas's cul-de-sac, but through the vaginal roof immediately behind the cervix. The patient died, and the au-

topsy showed that the peritoneum was not drained, because its duplication took place at a point so much lower than that at which the tube passed as to allow the accumulation of nearly 5 oz. of reddish serum in Douglas's pouch. Dr. Sims's report on



FIG. 4.

this subject, published in the *New York Med. Journal*, presents a photographic representation of the parts after removal from the body. Fig. 4 is a correct outline of the same. The notes of the autopsy say that "Douglas's cul-de-sac extended down between the rectum and vagina an unusual depth, to within two inches of the perinæum."

From clinical observation I am quite sure that similar extensions downward of the peritoneum are not uncommon.

It may then be safely asserted that if the case is one of pelvic peritonitis, with liquid effusion, and a tumor is formed, it must of necessity appear in Douglas's pouch, and be appreciable in the posterior cul-de-sac of the vagina, and if the effusion be considerable it must come down below the level of the cervix.

In explanation of the tumor which appeared in the anterior cul-de-sac, and which was part and parcel of that in the posterior and lateral culs-de-sac, it is sufficient to mention that the peri-

toneum, in being reflected from the anterior wall of the uterus upon the bladder, forms the utero-vesical pouch which is capable of holding at least an ounce of fluid, as shown by post-mortem demonstrations after ovariectomy.

With a correct knowledge, therefore, of the anatomy of the pelvic peritoneum and the distribution of the pelvic cellular tissue, it is easy in many cases to make a diagnosis between peritonitis and cellulitis.

But the physical signs just mentioned as characteristic of pelvic peritonitis, with effusion, are identical with those which may appear in pelvic hæmatocele, with this one exception : that while in pelvic peritonitis the tumor is confined strictly to the true pelvis, in hæmatocele it may, if the hemorrhage is considerable, reach as high as or above the umbilicus.

In making this statement, it is admitted that an abdominal tumor may be formed as the result of peritonitis, but not of pelvic peritonitis, unless this disease should extend beyond the limits of the true pelvis and invade the cavity of the abdominal peritoneum. In many cases the diagnosis between peritonitis and hæmatocele is readily made, but in others it is extremely difficult, and may require a resort to the aspirator.

Aran declared that pelvic peritonitis never forms an abdominal tumor. My comparatively limited observations confirm this statement, and I know of no recorded cases which contradict it. Out of twenty-three cases of pelvic peritonitis reported in the *Memoirs of Bernutz and Goupil* there is but one which seems to be an exception to this rule. I allude to case XXVI. Bernutz thought this case proved Aran's position incorrect. At the risk of being regarded presumptuous, I think that an analysis of this case will impress one with the doubtfulness of Bernutz' diagnosis. He himself admits that it might easily have been taken for hæmatocele. His diagnosis of peritonitis appears to have been based chiefly upon the absence of the rational symptoms of hæmatocele, and upon the discharge of pus through the rectum. The patient did not die, and consequently the diagnosis was not proven.

Now, no one knew better than Bernutz how often hæmatocele fails to make itself known by rational symptoms. Out of twenty-eight cases of hæmatocele reported in his memoirs, the diagnosis being confirmed in twenty of them either by puncture or au-

topsy, symptoms of hemorrhage were presented in eight only. Upon this feature of hæmatoceles he especially dwells, and shows how very often anything like syncope is absent, even in the formation of large hæmatoceles.

According to his observations, syncope or collapse occurs, as a rule, only in those hæmatoceles which are due to rupture of the ovary, while it is uncommon in menstrual hæmatoceles.

Without further comment upon case XXVI. I will relate one which I think was identical in character with it, both having their origin in sudden suppression of menstruation.

CASE III. *Menstrual pelvic peritonitis resulting from exposure and sudden suppression of catamenia. Rational symptoms of internal hemorrhage followed by the formation of a tri-lobed abdominal tumor. Pus discharged through the rectum, but the tumor disappeared in part only.*

In April, 1874, I saw, in consultation with Dr. J. Chandler, the wife of a merchant with the following history: In August, 1873, the absence of her husband made it necessary for her to attend personally to the business of his store. One evening, after having suffered all day from the excessive heat of the weather, she retired to her room and laid aside all her clothing except the chemise. Attired in this garment only, she took a seat after night on the balcony in the open air. She was menstruating at the time. There was a strong breeze blowing, and she remained out until nearly midnight. The next morning she felt sick, and discovered a complete suppression of menstruation. During the day she was seized with rigors and sharp abdominal and pelvic pains. Fever rose, and she became very ill. After the lapse of five or six days she began to have attacks of great prostration accompanied with pallor of face, coolness of surface, and almost complete syncope, during which her sister, who was in close attendance upon her, and her friends thought she would die. Soon after this, she cannot say how long exactly, a tumor was discovered on the right side of the abdomen. This gradually increased in size and extended towards the left side. In September pus began to be discharged through the bowel in considerable quantity. This was accompanied by a diminution of the abdominal swelling, but the tumor did not disappear. This purulent discharge has con-



tinued with some intermissions until the present time, a period of nearly eight months. The patient said that she had suffered from deranged uterine health for several years; that her menstruation had been irregular, appearing every two or three weeks, and she had had no children for five years.

Since the menstrual suppression in August, the courses had appeared four times, at an interval of two weeks. For the past three months menstruation nearly normal, but accompanied by slight fever with evening exacerbations.

There is observed a tri-lobed abdominal tumor, most prominent on the right side, least so in the middle, and rising half way to the umbilicus.

A vaginal examination resulted as follows: There was no pelvic tumor. The pelvic roof was hard, but not tender. The hardness not localized, but general. The uterus not displaced, but immobile. Cervix large and hard. Os uteri enlarged.

We were of the opinion that the patient was first attacked with menstrual pelvic peritonitis, which soon after was complicated with hæmatocele. The peritonitis resulted in suppuration, and the pus was discharged through the rectum—the hæmatocele had undergone only partial removal.

The patient was under my observation for a short time only, but I learned from Dr. Chandler two months afterwards, that pus was still discharged in small quantities, and that the tumor had undergone but little change.

The syncope and collapse were well-marked symptoms, as the husband and sister both testified.

In the case reported by Bernutz, the tumor was discovered on 17th September. Pus began to be discharged on the 24th Nov.; but on Feb. 4th, after all discharge had ceased, the tumor, though diminished in size, still remained.

Therefore, I conclude that Aran's law is correct, and that Bernutz' large experience has furnished no exception to it.

One more case will now be related in which the recognition of the pelvic peritonitis, by preventing all local interference, probably saved the patient a violent attack of inflammation.

CASE IV. *Chronic peritonitis causing ante flexion and giving rise to a tumor on the right side of the uterus.*

Mrs. A. B., æt. 23 years, residing in Arkansas. Examined on

20th Dec., 1874. Has been married a little over three years. Prior to marriage her health was good, and menstruation normal in every respect. Three months after marriage, while running up a hill she fell. The same evening severe paroxysmal pains in the pelvis set in. For a week she suffered excessively, and was confined to her room by the severity of the pains. At the end of this time menstruation appeared, it being just four weeks from the date of the previous menstruation. The hemorrhage was free, painful, and accompanied with clots, and the attending physician expressed the opinion that a miscarriage had occurred; but the patient stated that nothing passed from her by which this opinion could be verified.

After this she was free from suffering until the next menstruation, when she again had severe pain. These attacks returning and being prolonged, and her general health beginning to suffer, she at once sought medical aid away from home. For a period of two years she was under the treatment of physicians in several of the large cities of this country. At a celebrated water-cure she was treated for "Irritable Uterus" during a period of eight months.

For some months before I saw her, she had been undergoing that peculiar course of gymnastic exercises known as the Movement Cure.

Under all this she was gradually, but steadily, losing ground. Her general health was now very poor. Hysterical attacks have become very frequent, and she is a great sufferer from acid dyspepsia.

Menstruation, though regular at present, has during the past three years presented every kind of irregularity—sometimes delayed and sometimes in advance of the proper time. Occasionally free and protracted, at other times scanty and lasting but a day or two.

Although slight backache and pelvic uneasiness precede the appearance of the menses, all pain ceases when the discharge begins; and during the continuance of the flow she is free from suffering. When, however, menstruation is over, pain begins and continues for eight or ten days. This pain is not continuous, but comes in paroxysms; it is deep in the pelvis, runs down into the right thigh, and is often attended with nausea.

An examination on the 20th Dec. resulted as follows: Mark-

ed ante flexion of both neck and body. Uterus low in the pelvis, and cervix resting upon its floor. No tenderness discoverable in any of the culs-de-sac except the right postero-lateral. Uterine mobility somewhat impaired, but not enough so to prevent the womb being thrown into a position of moderate retroversion by bimanual manipulation. Putting the organ in this position necessarily brought the cervix forward, and while it was thus held by the left hand, making pressure against the fundus through the abdominal wall, the fingers of the right hand, introduced into the rectum, discovered on the right side of the womb a hard, tender, and very sensitive tumor, the size of an English walnut. This tumor extended from about the level of the os internum to the fundus. It was attached to the womb on its lateral and posterior surfaces, and was therefore most satisfactorily explored through the rectum. A distinct furrow of separation could be felt between it and the womb.

Four days afterward a second examination was made with the same results. The tumor moved with the womb, was evidently attached to its side, and was remarkable for its hardness and tenderness on pressure. It could only be fully explored by bimanual palpation through the rectum.

Was this a fibroid, a dislocated adherent ovary, or a peri-uterine deposit, the result of pelvic peritonitis? I was inclined to regard it as a dislocated ovary, which, through a partial peritonitis, had contracted adhesions to the womb. The examination showed the depth of the womb to be two and three-quarter inches, and the uterine canal readily allowed the passage of the large sound.

The situation was explained to the lady and her husband, and the danger of local treatment under the circumstances pointed out. Absolute rest in bed for a couple of days was enjoined. In eight or nine hours afterward she had a chill with slight fever and general aching and uneasiness. I then learned that during the past two years she had had from time to time rigors with slight fever, which were repeated every day or two for many days. On the second and third evenings after my examination, the chilly sensations returned in the evening, and were followed by fever. She was persuaded to keep her bed for several days, but she did so reluctantly, saying that she had had similar attacks before, and never paid much attention to them.

Menstruation now came on, and she got through it quite comfortably, though she complained of soreness in the pelvis and occasionally of pain. The evening of the disappearance of the menses, she went out to call on a friend who was passing through the city on her bridal trip. When she returned, she had a decided chill, followed by fever and nausea. The old pains now set in and continued during the fever, which lasted for eight days. The pulse ranged from 96 to 110 per minute, and the thermometer as observed did not go above 102.5° F. The womb became very tender, and its mobility was much impaired. A distinctly marked swelling appeared in the posterior vaginal cul-de-sac; this was very tender to the touch, and gave rise to great pain in defecation. The calls to stool became very frequent, and she complained that the passage of feces was obstructed by the sudden pressure of something upon the rectum. The bladder was very irritable.

A blister was applied over the hypogastrium, the fever subsided, the swelling and tenderness disappeared. The patient had had a reaccession of peritoneal inflammation.

She was kept in bed. Fly-blisters were applied at intervals of five days, and hot vaginal douches used three times a day. Menstruation, which should have appeared on the 25th January, was delayed until 4th February, and continued until 9th. The pains did not follow it, and she did well.

On the 15th February a careful examination of the pelvic organs was made. Uterine mobility in a great measure restored. The tenderness formerly felt in the right cul-de-sac was scarcely appreciable, but to the exploring finger there was given a slight sense of resistance. On exploring the posterior surface of the womb through the rectum, the tumor was discovered, but very much changed. Its hardness, which previously resembled that of a fibroid, was gone; its size was appreciably lessened, and it was scarcely tender at all.

There was still hardness, fulness, and tenderness in Douglas's pouch, though not so marked as at previous examination. Fly-blisters at intervals of a week and the hot vaginal douche were continued up to the next menstrual period, which occurred on 4th March. This menstruation was attended with slight pains, but with no febrile disturbance. It was not followed by the pains which hitherto had harassed her for eight or ten days.

An examination was made on 12th March; and though evidences of peritonitis were still to be found in the diminished mobility of the uterus and in the inflammatory deposits in the retro-uterine pouch, the tumor had quite disappeared.

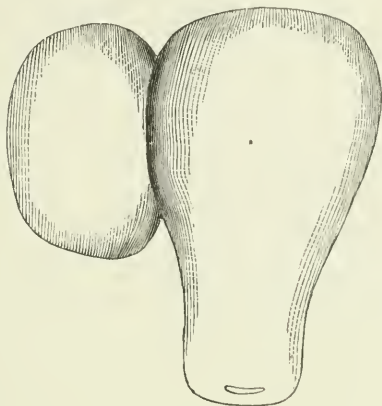


FIG. 5.

This case was regarded as one of chronic pelvic peritonitis, and the tumor as a result of the inflammation. Fig. 5 represents the position and comparative size of the tumor. Repeated attacks of inflammation have occurred by the patient's mode of life, and perhaps from the treatment, as she says one of her physicians used intra-uterine injections which gave her great pain. She has taken much exercise on horseback, and has never known the necessity for caution in such matters.

In reference to the tumor I would add that, so far as my information goes, Grisolle was the first to appreciate the existence of circumscribed swellings in contact with the womb in peritonitis. After him, Aran and Bernutz both recognized them, and distinguished them from the phlegmons of the broad ligament.

In the foregoing remarks my object has been to show that in a large proportion of cases an exact diagnosis can and should be made. My own observations, which have been carefully made during the past five years, confirm the conclusions of Bernutz. It is true, I have had no opportunity to make autopsies, but the clinical features of those cases which have fallen into



my hands accord so entirely with those delineated in his memoirs, that I feel warranted in believing that their pathology is the same.

NOTE.—This patient, after an absence of four months, returned to Memphis, and was examined on 25th of June. No signs of a tumor were discoverable; its former position was marked by that feeling of resistance which is usually conveyed to the finger by cicatricial tissue. The dyspepsia, the deranged menstruation, and the attacks of pain continuing for eight or ten days after menstruation, have all disappeared. Her health is restored.

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## DIPHTHERIA.

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REMARKS UPON CASES IN THE CHILDREN'S CLINIC, SUMMER COURSE, BELLEVUE HOSPITAL MEDICAL COLLEGE.

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BY J. LEWIS SMITH, M.D.

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GENTLEMEN:—Two cases of diphtheria, and two of diphtheritic paralysis have now been presented to you in these clinics. These cases offer no features of special interest, but they furnish the occasion to make some remarks on this very interesting, and at present common and fatal disease. Some of the more important facts relating to the causes and nature of this malady which have been either fully established or rendered highly probable, may be stated as follows:

1st. Diphtheria is a local malady in its commencement in most instances, occurring from lodgment of the diphtheritic poison at some point upon the mucous membrane, or upon the skin denuded of its epidermis, or upon an open sore. When thus localized it may, by proper local treatment applied early, be cured, and the system remain unaffected.

2d. When diphtheria has a local commencement, infection of the system occurs by absorption of some of the poisonous substance, through the absorbents or capillaries, or both, which connect with the seat of the disease upon the surface. What this substance is which thus infects the system and produces the constitutional symptoms of diphtheria is unknown. Much confusion and difference of opinion exists in regard to it. The following are theories respecting it: that it is a virus which is

peculiar (diphtheritic) and quite distinct from the bacteria; that it is bacteria; that it is septic poison, absorbed from the inflamed surface, and not different from the poison in ordinary septicaemia. And then there are the different views in regard to the nature of the septic poison, that it is the bacteria, a secretion of the bacteria, etc. What it is which produces the external inflammations of diphtheria, and what it is which infects the blood and the relations of this substance to bacteria on the one hand, and to septic poison on the other, must be determined by future investigations.

3d. Acute cervical adenitis and cellulitis, producing tumefaction along the neck, are of grave import in diphtheria, since they show that the poison has entered the lymphatics, and infection of the system is inevitable. They sustain the same relation to diphtheritic pharyngitis as the bubo sustains to a chancre, or as adenitis in the axilla to a poisoned sore upon the hand or arm.

4th. There can be little doubt that the diphtheritic poison sometimes enters the system through the lungs in inspiration. My friend Dr. Heitzmann informs me that he made the post-mortem examination of a child who died within the first day of diphtheria, which was prevailing in the family. The examination was made soon after death, and portions of the lungs were placed in a solution of bichromate of potassa to prevent decomposition. He observed bacteria under the microscope in the minutest bronchial tubes, and no pseudo-membrane could be discovered on any of the mucous surfaces. The presence of bacteria in the lungs at so early a period indicated that the diphtheritic virus had probably entered the blood through these organs.

We therefore recognize two modes of systemic infection, namely, by inoculation upon one of the tegumentary surfaces; and through the lungs,—modes in which it is well known that certain other acute infectious diseases are, or may be, communicated, as for example, scarlet fever and variola.

5th. In whatever way the virus enters the system, it is specially attracted to the fauces, and therefore pharyngitis is commonly its earliest and most severe local manifestation.

6th. It is customary in medical treatises to classify diphtheria among the acute infectious diseases, along with scarlet fever and

measles. Unlike those diseases, however, it often occurs in a secondary as well as a primary form. It is an interesting and important fact that diphtheria instead of being incompatible with other distinct morbid processes, sometimes engrafts itself upon them, especially upon the other acute infectious diseases. "Diphtheria," says a foreign writer, "develops very rapidly under the influence of poisonous miasms—during the prevalence of hospital gangrene, putrid fevers, and bad epidemics of typhus fever, and under these circumstances it not infrequently reaches its highest point of virulence and its widest extent." In this city most cases of secondary diphtheria occur as complications of scarlet fever and measles. The mortality, indeed, of these eruptive fevers is greatly increased by the frequent supervention of diphtheritic inflammation upon the fauces or in the larynx, in cases which would otherwise do well. An interesting fact I have several times observed, namely, that diphtheria originating upon the inflamed surface in scarlet fever or measles, may become dissociated, and spread as an independent malady. Thus in one family three children affected with severe anginose scarlet fever, took also diphtheritic pharyngitis before the efflorescence on the skin had disappeared. A few days subsequently diphtheritic pharyngitis appeared in the father without scarlet fever.

7th. Certain recent writers (Geo. Johnson and others) state that "membranous croup and laryngeal diphtheria, as we now see them, are one and the same disease" (London *Lancet*, Jan. 16, 1875). There can, however, be no doubt that there is a membranous croup which is quite distinct from diphtheria. I saw many such cases in New York prior to 1858, when there had been no diphtheria in the city for many years. In no one of these cases was there the history or any evidence of contagiousness; but, on the other hand, as they occurred singly, the proof was strong of their non-contagiousness. Nevertheless, at the present time, when the diphtheritic poison is so abundant in the atmosphere, we certainly have few cases of membranous croup which are not diphtheritic, or do not become such. It is not improbable that the exudat of true croup affords a nidus in which the diphtheritic virus lodges and multiplies so as to transform a simple croupous into a diphtheritic inflammation, just as we have seen scarlatinous pharyngitis becomes diphtheritic. In

no other way can I explain the comparative infrequency of croup as we observed it in former times.

Discoveries in reference to the nature of diphtheria obviously lead to changes and improvements in the treatment. The following treatment I have found much more successful, both in private practice and in the institutions with which I am connected, than the treatment mainly or entirely by tonics, which was formerly in vogue.

#### PREVENTIVE MEASURES.

The diphtheritic virus, like the scarlatinous, may remain for weeks or months in a locality, or in apartments, notwithstanding the use of the ordinary disinfecting and sanitary measures. In East 55th street two families resided in a brown-stone house, the sanitary condition of which was apparently good. In December, 1874, diphtheria occurred in one of these families, who occupied the lower floor and the basement, causing the death of two of the children. The other family, in order to escape the danger, immediately removed to another part of the city, where they remained two months, returning home on March 6th. On March 14th and 15th, eight and nine days after the return, their two children, aged  $2\frac{1}{2}$  and  $4\frac{1}{2}$  years, who had been allowed free access to the room in which the fatal cases had occurred, also took severe diphtheria, one of them dying.

In another family, living in the suburbs of New York, the mother contracted diphtheria from her brother's child, who died of the malady a few blocks distant. Returning home, she occupied a small room, remaining constantly in it, and by prompt local treatment was soon convalescent. Her only child, a boy of six years, was excluded from her companionship about one month, after which he was allowed to enter the room, and slept in it. Within a few days, namely, thirty-five days after it commenced in the mother, the diphtheritic patch appeared upon his fauces. In one of the asylums of this city diphtheria has been prevailing more than a year, the cases occurring mainly in one of the buildings, and with so little break or intermission that it appears that the diphtheritic virus has not been eradicated from one or more of the wards since the first case occurred. Such instances show the danger of admitting children into rooms where diphtheria has occurred, until a considerable

period has elapsed, and thorough disinfection has been employed.

When diphtheria is prevalent, indisposition on the part of a child, and especially febrile symptoms, or defluxion from the nostrils, should at once arrest attention. Although there is no complaint of soreness of the throat, the fauces should be carefully inspected, and if they seem too red, frequent gargling with one of the chlorates should be prescribed, or if the patient is too young to gargle he may swallow the solution, care being taken that the quantity swallowed does not exceed from two to four grains every hour or second hour. If the redness be considerable, and especially if a little whitish substance, whether a secretion or exudation, appear in the depressions over the tonsils, it is safer, in addition to the use of the chlorate, to brush the fauces with the carbolic acid mixture presently to be described, two or three times daily, or oftener.

If diphtheria occur in a family, not only is prompt isolation from the other children imperatively required, but the fauces of these children should be examined daily, and if the least evidence of inflammation appear, the treatment recommended above should be immediately employed. By such precautionary measures there can be little doubt that much of the diphtheria which is now so fatal might be prevented.

Does quinine exert in any way, or to any extent, a controlling influence over the diphtheritic virus? My observations do not enable me to give a positive answer. I can, however, recall to mind a few instances in which children, who had been exposed to diphtheria from its presence in the family, took quinine in moderate doses each day, as a preventive, and although the disease appeared in them after a few days, its type was mild, while I recollect no instance in which the malady occurring under such circumstances was severe. I therefore think favorably of the use of quinine as a preventive in children who are so exposed to the diphtheritic virus that there is a strong probability that they will contract the malady, although I believe it is not so important or necessary as a strict surveillance of the state of the fauces, and the employment of topical remedies as directed above.

*Treatment.*—It is obvious, if the views expressed in regard to its pathology are true, that the early topical treatment of



diphtheria is of the utmost importance. Whatever may be our opinion in regard to the nature and causes of diphtheria clinical observations teach us that the gravity of this malady is in most instances proportionate to its local manifestations, at least in the commencement of the disease. Now we certainly have it in our power to control greatly these manifestations, namely, the diphtheritic inflammations, diminishing their extent and intensity, and checking or diminishing the fibrinous exudation. If, by our treatment, we can limit the exudation to a small surface, or can remove it so that the inflammation from croupous becomes catarrhal at an early stage of the malady, the patient is probably safe. This is a general fact in reference to the treatment of diphtheria which is abundantly established by clinical experience, and which of itself justifies local treatment designed to moderate the inflammation. But there are certain special benefits to be derived from local remedies which are so important, that in my opinion no one can properly treat the disease who does not fully appreciate them. Both clinical observations and experiments on animals have shown us that the diphtheritic pseudo-membrane contains the specific virus in a very inoculable and energetic state, and the air as it passes over the membrane becomes more or less impregnated with the poison. Hence the source of the great danger which exists, not only of the communication of the disease to others, but of auto-infection, for inflammation of the larynx ordinarily occurs secondarily to that of the fauces, and probably at least in many instances by the transference of the virus from the surface of the latter to that of the former during inspiration. Prompt treatment, therefore, of the fauces or of the nostrils by disinfectants is the most reliable means which we possess of preventing the occurrence of that fatal form of diphtheritic inflammation, namely, the laryngeal, in one who has diphtheria.

Another object which we may expect to accomplish by local treatment, if the inflammation is upon a surface which is accessible, is the prevention of blood poisoning, whether this poison is the bacteria, or a secretion of the bacteria, or a substance which is developed independently of these organisms, though associated with them. Since I have inspected the fauces more carefully and frequently in scarlet fever and diphtheria, and have made use of local treatment whenever any whitish sub-

stance secreted or exuded, appeared over the tonsils. I have much less frequently observed extensive swelling along the sides of the neck, which, as we have said, originates from and indicates the passage of the poison along the lymphatics of the neck into the system, and which is therefore so generally prognostic of an unfavorable ending.

In certain cases the proper employment of local measures, even when the inflammation is upon a surface which in ordinary instances it is easy to treat, is difficult or impossible. Thus in my practice, a little girl of 11 years died after a sickness of only four days, with no treatment or even inspection of the fauces, on account of the fierce resistance which she made ; and cases are more frequent of difficulty in the proper treatment of the nostrils. But such instances are exceptional. Ordinarily with a little tact, and with the use of a proper instrument, the application can be made quickly and sufficiently to either the faucial or Schneiderian surface.

Local treatment should not be painful. The day of escharotic and powerfully irritating applications to the throat has passed, and the expression, "burning the throat," so often heard in families, is a misnomer as applied to the treatment of the present time. It is ordinarily best not to attempt to tear off the membrane, for its forcible separation irritates the inflamed surface, and promotes hemorrhage. Whichever disinfecting substance we employ, should be applied in such a way that it penetrates the pseudo-membrane, and if possible touches and bathes the surface underneath. I prefer making the application with a large camel-hair pencil rather than with the sponge, which is more irritating and which applies a less quantity of liquid to the fauces.

Unfortunately, in many instances in private practice the full benefit of local treatment cannot be obtained, because the physician is not summoned till the malady has continued for a day or more, and the system is perhaps infected at the time of his first visit. In order to ascertain the full benefit which can be derived from local measures, statistics should be obtained of cases treated from their commencement, or within a few hours from their commencement. Such statistics are furnished by the Catholic Foundling Asylum of this city. Diphtheria has prevailed in this institution during 1874, and up to the pre-

sent time (June 1) in 1875. The Sisters having had more than a year's constant experience with the disease, detect the initial symptoms, examine the fauces, and have commenced the local and general treatment before the daily visit of the physician. In this institution in the first five months of 1875 thirty-two cases of diphtheria occurred, and of these cases only six died; three of laryngitis, and three of blood-poisoning. One of the six fatal cases ought in fairness to be excluded from the statistics, as it was admitted into the asylum on the sixth day of the disease. Possibly now and then a case might have been under treatment which was not true diphtheria, but a large proportion of the cases I saw and examined myself, and even if the doubtful cases were rejected it would not materially change the proportion of recoveries.

I will briefly outline the mode of treatment employed with so good a result in the Foundling Asylum, and by which in my private practice during the last year I have certainly saved a much larger proportion of cases than I had been able to cure by any other measures which I had previously employed.

As soon as the case comes under observation the following mixture is applied every second or third hour over the fauces by one or two applications of a large camel's-hair pencil.

R. Acid. carbolic.....	gtt. vi.-x.
Liq. ferri subsulphatis.....	3 iij.
Glycerinæ.....	5 j.
	Misce.

If there is discharge from the nostrils indicating diphtheritic inflammation of the Schneiderian membrane, a little of the same mixture diluted with an equal quantity of warm water is injected into each nostril every three to six hours. In doing this the child is placed upon its back, with the head thrown backward and the eyes covered by a towel, to prevent the liquid from entering the eyes. A small glass ear or nostril syringe, with a knob or button at the end of the nozzle, is the best form of instrument for these injections. One-third to one-half of a teaspoonful of the diluted mixture is a sufficient quantity to employ for each nostril. This application properly made, prevents decomposition, removes the offensive odor, and, which is of the greatest importance, prevents blood-poisoning;

it immediately arrests the movements of the bacteria, and probably destroys them, as I have observed in experiments with the microscope.

Quinine, in doses of one to two grains, according to the age and severity of the case, is administered about every fourth hour, and each hour in the interval half a teaspoonful to one teaspoonful of the following:

R. Potas. chlorat.....	3 i-ij.
Tinc. ferri chlorid.....	3 i.
Syr. simplic.....	5 iv.
	Misce.

A little chlorine is set free in the above mixture, and the quantity may be increased by adding a few drops of dilute muriatic acid. No drinks are allowed for a few minutes after its administration as well as after the use of the brush, so as not to wash it away too quickly from the fauces.

In three or four days, if the case progresses favorably, these remedies are employed less frequently, but they are not discontinued until not only the pseudo-membrane has disappeared, but the inflammation has in great part abated. For not infrequently the fibrinous exudation reappears after it has been totally removed, if the pharyngitis remain. Thus I have known it to reappear after it had been absent an entire week, or even longer. Hence also the need of daily inspection of the fauces until convalescence is well advanced. When the inflammation has begun to abate, and there is no reappearance of the exudation, a gargle or drink of chlorate of potash in water usually suffices for topical treatment.

Such is the treatment, substantially, which has proved so successful in the Foundling Asylum. From my observations of its effects not only within this institution, but in private practice, I can confidently recommend it.

The employment of tonics, especially of quinia and iron, in the treatment of diphtheria is almost universal in the profession. Our reliance must be upon these agents in those cases in which the system is infected from the first or from an early date, more than upon topical remedies.

Thus one of the fatal cases in the Catholic Foundling Asylum was a girl aged  $3\frac{1}{2}$  years, who sickened with diphtheria on

March 25th, 1875. On the 26th her pulse was 160, temperature  $102\frac{1}{2}^{\circ}$  and a diphtheritic patch had appeared over the right tonsil. On the 28th there was a free muco-purulent discharge from the nostrils, with a temperature of  $100\frac{3}{4}^{\circ}$ , and a pulse of 128. The features were pallid and flabby, presenting the appearance of profound cachexia. On this day free epistaxis occurred after the use of the syringe, although it was employed gently; subsequently, repeated hemorrhages occurred. On March 31st the skin was cool; although milk-punch was liberally employed, the temperature was  $101^{\circ}$ , and the pulse 88. Death occurred April 1st from the cachexia. Her cough throughout was slight, and the respiration without embarrassment. At the autopsy, the mucous membrane of the larynx, trachea, and bronchial tubes was found uniformly and greatly injected, but without any fibrinous exudation; lungs healthy, except quite large extravasations of blood in the posterior part of one lung; appearance of heart normal, and small clots in each of its ventricles; other organs of the trunk (spleen, liver, kidneys, etc.) apparently normal; urinary bladder contracted, and nearly empty. The urine in this case, which was examined a day or two before death, either contained no albumen, or only a trace. The connective tissue behind the angles of the lower jaw, which had been tumefied during life, presented a deep red color; also with extravasations of blood. The spleen and half a kidney, placed in a solution of bichromate of potash immediately after their removal from the body, were examined microscopically by Dr. Heitzmann, but no bacteria or anything abnormal was discovered in them. In cases like the above, local treatment, however early employed, will probably fail to prevent contamination of the system, either because this has already occurred before the inflammation appears and the disease is manifested, or because the inflammation from which the system becomes infected is upon a part which is concealed from view, and is not therefore detected and treated sufficiently early. But as such cases are exceptional, they furnish no argument against the employment of local measures in the treatment of ordinary diphtheria.

When the inflammation abates, and the pseudo-membrane no longer reappears, if the patient is not speedily restored to health the poison has entered the system. Pallor, loss of



strength and appetite, flabbiness of the flesh, hemorrhage, etc., indicate a profound blood-change, and now our main reliance must be on stimulants and tonics, with the most nutritious diet.

Laryngitis may occur in the course of diphtheria without any marked symptoms referable to the larynx, provided that the inflammation remain catarrhal, as in the case related above. But if fibrinous exudation occurs in the larynx, symptoms of obstructed respiration are developed, and the condition is then one of imminent peril. Prompt measures are required to relieve the patient, but the result will probably be unfavorable, as we have already stated. It will be necessary sometimes to prescribe one of those emetics which are but slightly depressing, as sulphate of copper, but even this should be administered with an alcoholic stimulant. Depressing emetics, as ipecacuanha and hive syrup, should be avoided. I have known sudden fatal prostration to occur after the use of the latter under such circumstances, in a strong child of eight or nine years. Quinine, steam as recommended in the treatment of croup, chlorate of potash, and muriate of ammonia, with alcoholic stimulants, are the remedies for diphtheritic laryngitis which will be found most useful.

Diphtheritic paralysis requires the use of strychnine with tonics. I ordinarily employ the elix. phosphat. ferri, quiniæ, et strychniæ of the shops. Each drachm of this contains gr.  $\frac{1}{60}$  of strychnia, and by dilution with water the proper dose can be administered to a child of any age. Thus, recently, a child aged six years, having paralysis of the muscles of the pharynx, recovered in about one week, by the use of one drachm of this medicine daily, given in four or five doses. I have not found it necessary in any case which I have observed, to employ electricity, but it is no doubt useful in expediting recovery, especially if the paralysis is in the limbs. The anæmic state which succeeds diphtheria requires the use of iron for several weeks.

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PUERPERAL FEVER.

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BY LEOPOLD PUTZEL, M.D., New York.

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" Bryce Prize " Thesis, Bellevue Hospital Medical College, 1875.

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IN treating of puerperal fever, want of space will not permit me to discuss the subject in all its bearings. I shall, therefore, restrict myself to a consideration of its aetiology, pathological anatomy, clinical history, and essential nature.

## ÆTIOLOGY.

The causes of the disease may be divided into two classes, viz. : individual or predisposing, and general or exciting. The first category includes physical want, mental depression, primiparity, and other conditions connected with labor.

I. *Physical Want.*—Many English and French observers, especially Ferguson, Gordon, Hervieux, and Voillemier, emphasize the fact that a low state of the system, occasioned by insufficient food, exposure, and other depressing influences, is a potent predisposing cause of this malady. But as these authors derived their views chiefly from an analysis of hospital cases, too much importance must not be attached to this agent, since the patients were also subject to the effects of hospitalism. In this connection, a striking fact, developed by Professor Barker, is worthy of note, viz., during the early part of last year the disease was proportionately more prevalent in this city among the well-to-do than among the poorer classes. Thus, among the former the deaths from metria were 1 in 36.8 births, while among the latter the mortality was 1 in 84. In significant contrast to this is the circumstance, that in the better circles the deaths from obstetrical complications were 1 in 147.3, while among the illy-provided poor they were 1 in 79 births. Ulysse Trélat, in the *Annales d'Hygiène* for April, 1867, makes a similar statement regarding certain epidemics which he has observed.

II. *Mental Depression.*—As early as 1603, Roderic à Castro observed that depressing affections of the mind frequently cause disease in the puerperal state. Since that time nearly all writers have recognized that this cause is potent for evil. The statistics of Bellevue Hospital show that a much greater proportion of unmarried than of married patients are attacked, and furthermore, the mortality is uniformly greater among the former. It has also been frequently observed, that patients illegitimately pregnant, and who had previously been doing well, after being reproached by relatives or visited by their seducer, have at once been attacked by the disease and rapidly succumbed to its fatal influence.

III. *Primiparity.*—The majority of authors are, we think, agreed upon the importance of this element. Ferguson found, in an analysis of 204 cases, that 80 were primiparæ, and that these furnished half the deaths. Hervieux found 119 primiparæ in 190 cases. Laserre's statistics showed that among 1,025 primiparæ, 89 became affected with the disease and 66 died; among 1,314 multiparæ, only 43 fell sick and but 21 died. The analyses furnished by German writers (Veit, Braun, and Quadrat) merely serve to substantiate this view. The statistics of the great lying-in hospitals of Europe also demonstrate that subjection to obstetrical operations renders the patient more liable to be attacked. These causes probably owe their efficacy to the fact that, in such cases, labor is more prolonged and the depression of the system correspondingly increased. We must, however, be cautious not to lay too much stress on the importance of these predisposing causes. This caution is fully justified by the above-mentioned statistics of the late epidemic in this city, taken from Barker on *Puerperal Diseases*. In this instance the predisposing causes were tenfold more active in the lower walks of life; nevertheless the epidemic raged with much greater severity among the better classes.

The general or exciting causes embrace meteorological conditions, vitiation of air, contagion and infection.

I. *Meteorological Conditions.*—The opinion has always been wide-spread in the profession, that this affection is more prevalent in cold and damp seasons. Hervieux, by a vast array of statistics collected by Laserre and himself from the Maternité

of Paris, and by Spaeth from the Vienna Clinic, corroborates the view which generally obtains. Trélat, also, has shown by copious records, that from 1848-51, inclusive, the mortality in European hospitals was a comparatively small one, and these years were marked by dryness and absence of changeableness in the weather.

II. *Vitiation of Air*.—That the vitiation of air produced in a hospital (the nosocomial malaria of Giralde's), especially in lying-in wards, and which is due to the accumulation of organic products of excretion from the lungs, the skin and the lochial discharges, as well as from passages from the bowels, is a fertile source of the disease, is admitted on nearly all sides. Considering the vast amount of testimony that may be adduced to support this view, we deem it unnecessary to enter into any discussion on the subject.

III. *Infection and Contagion*.—The question of the infectious or non-infectious character of the disease is of vital moment with regard to the consideration of its essential nature. The majority of medical men view the subject through the medium of their pre-established notions. The only proper method of solving the problem is to ascertain whether there are any examples of so-called infection which cannot be explained on any other principle. Negative cases are useless in this connection; one indisputable positive example is conclusive. Such a one, we think, is the following case cited by M. Dubois, in the discussion held in 1858 before the "Académie Imperiale" of Paris: "In a memoir read before the Medico-Chirurgical Society of Edinburgh, Dr. Moir related the following case: Dr. Reid, while in Ireland, assisted some poor women in their confinement. One of them succumbed to puerperal fever, and the same fate befell those who afterwards came under his care. Soon after this he quitted Ireland, and in the following autumn, during a short sojourn in London, again acted as accoucheur to a patient. The doctor happened to wear the same clothes he had used in Ireland; his patient died of puerperal fever with two other women whom he attended at the same time. Some months afterward, while on board a vessel at sea, he attended one of the passengers in her confinement. As she was very destitute, he gave her some clothes which he had used in London; this woman also died of puer-

peral fever, and four other women, whom he delivered during the voyage, perished from the same disease."

This case needs no further comment. It appears to us to be an incontrovertible proof of the infectious character of puerperal fever, and must, in our opinion, appear so to all who view the matter with an unbiassed judgment. Of the contagious character of the disease few medical men of the present day have, if we may be allowed the expression, any practical doubts, that is, all act in accordance with the belief that they may be carriers of the puerperal poison. Schroeder, whom we may take as the representative of the German school, holds that in the strictest sense of the term it is not contagious, but merely, to use his own expression, "*manuell übertragbar*." As Schroeder himself confesses, however, this view starts from the theory that the introduction of septic material into the system causes the disease, and as a matter of course, the malady not being a specific one, it is not strictly contagious.

#### PATHOLOGICAL ANATOMY.

In this subdivision of the subject we shall combine with the results proven by various writers an analysis of thirty-one (31) post-mortems which we have carefully collated from various sources. Twenty-two (22) of these we had the privilege of witnessing at the Bellevue Hospital Dead-house, where they were carefully made by the curators of the institution. Several of these cases we had the opportunity of observing clinically, from day to day, in the wards of Dr. Janeway, during the epidemic of last spring. In describing the anatomical lesions found, we will begin with those observed in the external genitals, thence proceeding with the organs in their continuity, not because it is a philosophical arrangement, but merely for the sake of convenience.

Many German writers describe a "puerperal ulcer," the seat of which is generally in the vulva; this ulcer has raised firm edges, and is covered with a yellowish, foul-looking exudation; it has a tendency to spread, and is very slow in healing. In no authors, except those pledged to the septicæmic theory, could we find a delineation of this lesion, and, in the analysis which we have made, not a single one answering to this description



could be discovered. Non-specific ulcerations, however, are not of very infrequent occurrence.

According to Virchow, inflammation frequently spreads from the ulcers to surrounding tissues, resulting often in effusion into the submucous cellular tissue, constituting œdema labiorum. This condition Virchow considers identical with ordinary erysipelas of the cutaneous surface, and therefore terms it "erysipelas malignum puerperale internum." Although a few of the cases which I have examined presented slight effusion into the cellular tissue, with congestion of the mucous membrane of the labia, they gave rise to no special symptoms during life, and showed no tendency to spread into the cellular tissue surrounding the vagina; in not a single case in which there was diffuse pelvic cellulitis could the origin of the inflammation be traced to erysipelas of the labia. The vagina is not frequently the seat of disease; of thirty-one cases, in two there was ordinary inflammation, as shown by the slate color of the mucous membrane and its swollen and softened condition; in two there were diphtheritic patches in the membrane, in one case extending into the cervix, and in the other involving some rents in the vagina; in a fifth case, sloughing ulcers were discovered in the posterior cul-de-sac.

*Endometritis.*—This is a quite frequent lesion. Hugenberger found 385 examples of it in an analysis of 1,614 post-mortems. In thirty-one (31) cases we observed fifteen (15) in which the lesion was marked, and in another subject the whole endometrium was intensely injected, but no other sign of inflammation existed; in two others there was a slight exudation at the junction of the cervix with the body, but the remainder of the membrane was normal in appearance. In endometritis, as usually observed in puerperal fever, the membrane presents the ordinary characters of catarrhal inflammation, with the exception that the muco-purulent discharge is generally of a dirty greenish-yellow color, and swarms with bacteria and vibrios. The placental insertion may take part in these changes, but only occasionally to so great an extent as the remaining portions; the placental insertion rarely sloughs away entirely, laying bare the muscular coat of the uterus.

In one case there was a superficial ulceration of the lateral part of the cervix uteri; posteriorly was a patch one and a half

(1½) inches long, with sloughing borders and a suppurating base; the margins of the laceration were diphtheritic, and running up from the posterior part of the cervix was a track of suppuration extending into the uterine subperitoneal tissue and into the broad ligaments; in three other cases I have seen circumscribed diphtheritic patches on the cervix, and in one specimen the whole endometrium was involved in the same process.

*Metritis.*—In 267 necropsies observed by Tonnellé and Robert Lee, there were 89 in which metritis existed; the diseased uterus presents two distinct sets of appearances. In the first variety it is soft and large from infiltration with inflammatory œdema, and also from cloudy swelling of the muscular fibres; in the second class, circumscribed and poorly-defined abscesses are scattered throughout the muscular substance. In nine (9) cases of metritis I have seen the latter variety three times, and in each instance it was attended with uterine phlebitis.

It is difficult to determine whether any causal connection exists between metritis and endometritis, although many observers claim that the former arises from the latter by extension of inflammation in continuity.

But the fact that, in the analysis which we have made, seventeen (17) examples of endometritis were associated with metritis only eight (8) times, and that in one (1) case of the latter disease the endometrium was perfectly normal, proves that, at least in some instances, one underlying cause produces both lesions.

*Parametritis.*—In no work which we could consult, was a statement made with regard to the relative frequency of parametritis compared to other lesions. We have observed its existence, however, in nearly twenty per cent. of all the cases analyzed. Generally the process is diffused over the whole superficies of the organ, but, in some exceptional cases, it is circumscribed. Parametritis is an acute inflammation of the uterine subperitoneal cellular tissue, and is characterized by effusion of serum into the meshes of the connective tissue and a swelling of the fibres due to imbibition of the exudation; in addition, it is attended by a round-celled infiltration due to emigration of leucocytes from the blood-vessels, and probably

also to proliferation of the connective-tissue corpuscles themselves. The cellular infiltration may become so disproportionately large as to merit the title of purulent infiltration. The affection may terminate in complete recovery by resorption of the effused material, the cells undergoing fatty degeneration ; or, on the other hand, pus cells may have been formed in such numbers that abscess formation is inevitable.

In rare instances, the inflammation is so intense that it terminates in necrosis, the cellular tissue becoming of a dark-brown or even chocolate color from imbibition of hæmatin and the presence of gangrenous tissue. These latter cases must, as a matter of course, terminate fatally.

Pathologists are not in unison concerning the ætiology of this affection when it occurs as a complication of puerperal fever.

According to Klebs and his followers, it arises sometimes from metritis, but generally from an extension of inflammation from the connective tissue surrounding the vagina, whence it may spread into the broad ligaments and iliac fossæ ; in rare instances it has been known to pass up behind the peritoneum as high as the diaphragm. In six examples of this lesion, four were unaccompanied by either of the processes above-mentioned, while in two instances it was propagated from the inflamed cervix. These facts prove that extension of inflammation does not play the all-important part which advocates of the septicæmic theory of puerperal fever would have us believe.

*Lymphangitis.*—In an aggregate of 412 post-mortems made by Tonnellé, Robert Lee, and M. Behiér, they observed 79 examples of lymphangitis, and nine (9) of these were accompanied with suppuration in the lumbar or inguinal glands. We have observed it in five (5) cases, or nearly sixteen per cent. of the whole number analyzed, and, in two subjects, the lumbar glands were also involved.

In addition to these cases, we had the opportunity of observing three post-mortems on puerperal-fever patients on three successive days, and in each of them lymphangitis existed. Unfortunately, no records were kept of these cases.

In this lesion the lymphatics are enlarged in calibre and are filled with a whitish or grayish thrombus. The thrombi generally start either from the uterus, especially the posterior wall,

or from its immediate neighborhood, and extend a variable distance along the course of the vessel. In rare instances, they have been observed to extend into the receptaculum chyli, and even to involve the thoracic duct.

Generally, the thrombosis is due to extension of inflammation from the surrounding cellular tissue to the endothelium lining the lymphatic vessel; it must be remembered that the internal coat of the lymphatic system is histologically identical with the endothelium of the vascular system. In consequence of the inflammation, the internal wall becomes roughened, causing the fibrin of the lymph to be deposited, with a greater or less number of lymph corpuscles which are entangled in its meshes. The thrombus may undergo suppuration and softening, giving rise to emboli in the same manner that they occur in the blood-vessels. The emboli are, however, always arrested in the nearest glands, and thus invariably proven innocuous. We shall consider hereafter the importance attached to this lesion by some pathologists.

*Thrombosis of Veins.*—This is much more frequently seen than the corresponding affection of the lymphatic vessels: thus in 412 post-mortems witnessed by Lee, Behiér, and others, in nearly sixty-two (62) per cent. was the affection present; we have observed it in forty-one (41) per cent. of all the cases which were analyzed. The significance of venous thrombosis has been variously estimated. M. Behiér and Robert Lee see in it the essence of the disease, while the majority of pathologists, on the other hand, consider it as merely secondary. Behiér bases his theory on the study of several epidemics which occurred in the hospitals of Paris and on the examination of 145 patients post-mortem, in 141 of which he discovered uterine phlebitis. Very frequently the thrombosis originates at the seat of the placenta; the thrombi generally extend only a short distance into the muscular tissue and are often broken down and softened; another favorite seat is at the junction of the broad ligaments with the cornua of the uterus. Dr. Janeway has called my attention to the frequency of thrombosis of the ovarian veins; in thirteen (13) cases of phlebitis we have seen this variety four (4) times, and in two (2) of them the thrombi extended into the vena cava. The importance of the latter form of the affection is shown by the fact that each case was

complicated with lobular pneumonia. The dependence of the latter disease upon the former is proved by the striking circumstance that in no other case did lobular pneumonia exist.

Generally the thrombi are soft, but they may also be hard and cord-like. When examined microscopically, the thrombi are found to contain fibrin, leucocytes, a few red blood-globules and endothelial cells, all undergoing granular and fatty degeneration; at times, also, vibriones are found in abundance.

*Peritonitis.*—Peritonitis is so frequently a concomitant of puerperal fever, that at one period the theory was entertained by many eminent obstetricians that the two affections were identical.

In 2,367 cases collected by Hugenberg, Tonnellé, Dugés, and others, it was present 1,229 times. In our analysis, in seventeen (17) general peritonitis existed, and in four (4) pelvic peritonitis alone was present. The appearances presented in this complication are not entirely uniform; they frequently vary considerably from those seen in idiopathic peritonitis. According to McClintock, of Dublin, the differences between the two varieties are very marked. In non-*puerperal* peritonitis, he states, the arteries are enlarged and engorged, giving to the peritoneum an arborescent appearance, and the fibrin effused is very sticky, causing the small intestines to adhere closely together: the quantity of exuded fluid varies considerably, generally being present in large quantity. In the peritonitis of *puerperal* fever, however, venous congestion is almost exclusively present, as shown by the bluish color of the peritoneum; the coagulable lymph, also, is not so sticky as in the non-*puerperal* affection, and is sometimes present in very small quantity. Although these statements are correct for a certain proportion of cases, they are not, by any means, true of all. We have frequently seen cases of *puerperal* peritonitis in which, coexisting with intense arterial congestion, the small intestines, and in fact all the abdominal viscera, were closely matted together by inflammatory products. The fluid effused, also, may vary from a barely appreciable quantity to several pints. In two cases, we have seen the fluid in the peritoneum of a brownish-yellow color and having a disagreeable, fetid odor. According to Foerster and Martin, *puerperal* peritonitis is usually due to spreading of inflammation from the



endometrium along the Fallopian tubes. The post-mortems which we have seen do not corroborate this view; thus in only four cases were there changes in the mucous membrane of the tubes. In one specimen the only alteration was intense arterial congestion; in another, pus was present in the outer two-thirds of the tube, but the inner portion was entirely normal; in the remaining two cases, pus was detected along the whole extent of the tubes.

That the inflammation does not arise from metritis, is shown by the fact that in three instances of extensive peritonitis, the uterus and uterine subperitoneal areolar tissue were perfectly normal; in another specimen, the liver was covered with a thick fibrinous exudation, while the remainder of the peritoneum was free.

According to Klebs and many other pathologists, the cause of the inflammation is always to be looked for in bacteria and micrococci, which, wandering through the lymphatics or blood-vessels of the uterus, first gain access to the peritoneum. From this serous membrane they may infect the whole organism by travelling along the lymph-spaces, or by penetrating the blood-vessels, in which they produce thrombosis and frequently secondary embolic processes. Afterwards, the bacteria will affect that part of the economy in which the emboli containing them happen to become lodged. Of course it will be impossible, in a paper like the present one, to discuss the parasitic theory of disease. We shall, therefore, content ourselves by stating that the researches of Billroth, Robin, Samuels, and Cohnheim, the results of whose investigations have only been made known within the past few months, prove conclusively that bacterial organisms are not the cause of septicæmia, and that the only part which they possibly can play, is to increase the activity of the process after it has once been started. Many other lesions, in addition to those we have described, have been observed, and there is perhaps no organ of the body which may not be the seat of disease. Thus ovaritis, nephritis, various affections of the intestinal tract, pericarditis, pleurisy, pneumonia, meningitis, suppurative synovitis, abscesses in the muscles, etc., are not infrequently presented on the post-mortem table, but want of time and space will not permit a discussion of these manifold disturbances.

## CLINICAL HISTORY.

The following typical case is taken from Tonnellé, not only because the clinical events are fully and tersely stated, but also because it is accompanied by an account of the post-mortem examination :

“*Mrs. A.*—The labor was normal and of moderate duration. On the second day after delivery, the patient experienced prolonged rigors, followed by abundant perspiration and abdominal pains; in the evening there was considerable mental agitation and, at times, violent delirium. This condition lasted through the night. On the morning of the third day she was weak, but perfectly calm and collected; she was, however, unconscious of what had transpired during the previous night. The abdomen was still tender, but not to so great an extent as when the attack began; the lochial discharge was natural in appearance; a slight diarrhœa began in the course of the morning. On the fourth day, the patient had an attack of syncope, from which she soon recovered, and which was followed by profuse bilious vomiting; there was considerable tympanites, but no pain. During the evening, she had a paroxysm of dyspnœa and extreme prostration. On the fifth day, the abdominal pain and tenderness returned, together with some psychical agitation and delirium. The diarrhœa, which had continued up to the present time, now entirely disappeared. On the sixth day the patient had cold sweats, irregular pulse, rapid breathing, involuntary evacuations, and vomiting. She continued in a state of collapse until the next morning, when she died.

*Post-mortem.*—A small amount of serous fluid was found in the cavity of the peritoneum; there was slight vascular injection of the pelvic peritoneum. A little pus was found in a few uterine veins, but otherwise the uterus appeared perfectly healthy. It is true, a slight amount of phlebitis existed in this instance, but the lesions visible after death were too limited in extent to have materially interfered with the course of the disease. In the case quoted, the onset was marked by a chill, but in a certain number there are undefined symptoms of malaise prior to the real onset of the disease. In some epidemics the prodromata are entirely absent, while in others premoni-

tions are usually present. A chill, the intensity and duration of which vary exceedingly, generally ushers in the disease. In the majority of cases, the chill is moderate in intensity and lasts but a few minutes; sometimes it is almost inappreciable, and, still more rarely, its intensity is such that it may render the physician suspicious of commencing pyæmia. In very exceptional instances, the chill is replaced by a fit of furious delirium. Frequently the chill is followed by abundant perspiration, but, most generally, diaphoresis is slight or altogether wanting. These symptoms are speedily followed by acute abdominal pain, accompanied with great tenderness on pressure. The pain may be limited to the hypogastrium, but, even if diffused, it is more intense in this region. The disease is not usually attended in the beginning with tympanites, but this symptom becomes developed at a later stage, even when peritonitis is slight, or altogether wanting. Thus, some epidemics are characterized by pleurisy or pericarditis, the peritoneum remaining unaffected, yet abdominal tenderness is almost invariably complained of.

On the other hand, I observed two patients, during the late epidemic at Bellevue Hospital, who uttered no complaint, notwithstanding the fact that the post-mortem revealed the existence of intense peritonitis.

It is quite diagnostic of this disease to have considerable mental agitation and even violent delirium at night, while, towards morning, the patient becomes calmer, and is perfectly conscious and subdued during the day.

Dr. Janeway has informed me that a patient recently came under his observation, in whom the mental disturbance was so marked as to mislead an eminent physician into the error of pronouncing the case to be one of puerperal insanity.

It is highly probable that the cerebral symptoms are due not only to the direct action of the poison of puerperal fever upon the brain, but also to the irritation caused by the circulation of excrementitious matters in the blood, the increase of these products being occasioned by the high temperature of the body.

The symptoms on the part of the digestive system are vomiting and diarrhœa; the former is usually bilious in character, and may be followed by a delusive feeling of relief. Diarrhœa is also a common symptom, and, in some epidemics, is very

marked. This is in striking contrast to peritonitis, in which diarrhœa is the exception and constipation the rule. When moderate in amount, diarrhœa is not by any means a sign of evil portent, but rather the reverse. The pulse is rapid from the beginning, ranging from 120 to 160 beats per minute; it is small and compressible, showing diminished force of the heart's action; towards the fatal termination, the pulse becomes thready and irregular.

The temperature varies from 102 to 106 degrees, and is not characterized by very marked oscillations; the evening rise in the thermometer is usually from  $1\frac{1}{2}$  to  $2\frac{1}{2}$  degrees.

Generally the temperature is raised two or three degrees when an inflammatory complication is developed, but the rise is not permanent. The respirations are usually hurried and shallow, even when no pulmonary complication exists. Some epidemics are characterized by frequent and sudden attacks of dyspnœa, the ætiology of which is by no means clearly ascertained.

The lochia and lactation present no constant phenomena; sometimes both discharges are established at the usual periods, and do not appear to be in the least affected by the disease. Generally, however, the secretion of milk is very scanty, and soon ceases altogether.

The lochia, also, are frequently either suppressed upon the invasion of the disease, or they may be offensive in character.

An unfortunate termination is usually betokened by increased tympanites with constant and marked dyspnœa, accompanied by a rise in the temperature and extreme prostration.

The duration of fatal cases is from three to ten days, but, in those which end in recovery, convalescence is commonly very prolonged, especially if the fever has been accompanied by severe complications.

The preceding symptoms, as we have already stated, are those of typical, uncomplicated cases; but such examples are not very frequent, as the perusal of the section on pathological anatomy will clearly demonstrate. Want of time will not permit us to describe all the variations of phenomena produced by co-existing affections: it will only be necessary to remark that in no complications do the symptoms equal in severity those peculiar to the disease when idiopathic; and, furthermore,

that the diagnosis of the complication is rendered thereby extremely difficult.

#### NATURE OF PUERPERAL FEVER.

This article would swell to unseemly proportions were a discussion entered into of all, or even the greater number, of the views which have been advanced to clear up this perplexing topic. Attention will, therefore, be directed only to the two theories which chiefly divide the attention of the medical world, viz. : to the theory of its septic origin, and to that of its zymotic nature. Our principal reasons for adhering to the latter view are as follows :

I. Puerperal fever is infectious and contagious ; septicæmia is not. The example quoted in the subdivision on ætiology cannot be interpreted to mean anything else but infection, and numerous similar examples recorded by various obstetricians tend inevitably to the same conclusion.

Of the contagious properties of puerperal fever, most medical men have now little or no doubt.

Septicæmia has never been known to track the practice of a physician in a country district. The number of authentic examples on record in which an obstetrician has conveyed the poison of puerperal fever from one patient to others, is almost innumerable.

II. It is a curious and instructive fact, that while one epidemic may be characterized by a certain lesion, such as uterine phlebitis or metro-peritonitis, another may be especially marked by the existence of pulmonary affections, or, perhaps, lesions of the brain. Again, in the beginning of an epidemic a certain complication may be prevalent, but, towards the close of the same epidemic, a different co-existing affection may predominate. Such facts are entirely inconsistent with the septicæmic theory, but they do not conflict with the doctrine of its zymotic character. Typhus fever is also distinguished by no pathognomonic post-mortem appearance, yet certain lesions are found in some epidemics which are totally absent in others.

III. The evidence afforded by a careful and impartial analysis of a considerable number of post-mortems, is in itself quite conclusive. It has been shown in the section on patho-



logical anatomy that peritonitis is sometimes produced when the uterus, uterine sinuses and lymphatics and pelvic fascia are normal. Pleurisy may be caused not alone by extension of inflammation from the peritoneum through the diaphragm, but also independently of any other lesion. A similar statement will hold true concerning other concomitant affections. These facts disprove the belief that metritis, parametritis, peritonitis, etc., are caused by the action of septic material absorbed somewhere in the genital tract. They rather tend to establish the existence of a specific poison in the blood, contaminating the whole system and rendering each organ liable to serious derangement of its nutritive processes. Exceptionally, patients die who have presented a well-marked history of this disease, although the necropsy does not reveal the slightest abnormal appearance. An example of this happened last spring in the hospital service of Prof. Lusk. The objection may be made that such cases are instances of acute septicæmia, but the stubborn fact remains that the clinical events are entirely different from those of the latter disease.

IV. A patient may be attacked with puerperal fever before labor has begun, even though the fœtus was healthy and no avenue existed for the introduction of septic material. Such cases are rare, but their occurrence is undoubted. Septicæmia cannot possibly occur under similar circumstances.

V. A very forcible argument, but one upon which sufficient stress is not usually laid, has been referred to by Dr. Barker. This writer states that during an epidemic of puerperal fever, gynecological operations are very likely to be attended by subsequent inflammation; and adds, in confirmation, that Drs. Sims and Emmet can predicate the existence of the fever at Bellevue by the great tendency at the Woman's Hospital to pelvic cellulitis and peritonitis after surgical operations. This condition of affairs is analogous to that existing in this city at the present time with regard to diphtheria. The latter malady is now wide-spread, and whole families are sometimes attacked by an epidemic sore-throat, dependent, no doubt, on a slight degree of infection with the diphtheritic poison. The analogy, in this particular, between puerperal fever and diphtheria, which is an undoubted zymotic disease, is too evident to call for further comment.

VI. The clinical history is different from that of septicæmia, as a review of the subject will show. In the latter, there are no prodromic symptoms; in puerperal fever, prodromata are not infrequent. The latter is usually ushered in by a chill; the occurrence of an initial chill in the former is very rare. In septicæmia, the nervous symptoms are especially marked, the patient being generally apathetic and sleepy, or even comatose. The temperature, also, rises very high at the onset of the attack, but later the thermometer indicates a fall to the normal or even below it. In puerperal fever the temperature varies from two to six degrees above the normal throughout the whole course of the affection, and usually rises towards the fatal termination. The frequently recurring chills of pyæmia, with the subsequent rise of temperature and exhausting sweats, the jaundice and the pulmonary complications so usually found in this disease, are sufficient to distinguish it from puerperal fever.

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COMMUNICATIONS have been received from Prof. WM. T. Lusk, New York, on "An Epidemic of Puerperal Fever in Bellevue Hospital;" Drs. ALEXANDER MURRAY, New York, on "Electricity in Labor, Post-partum Hemorrhage, and the Asphyxia of New-born Infants;" BEVERLY ROBINSON, New York, "On the Sequelæ and Prophylaxis of Simple Tonsillar Hypertrophy in Children;" A. SERDUKOFF, Riazan, Russia, "Clinical Observations on the Mild Forms of Inflammation of the Tissue Surrounding the Uterus during the Puerperal State."

*Julius Pauly*, Zduny, Prussia, on "Dermoid Cysts of the Ovary."

## CLINICAL CASES.

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### POLYPOID ELEPHANTIASIS OF THE RIGHT LABIUM.

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By JAMES R. CHADWICK, M.D., Boston.

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MRS. J. A.—, 54 years of age, came to my dispensary to seek relief from a tumor depending from the genitals. Eighteen years before, she had had an abscess in the right labium, but noticed no traces of this until three years ago, when a small wart began to protrude from the spot where the abscess was lanced, and has grown steadily to the present dimensions. It has been quite free from pain, except when it has become chafed by her legs in walking; in this way it has been often excoriated, and has discharged blood and pus. No history of syphilis or ill-health of any kind.

Upon examination a tumor somewhat larger than a hen's egg was discovered cradled in a suspensory bandage, and connected with the right labium by a pedicle two inches long. The pedicle, which at its narrowest point was no larger than my little finger, took its origin at about the level of the clitoris, and seemed from the feel to consist exclusively of skin. The tumor was soft, covered with integument devoid of hair, of uniform density, except about the irregular cicatrix of an old ulcer.

The diagnosis was comparatively simple, in spite of the unusual pediculated form of the hypertrophy. Fibroid, sarcomatous, and other heterologous growths, besides being extremely rare in this locality, were excluded by the characteristics of the tumor. The possibility of there having been a hernia of the ovary, omentum, etc., with a subsequent closure of the canal, was entertained, but speedily abandoned. A cyst was, on the

whole, not unlikely, and could not be definitely excluded, owing to the difficulty of recognizing fluid in so small a cyst. The tumor was removed by simply cutting through the pedicle where it was smallest. After three days the stump had retracted to a level with the surrounding integument.



On section, the whole tumor was found to consist of a uniform white, glistening, fibrous tissue, full of serum. There was no capsule or line of demarcation between the centre of the growth and the integument, but the very same structure extended even to the layer of epidermal cells. The cut surface of the pedicle presented an identical appearance. Under the microscope nothing was found but fibrous tissue intermingled with elastic fibres. The lumina of many vessels were recognized, and near the surface the papillæ of the skin were seen

to be much enlarged. No hair-follicles were found, and only one sweat-gland. Small groups of fat-cells were met with here and there.

Exceptions might be taken by some to my naming this growth an elephantiasis, owing to the restriction of the morbid process to so limited a portion of the labium, instead of involving the whole part, as is common. But as the histological elements of the growth show it to be a simple hypertrophy of the connective tissue of the skin without the development of any adventitious tissues, it comes properly under this class of disease, and cannot be excluded from this category because of its accidentally assuming an unwonted shape. The absence of attacks of lymphangitis is of no weight, as they have not been so universally observed in this locality as in elephantiasis of the leg, etc.

This affection of the labium is rare in this country except among the negroes.

#### A CASE OF GALACTOCELE.

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BY JAMES R. CHADWICK, M.D., Boston.

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As this form of tumor in the breast is very rare, I venture to call attention to it by giving a few brief notes of a case that has recently been under my care.

A girl, twenty-two years of age, who, owing to a whim, had nursed her child for five months from the left breast only, began to notice an unusual tumefaction of that organ, without having received any blow or other injury. The swelling ached at times, but was not generally painful; on several occasions it subsided almost completely. There was never any hardness, throbbing, or darting pain, sense of weight, tenderness, or any of the constitutional symptoms of a mammary abscess. She continued to nurse the child from that breast as before without suffering

Six weeks after its first appearance the girl came to my dispensary. The left breast was uniformly distended, the integu-



ment of natural color, except a spot an inch to the right of the nipple. This was of a light reddish hue, and had a white dot in the centre; it was neither surrounded by indurated tissue, nor had it the dusky look of an abscess that is pointing. It was irregular in shape, and resembled an ecchymosis. As, however, distinct fluctuation could be felt over the sternal half of the breast, and there was an evident thinning of the integument at the red spot, a small incision was made, from which gushed fully two ounces of a thick, creamy, inodorous fluid. The color and consistency of this, taken in connection with the whole train of symptoms, caused me to submit the fluid to Dr. Fitz, by whom it was pronounced to be milk. A slight probing of the cavity showed that it overlay the mammary gland and extended two inches in the direction of the sternum. No sinuses could be detected. It is now ten days since the incision was made; the discharge has ceased, and the cavity is fast filling up. With orders from me the child has been weaned, which fact probably accounts for the rapidity of the cure.

These collections of milk take place in two ways: by the dilatation of a lacteal duct owing to obstruction, or by the rupture of one or more milk veins, and the escape of their contents into the cellular tissue. From the absence of anything like a cyst-wall in the above case I take it to belong to the second variety. The discoloration and thinning of the skin seems to have been an exceptional feature.

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MEDICAL SOCIETY OF THE COUNTY OF ALBANY, N. Y.

SEMI-MONTHLY MEETING, MARCH 17, 1875. THE PRESIDENT, DR. JAMES S. BAILEY, IN THE CHAIR.

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A CASE OF OBSTRUCTED LABOR CAUSED BY A RAPIDLY FORMING TUMOR.

DR. WM. H. BAILEY said, in presenting this case, that it would perhaps prove more interesting and instructive to refer to the previous health of the patient, to note the diseases she had been subject to in order to understand the relation they might bear to the malady he was about to describe.

In early life Mrs. T. had lived in the country, and enjoyed all the freedom and outdoor exercise necessary to secure good health and a vigorous constitution. Her health was uniformly good until an exposure to the fresh paint upon the house in which she lived, which produced a severe attack of colic. She recovered fully from this, but was ever after liable to renewed attacks upon any unusual exposure or by any imprudence in diet. It was in one of these paroxysms that he first saw her. She had married and moved to the city; she had suffered less and at longer intervals than when in the country; she believed the change of air and the better protection afforded her upon sidewalks rendered her less liable to these painful attacks.

In September, 1869, Dr. Bailey attended her in her first accouchment; it was a tedious and very severe labor. The head was large, and the pains, although severe, were not sufficient to expel the child. After she had been in labor about fifteen hours it was thought best to use the forceps; even with them the delivery of the child was accomplished with difficulty. The mother was greatly exhausted, but nothing occurred during the first few days to cause any particular anxiety. At about the end of the second week her symptoms seemed more grave. Her pulse was feeble, her weakness marked, and whenever she was raised from the pillow she grew very faint. These symptoms, it was feared, indicated the formation of a heart clot. The heart's sounds were normal, but very indistinct. There was no localized swelling or pain, no distress in breathing so long as she maintained the horizontal position; her pulse was rapid, averaging about 100 per minute, not differing materially from what it had been since her confinement. Her prostration was very decided, and fearing what appeared to be the threatening of a serious complication, entire rest and the removal of all excitement was strongly enjoined. The lochial discharge had continued without interruption. The genital organs had been much swollen, but were now decidedly improved. Vaginal injections of tepid water with a weak solution of carbolic acid had been freely used. Hot fomentations had been liberally applied over the region of the womb. With this treatment the graver symptoms gradually yielded, and the friends of the patient were greatly gratified by the steady but constant progress to-

wards convalescence. Her health was fully restored, and in September, 1871, she was delivered of a second child. This was an easy labor and rapid. There was no complication, and she was soon able to resume her ordinary duties.

In 1873 she had a miscarriage at about the third month. Nothing extraordinary occurred at this time. She was advised to remain in bed certainly two weeks, simply as a measure of precaution.

Some time in the spring following a feeling of discomfort in the region of the womb attracted his attention. There was no pain, no leucorrhœa, no tenderness, but after being upon her feet for some time there was a sense of fulness and a downward pressure similar to prolapsus uteri.

Upon examination she discovered a round smooth surface presenting, and as she believed crowding back the womb. When erect she was conscious of its presence, but when in a horizontal position it entirely disappeared. When long upon her feet the uneasiness amounted to pain, but knowing that she was pregnant she felt no solicitude about her condition, believing it to be a necessary accompaniment, and that a few months would bring relief.

About the middle of August she was greatly disturbed by what appeared to be a severe cold, but it proved to be whooping cough. This cough continued with varying severity during the balance of her pregnancy. It caused great tenderness all over the abdomen, with severe pain in her left side; but the difficulty that troubled her most was that peculiar sensation impressing her with the idea that the membranes were liable to be ruptured at any moment whenever she might happen to be coughing. She called Dr. Bailey's attention to this feeling and the apprehension it caused her, but she seemed cheerful and well, excepting her cough; therefore he did not pursue the inquiry sufficiently far to realize the extent of the difficulty or to understand the real nature of her trouble. In November, 1874, she was delivered of her third child at full term. At first the pains were regular, but not severe. Upon making a digital examination there was discovered a small tumefied space just within the vagina upon its posterior surface. He was unable to satisfy himself in regard to the nature of this unusual fulness, but believed it could not in any way complicate labor. The

labor was slow, and even after the rupture of the membranes the advancement was very tardy. The vaginal tumor seemed a little larger. In order to relieve the mother, and if possible to save the life of the child, it was thought best to deliver with forceps.

The attention of Dr. Quackenbush, who came to the assistance of Dr. Bailey, was called to this tumor, but the protuberance was so slight it was believed to be nothing serious, and therefore the instruments were applied. It was noticed that the downward pressure of the head increased the size of the tumor, so much so that it filled the space between the blades of the forceps and crowded upon either side of the instruments. The vagina being thus completely filled, the forceps were removed, and a more careful examination made; after a thorough exploration it was found to be a subperitoneal tumor of considerable size, materially obstructing the passage of the child; in fact, so completely filling the vaginal space, that it would have been impossible to have delivered the child without first evacuating its contents.

It not only filled the vagina, but it was forced out beyond the labia almost to the size of a medium-sized fist. It appeared like a distended bladder, and when the lance was introduced about three pints of serum escaped, after which the forceps were applied and the child delivered without any unusual difficulty. There was nothing remarkable in the remaining part of the labor. The patient was very much exhausted, but seemed quite comfortable. She did not rally rapidly, but seemed to be doing well till about the third day, when the tenderness over the abdomen increased; her pulse ran up to about 100, and her bowels were extremely tympanitic. We were much alarmed regarding her safety, but after a few days she improved, and to-day is in good health, although not so strong as formerly. The serum from this cyst continued to discharge for a number of days. Towards the last the odor was fetid, from which we concluded that the sack was destroyed, and if our conclusions are correct, there is no probability of the return of the tumor.

#### UTERINE POLYPUS.

DR. E. B. TEFFT reported the following case: Mrs. H., æt. 38,

married twelve years, and never pregnant, a large, plethoric woman, had from puberty suffered from leucorrhœa and from excessive and painful menstruation, which had lately increased at every recurring period. She was first seen in July, 1869. At this time hemorrhage was very great, and attended with considerable pain. The physician was not allowed to make an examination per vaginam, and could only guess at the cause of the trouble and content himself by checking the hemorrhage. At the two following menstrual epochs the same thing occurred, only with increased hemorrhage and pain. At the next period hemorrhage was so alarming that an examination was submitted to. The os uteri was sufficiently dilated to admit the finger, and the cavity of the uterus was found to be occupied by a tumor about the size of a hen's egg, around which he could pass his finger and feel that it was attached by a short pedicle to the anterior surface of the uterine cavity, a short distance above the os. The hemorrhage at this time was accompanied by severe pains resembling those of parturition. Ergot was given with a view of expelling the tumor into the vagina, which was not accomplished, and he was obliged to resort to the tampon to control the hemorrhage. From this till the next monthly period she was not entirely free from either pain or hemorrhage. He then again administered ergot, and the polypus passed through the os into the vagina. Its immediate removal was advised, but consent was refused. Dr. H. M. Edsall, at Dr. Tefft's request, saw the case with him, and concurred in his advice as to its removal. When the patient and her friends were informed that its removal was the only thing to be done, and that in its new location the tumor would be likely to increase rapidly in size and occasion still greater trouble than heretofore, we were dismissed, and Dr. Hunter, an older physician, of considerable local reputation, took charge of the case. His advice, as afterwards was learned, was to let it alone, which he did, merely directing his treatment towards checking the hemorrhage and relieving the pain. He attended the case about a year, when he died, and she again came under Dr. Tefft's control. She was then confined to her bed, was exsanguinated, and greatly reduced in flesh and strength. The hemorrhage was constant, and at the monthly period was very great. Examination per vaginam revealed a



large tumor completely filling the passage and distending it; its lower end slightly protruding from the orifice, and in a sloughing condition, a thick greenish discharge of horrible stench coming from the vagina. On account of the pressure of the tumor on the urethra and rectum, the patient was unable to evacuate the bladder except by catheter, or the bowels except by enema.

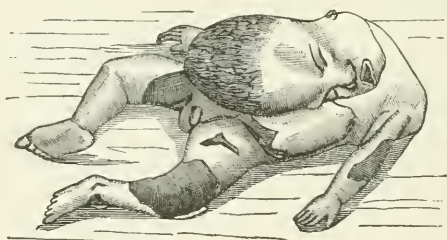
A proposition was made for its immediate removal, and permission again refused. The old doctor had said it would slough off.

After attending the case for three days in this condition, and fearing she would die at the next menstruation, her friends were informed of the facts. Dr. Tefft then refused to do anything more unless permitted to remove the cause of the trouble. Reluctant consent was given, and, with the assistance of Dr. Edsall, the tumor was removed. Chloroform was administered. We then endeavored to bring the mass outside of the vagina by means of tenacula, but failed. Obstetrical forceps were resorted to, and the tumor delivered. The pedicle, which was 2 inches in length and about  $1\frac{3}{4}$  inches thick, was divided near its middle. No hemorrhage of any amount followed the use of the knife. The parts were washed out with tepid water and the patient put to bed. Some tenderness in the hypogastric region and slight febrile excitement followed, which yielded to opium, and the patient made a good recovery, and in four months was able to go about her household duties. The tumor removed was nearly pear-shaped, fibrous in character, invested by mucous membrane, and measured seven inches in length by five and a half in its greatest diameter, and weighed a trifle less than four pounds. Dr. Tefft had seen the patient occasionally since the operation five years ago. She did not menstruate for two months after its removal, but since then has done so regularly and normally. She has never become pregnant.

CASE OF SPONTANEOUS EVOLUTION OF A HYDROCEPHALIC FŒTUS.

DR. WM. MORGAN presented the following case: Pauline K., a German woman, æt. 24, in her second labor under the care of a midwife, was taken in labor twelve or fifteen hours before he was called. He found her having strong expulsive pains, her general condition being good, the left hand of the fœtus

was protruding through the vagina and the os was fully dilated. The attempt was made for about ten minutes to push up the shoulders. It was then determined to turn and deliver by the feet, when a later examination showed that spontaneous version had taken place, the breech being already firmly engaged at the superior strait, and the child was entirely delivered by the next pain. The epidermis was macerated, and the foetus had evidently been dead several days. It was hydrocephalic, but the serum had mostly escaped. To this fact attention was particularly directed, there being no external point of escape for the fluid. The head consisted simply of a loose bag formed by the integuments, the cranial bones being all disarticulated, each being freely movable, so that it could be turned in any direction. There was a small amount of fluid material partly filling the cranial cavity. The whole constituted a condition and appearance that is very well illustrated in the accompanying illustration.



L. B.

This spontaneous evacuation of the serum from the hydrocephalic head is even a more interesting circumstance than the spontaneous version in the case during the second stage of labor.

The cranial contents were found to consist of a fluid of creamy consistence, of the color and appearance of red paint. It was about half a pint in amount, and could be nothing else than the disorganized brain tissue. No point of escape of the serum was discovered. The amount in which it had accumulated can be seen from the degree of distention of the cranium and its size relative to the body, in the wood-cut.

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## TRANSACTIONS OF THE NEW YORK OBSTETRICAL SOCIETY.

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STATED MEETING, OCTOBER 20, 1874. THE PRESIDENT, DR. PEASLEE, IN  
THE CHAIR.

DR. MONTROSE A. PALLEN, who was present as a guest, read a paper on

“VAGINAL CERVIPLASTY IN LIEU OF AMPUTATION OF THE CERVIX  
UTERI IN CERTAIN FORMS OF INTRAVAGINAL ELONGATION.”<sup>1</sup>

The principle on which Dr. Pallen had devised his operation was that in some cases of apparent hypertrophy of the cervix there was really no hypertrophy, but a too high insertion of the vagina, which was remedied by detaching the vaginal insertion and re-attaching it lower down, thereby shortening the vaginal portion of the cervix and removing the necessity of its amputation. He has operated successfully in three cases, the first four years ago. The operation was described in detail.

DR. EMMET said that the operation was certainly novel and striking, but was, he thought, founded on a wrong assumption as to the place of insertion of the vagina. He did not see how Dr. Pallen's plan would answer better than the usual amputation of the cervix and union of the mucous edges according to Sims, except by making a broader stump. He has frequently passed the sound up to the fundus, and drawn the uterus with the vulsella forceps over it to the length of six or seven inches; when the woman stood up the uterus would be 5" long, when on her hands and knees only 2½". Such cases of ductile putty-like cervix are liable to mislead the physician as to the actual length and position of the uterus. By putting the patient on her hands and knees the real length and position of the uterus will be ascertained, as well as the exact spot of insertion of the vagina. By always employing this precaution before performing amputation of the cervix an injury to the peritoneum can always be avoided.

DR. BYRNE said that he had had considerable experience in

<sup>1</sup> See Vol. VII., No. 4, p. 604.

the amputation of the cervix in the ordinary way, and of late by galvanic cauterization. His last twenty cases were operated on solely by the cauterization, for two reasons. 1. Because he had never seen inflammatory action of any kind whatever after the cauterization; and 2, because with proper care and the continual support of the uterus by a glycerine and cotton tampon, the uterus after cicatrization will be found higher in the pelvis than it was before. One patient of his, operated upon ten days ago, goes about at present and has had neither pain nor inconvenience from the operation; she is one of three operated on without anaesthetics. He thinks, with Dr. Emmet, that Dr. Pallen is in error as to the principle of his operation. He would remark that amputation by galvanic cauterization is sometimes followed by contraction of the cervical canal, which is easily relieved by careful and persistent dilatation with bougies or Dr. Nott's dilator. Only three cases of such contraction have come to his notice. Within the last six months three of the patients whose cervixes were amputated by the cauterization for elongation and sterility have been safely confined.

If the uterus is firmly grasped by a double tenaculum, and pushed up and down two or three times, the point of insertion of the vagina can be accurately ascertained.

DR. JAMES D. TRASK, of Astoria, N. Y., also a guest, read a paper on

#### INJECTIONS OF TINCTURE OF IODINE INTO THE UTERUS FOR POST-PARTUM HÆMORRHAGE.<sup>1</sup>

After recounting the manifold objections to the use of the perchloride of iron in this class of cases, the formation of large, firm coagula, which are retained in the uterus, owing to the frequent want of contraction following these injections and give rise to septic infection, the chance of the fluid passing through the tubes into the abdominal cavity, etc., Dr. Trask proceeded to relate a number of cases of the successful and safe use of tr. iodine, which he considered to be at least a desirable substitute for, if not absolutely superior to, the perchloride of iron.

DR. WASHINGTON L. ATLEE, of Philadelphia, who was present as a guest, said that no very special mention had been made of the danger of the injections of perchloride of iron, or tr. iodine passing through the Fallopian tubes into the abdominal cavity, which accident he had seen once in a non-puerperal uterus. No force whatever should be used in making these injections.

<sup>1</sup> See Vol. VII., No. 4, p. 613.

DR. TRASK said that in post-partum hemorrhage there is but little danger of this occurrence, because the cervical canal is wide open and the fluid can easily escape.

DR. PEASLEE said that the cases were not very uncommon in which the intestines had been found blackened at the autopsy of patients who had died from post-partum hemorrhage, for which the solution of the perchloride of iron had been injected. In the non-puerperal cases in which this accident has occurred, without doubt, the cervix had not been sufficiently dilated before the injection, a precaution now acknowledged to be imperative.

DR. EMMET said that the injection should be made carefully, so as not to cause the internal os to contract, and thus prevent the return of the fluid and force it through the tubes. He has generally attached a sponge to the nozzle of a syringe and saturated it after it had been passed to the fundus, and then swabbed out the uterine cavity. Iodine is thus used by him after the removal of fibroid tumors of the uterus with the result of always arresting hemorrhage and thus far preventing septic infection. If the fluid is applied first to the fundus, it produces active contractions and is at once expelled, and no ill effects follow.

DR. ATLEE said that the most dangerous cases of post-partum hemorrhage which he had seen, occurred after an impression on the stomach from a heavy meal; that no case of severe flooding after delivery had come to his notice in which the stomach was not overloaded with indigestible matter. The rule would devolve therefrom to give women in labor little or no solid food.

DR. LENTE, of Cold Spring, N. Y., also a guest, said that he had seen cases such as Dr. Atlee mentions. He has followed Dr. Wynn Williams' method of passing in a sponge attached to the syringe by a string, and pushing the piston of the syringe only far enough to saturate the sponge, which point he had learned by trial before introducing the sponge. He thought it was better to use mechanical means inside and outside of the uterus than to inject strong chemical fluids. He would have great faith in the faradic current passed through the uterus.

DR. LUSK said that he had had three cases of post-partum hemorrhage in which he injected the solution of the persulphate of iron. Two of these did well; in the third, however, the uterus remained large. On the third day the respiration suddenly became rapid and stertorous, the pulse much accelerated, and the patient died under the symptoms of embolism and septicæmia. Since then, three years ago, he has used no



intra-uterine injections, and would be very unwilling to try them again.

DR. PEASLEE said that in collapse from post-partum hemorrhage he had repeatedly found brandy introduced into the stomach and *hot* water injected into the uterus to be the best agents to produce excito-motor action and active uterine contraction.

DR. GEORGE T. HARRISON, also a guest, related the following

CASE OF POST-PARTUM HEMORRHAGE ARRESTED BY THE INJECTION  
OF CHURCHILL'S TINCTURE OF IODINE.

Having repeatedly witnessed at the Woman's Hospital, in Dr. Emmet's practice, the remarkable power of Churchill's strong tincture of iodine in promptly arresting hemorrhage when injected into the cavity of the uterus, after the removal of intra-uterine growths, etc., and having obtained like gratifying results in my own practice, I determined to adopt the suggestion made by Dr. Emmet, and use this remedy in any case of post-partum hemorrhage which might occur in my practice where its use would seem clearly indicated. I encountered such a case last June. The patient was a lady about forty-two years old. She had previously given birth to five children at term. Labors all natural. Five years ago had a miscarriage at the third month of pregnancy; about a year after this latter event, was received into the Woman's Hospital and operated on by Dr. Emmet for laceration of the cervix uteri. Health very good after the operation up to the time of confinement. June 15th, I was called, and found on my arrival labor well advanced; in fact, the child's head was pressing upon the perineum strongly. A few minutes afterward a well-developed boy was born. The placenta was delivered without difficulty and the uterus contracted properly. After applying the binder and arranging the mother comfortably in her bed, I turned my attention to the child, the nurse not having yet arrived. The child being properly disposed of, I turned around to ascertain the condition of the mother, when she asked in feeble accents for some water, and to my horror I found the face blanched. Evidently I had before me a case of sudden and alarming hemorrhage. In a moment I passed my left hand into the uterine cavity and rapidly turned out the clots, the right hand grasping the uterine body through the abdominal coverings and endeavoring to excite contraction. Contraction was soon secured, but in a few minutes the walls were again relaxed, and fresh hemorrhage ensued. By the same manoeuvre contraction was again produced; a teaspoonful of Squibb's

fluid ext. of ergot was now given by enema and I despatched a messenger to Dr. W. T. White, who lived near, to come to my assistance. Meanwhile I obtained some Churchill's tincture of iodine, a flexible catheter and a small hard-rubber syringe. On Dr. White's arrival, at his suggestion we used injections of iced water, but without securing permanent contraction of the womb. It would contract properly, but in a few minutes the walls would relax and could not be felt through the abdominal coverings. In this state of relaxation the blood would ooze forth freely. As my patient had already lost an enormous quantity of blood, I determined to inject iodine with the sanction of Dr. White. A small quantity, about half a teaspoonful, was first injected, but without any perceptible effect. On washing out the catheter, however, I found out that the catheter had been choked up by clots of blood which had passed into the opening at its end, and hence none of the iodine had come in contact with the uterine walls. Clearing out the catheter, I injected a teaspoonful, more or less, of the iodine into the uterus, and after this I had no more hemorrhage. And this notwithstanding that a permanent contraction of the uterus did not occur for several days. For the womb would contract, and through the abdominal coverings could be felt as a round ball, and in a few minutes would become again relaxed, in a short time to be again succeeded by a contraction. The posterior wall I found to be the seat of an interstitial fibroid tumor, and this, by preventing a uniform contraction of the womb, may have had some bearing on the etiology of the hemorrhage. Dr. White confirmed my diagnosis in regard to the existence of this growth. It is interesting to note that this growth underwent a retrograde metamorphosis, *puri passu* with the involution of the rest of the uterine tissue. The only trouble was that it caused a very profuse discharge of very offensive odor, and though I carefully, twice daily, washed out the uterine cavity with a solution of carbolic acid and hot water, my patient exhibited symptoms of septicæmia. By the use of quinine, tincture of chloride of iron, and good diet in addition to the local treatment, she eventually made a good recovery.

The annual election of officers was then held, and the following gentlemen were elected for the year 1874-75:

*President*, DR. JOHN BYRNE; *1st Vice-President*, DR. WM. T. LUSK; *2d Vice-President*, DR. JOSEPH E. JANVRIN; *Corresponding Secretary*, DR. EMIL NOEGGERATH; *Recording Secretary*, DR. PAUL F. MUNDÉ; *Treasurer*, DR. G. S. WINSTON; *Committee of Admission*, DRS. A. JACOBI, E. R. PEASLEE, H. F. WALKER; *Committee of Publication*, DRS. T. G. THOMAS, B. F. DAWSON, PAUL F. MUNDÉ.

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STATED MEETING, NOVEMBER 3D, 1874. THE PRESIDENT, DR. BYRNE, IN THE CHAIR.

CASE OF UNCOMMON CONGENITAL MALFORMATION OF THE ARM.

DR. JACOBI exhibited a specimen in behalf of Dr. C. F. RODENSTEIN. It was taken from a coroner's case, and consisted of the right humerus, forearm and hand of an infant prematurely born at 7 months. The hand was provided with 4 intact fingers, the thumb and radius were both absent, the carpus and metacarpus perfect. This is quite an uncommon malformation, but less rare than absence of the scapula or clavicle. The manner in which this deformity occurs is as follows: At about the 4th week the extremities begin to sprout forth, the upper slightly ahead of the lower. At end of the little stump of each extremity appear a small number of excrescences, sometimes only 3 or 4, generally more than 5, as many as 9 in number, which develop into phalanges and fingers or toes. As the extremity grows from below upwards these phalanges gradually shrink and disappear until the requisite number of 5 remains. If the thumb should accidentally atrophy the radius would also be absent. Most cases of supernumerary fingers or toes occurring hereditarily in families must be considered as arrests of development, the extra numbers are but imperfectly developed and have escaped the usual atrophy. When an imperfectly developed extremity, with small fingers or toes attached, is seen, it must be considered to be an arrest of development. A limb deformed by spontaneous intra-uterine amputation never possesses that small sprout.

DR. POOLEY said that in the *American Journal of the Medical Sciences* for April, 1864, he had reported a case of a girl twelve years of age, who had no thumb and no radius on one hand, quite analogous to the case just reported.

OVUM OF FIVE WEEKS.

DR. DAWSON exhibited an ovum of about five weeks, the result of a miscarriage from muscular exertion; the mother did not even know that she was pregnant, and believed her menstruation to have been tolerably regular all along. The fœtus is about 6''' in length, the extremities just beginning to sprout, the eyes visible; the umbilical cord was straight; near the abdominal end was a swelling, which might be either the umbilical vesicle or a mere gelatinous swelling in the cord; Dr. Dawson thought the former.

DR. JACOBI said that the specimen was exceedingly interesting and rare, as he had no doubt that the swelling at about one-third

of the umbilical cord from the abdominal end was the umbilical vesicle, which was rarely found. The age of the ovum was certainly not more than  $5\frac{1}{2}$  weeks, for the abdominal plates were not quite united.

Dr. T. G. THOMAS related a

CASE OF REMARKABLE RECOVERY AFTER CEPHALOTRIPSY AND  
PERITONEAL RUPTURE.

A little over a week ago he was called to a deformed woman who had been in labor 20 hours; several attempts had been made by the attending physicians to deliver her with the forceps, but without success. Dr. Thomas found the child dead, the pulse of the woman 120, her skin and the vagina hot and dry. No anæsthetic had previously been given, because the family strongly objected, but Dr. Thomas immediately put the patient under the influence of ether and examined. He found a rent behind the uterus of the size of a silver 25 cent piece, through which he passed his finger up into Douglas's cul-de-sac, and felt a loop of intestine which he pushed up as far as he could. The foetal head was on the brim of the pelvis; he perforated with Braum's curved trephine, seized the head with the crotchet and pulled it down until he could easily apply the cephalotribe and extract it. He, as well as the attending physicians, took a gloomy view of the case. Over a week, however, has now elapsed, the temperature of the patient has never risen, and her recovery is extremely probable.

He has seen only two other cases of vaginal rent in which recovery took place; in one case in which the peritoneal cavity was not opened, severe cellulitis and a large abscess ensued.

Dr. BYRNE asked whether anything had been done to prevent the intestines from falling down again after delivery.

Dr. THOMAS said that he had thought of this occurrence, but had preferred not to do anything after the operation, as the family were very much demoralized by the case. The intestine was watched, but no signs of its prolapse manifested themselves. He would remark, moreover, that he has rarely seen the small intestine in Douglas's cul-de-sac in the non-pregnant woman, never during pregnancy.

Dr. PEASLEE said that the small intestine never comes down into Douglas's cul-de-sac in a non-pregnant woman, but certainly not when the pelvis is filled up by a large body, like the uterus immediately after delivery.

Dr. THOMAS, in this connection, referred to an operation which he had performed several years ago as a substitute for Cæsarean section, and which he had then described and called

## GASTRO-ELYTROTOMY.

In a moribund woman, near the end of pregnancy, he made an incision in the inguinal region, parallel with Poupart's ligament down to the peritoneum; then with the sound he pushed up the vagina and cut down upon it, enlarging the opening with the scissors. The hand was then passed through the large incision into the cervix, which had been dilated with Barnes's dilator, the feet of the child were seized, version was performed, and the child extracted; it was alive and lived several days, dying from causes in no way connected with the operation. The woman was pulseless, and died in four hours. The time occupied in performing the operation was very short, not longer than necessary for Cæsarean section. Dr. Thomas thought that he could perform the operation and deliver the child in five minutes. He did not mean to say that he would perform this operation always in preference to Cæsarean section, and was well aware of the dangers from cellulitis, septic infection and hemorrhage, especially in the vascular condition of the vagina during pregnancy, but he was inclined to think that the operation had a future. As far as he is aware only one operation of the kind has since been performed, by Dr. Skene, of Brooklyn; the child had been perforated before, and the woman, exhausted previously to the gastro-elytrotomy, died in seven hours. Dr. Thomas thought that drainage through the vagina would diminish the chances of septicaemia. From the description of the operation one might think it a very difficult one; this is not the case, however, for the operation is surprisingly easy in every detail.

## CASE OF CRANIOCLASIS.

DR. MUNDÉ related a case in which he had not been as successful with Brann's cranioclast<sup>1</sup> as he had expected to be after his experience with the instrument on the cadaver, in Vienna, and the universal good results he had seen from its employment in the lying-in wards in that city. In June, 1873, he was called at 5 o'clock A.M. to see a woman, multipara, who had been in labor some forty-eight hours, and was very much exhausted. The head of the child, which was evidently dead (the prolapsed umbilical cord being pulseless), was fixed on the brim of the pelvis too high to enable its being firmly grasped by the forceps, which had been previously repeatedly applied by Dr. Wakeman, the attending physician, without success, but not high or movable enough to make the easy introduction of the hand be-

<sup>1</sup> See Vol. VI., No. 1, p. 1.



side the head and podalic version practicable. On consultation it was thought best to wait a few hours until the head should descend, when the forceps could be applied or craniotomy performed. Braun's colpeurynter was introduced into the vagina for the purpose of exciting uterine contractions, which had entirely ceased and still more dilating the os uteri, and brandy and aromatic spirits of ammonia were given the patient. Five hours later, at 10 A.M., the colpeurynter was removed, and the head found scarcely advanced into the pelvis, which was not deformed. No uterine contractions had occurred. It was decided to perform craniotomy, as probably the easier operation, in preference to trying to extract the evidently very large head with the forceps. The head was perforated with Braun's curved trephine, and the cranioclast applied; unfortunately the elevation of the head and its impaction in the superior pelvic strait prevented Dr. Mundé, notwithstanding repeated trials, from getting the outer blade over the face, as should be done; consequently, after drawing the head down a little, the bones of the head tore out, and the cranioclast had to be reapplied. This was done three times, passing it up as far as possible, but the child having probably been dead some time, the calvaria had become so soft and friable that the least traction completely separated the bones and scalp grasped between the blades from the bulk of the skull. After seeing the uselessness of further attempts with the cranioclast, the head was seized by a crotchet, drawn down and extracted after a great deal of hard traction by both the attending physician and Dr. Mundé, who were completely exhausted by their exertions. It required some force also to extract the unusually large body of the child, the shoulders having become wedged into the pelvic outlet. Unfortunately the child was not weighed, there being no suitable scales at hand. The woman suffered from a severe attack of pelvic cellulitis, but recovered after a prolonged illness.

#### CASE OF FIBRINOUS POLYPUS.

DR. MUNDE further related the continuation of the case of hydatidiform mole, which he had reported to the Society at the meeting, May 19th, 1874.\* He saw nothing of the patient for two months, when he was again called and found that she had been flowing more or less since the removal of the mole, for which various internal hemostatics had been given ineffectually by the family physician. On examination the uterus was found to be somewhat enlarged, and the os slightly patulous. Dr.

\* See Vol. vii., No. 3, p. 475.

Mundé suspected that some portion of the mole was still retained in the uterus, and was the cause of the hemorrhage. For the sake of diagnosis and radical treatment he therefore dilated the cervix fully with sponge tents, and on passing the finger into its cavity, he found a tolerable firm tumor of the size of a large English walnut attached to the posterior uterine wall. With the aid of Dr. Treskatis, the family physician, this tumor was thoroughly removed with the curette, and every vestige of excrescence or granulation scraped from the internal uterine surface. The patient made a good recovery, and the metrorrhagia did not return. On examining the tumor, which macroscopically had the appearance of a fungoid sarcomatous mass, under the microscope it was found to consist of distinct placental villi, blood-corpuscles, and fibrous tissue, and was, therefore, undoubtedly what is known as a "fibrinous, or placental polypus" (Scanzoni, *Lehrb. der Weibl. Sex. Org.* I., p. 297), formed by the attachment of coagulated blood to the remains of the molar placenta. The importance of carefully examining the uterine cavity, and removing all remnants of a mole, or the placenta after abortion, particularly when the ovum was discharged piecemeal, thus becomes apparent.

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STATED MEETING, DECEMBER 1ST, 1874. THE PRESIDENT, DR. BYRNE, IN THE CHAIR.

#### CLAMP FOR COLPORRHAPHY.

DR. DAWSON exhibited a clamp with teeth devised by him for narrowing the vagina; it is designed after that of Dr. Thomas, and is to be left in the vagina until union has taken place.

DR. NOEGGERATH said that he uses a semicircular clamp with teeth, with which he catches up as large a portion of the prolapsed vagina as possible; under this he lays wire sutures and then cuts off the enclosed piece of the vaginal mucous membrane and closes the sutures. In this manner a much greater constriction of the vagina is obtained, than by simply excising a straight fold, or leaving the clamp in the vagina. He has never caused an injury to the bladder when his clamp was used, and hemorrhage was always avoided. The *rationale* of his operation is, that a semicircular constriction of the vagina produces more traction on the lateral vaginal attachment, and therefore more chance of permanent benefit, than the excision of a mere longitudinal fold. In one case on which he operated in this manner, the upper vaginal wall was first con-

cave after the operation; two years later, there had been no relapse, only the upper wall of the vagina had become slightly convex.

DR. PEASLEE thought that Dr. Dawson's clamp was an improvement on Dr. Thomas', but that with the handle and screw it was too long to leave in the vagina. He would not be so sure as Dr. Noeggerath that the bladder could not be injured during the operation for cystocele; he had often found the vaginal mucous membrane very closely united to the bladder and very hard to detach. He prefers dissecting off the mucous membrane and then removing it. He related a case in confirmation of Dr. Noeggerath's theory of operation in which he operated both for rectocele and cystocele; the patient wore a pessary for one year, and now after two years there is no relapse and the uterus remains high in the pelvis. When the operator has no clamp, he may secure a similarity of both sides of the incision by using a copper director grooved on both sides, which is bent at the desired curve, passed through an incision under the vaginal mucous membrane, and one-half of the incision traced out; the sound is then turned to the other side and the other half of the incision marked. Without a clamp there is always likely to be much hemorrhage.

DR. BYRNE thought that a clamp is of great assistance in the operation of excision of portions of the vaginal mucous membrane, and that it serves to avoid often profuse hæmorrhage. As to the permanency of the result to be derived from the operation, he confessed that he was disappointed; he had been unfortunate in this respect, and had lost all confidence in this operation for the relief of vaginal or uterine prolapse. If a firm submucous adhesion could be obtained by running a heated platinum wire in columns under the mucous membrane, permanency of constriction might possibly be gained.

#### NEW GALVANIC CAUTERY BATTERY.

DR. BYRNE exhibited a galvanic cautery battery which he had just constructed, and the small size and great power of which confirmed the predictions made by him at a former meeting.<sup>1</sup> The glass jar holding the element is only 6" by 4" by 3" in size; the battery consists of eight negative poles of copper covered with platinum foil  $\frac{1}{1000}$  inch thick, put on by the process called hand-plating; the edges of the copper where the platinum foil comes together are filled in with lead. There are 4 zinc plates, that is one-half the superficial amount of the copper. The diameter of the elements is  $1\frac{1}{4}$ " by  $4\frac{1}{2}$ "; the

<sup>1</sup> See Vol. VII., No. 3, p. 479.

quantity of metal surface, negative and positive, under action is 28 square inches. This battery will suffice for all ordinary operations, probably for 19 out of 20 in which the galvanic canterry is applicable. It heats 9" of heavy platinum wire. The fluid used is a solution of bichromate of potash in dilute sulphuric acid. Dr. Byrne set the battery in action in presence of the Society, and instantaneously heated to a white heat a large loop of heavy platinum wire and a cone made of 5" of No. 17 wire. Ordinary No. 22 platinum wire was melted in a few seconds by the intense electrical heat generated. The conductors used were almost 5 feet long. For the purpose of removing the particles of hydrogen from the surface of the negative element and exposing them continually to the influence of the battery fluid, thus intensifying the strength of the current, a stream of air was forced into the fluid by means of a small rubber bulb and tube attached while the battery was in action. Any other manner of displacing the particles of hydrogen would answer equally well. The actual cost of constructing this battery should not exceed \$25.

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## TRANSACTIONS OF THE PHILADELPHIA OBSTETRICAL SOCIETY.

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STATED MEETING, DEC. 3, 1874. DR. J. L. LUDLOW, VICE-PRESIDENT,  
IN THE CHAIR.

### PELVIC CELLULITIS FROM ABORTION.

DR. R. G. CURTIN read the history and presented a specimen of a case of pelvic cellulitis caused by an attempt to produce abortion. On post-mortem examination there was found a small puncture on one side of the cervical canal, and an abscess in front of the uterus and back of the vesico-uterine fold.

DR. LUDLOW referred to the great prevalence of the habit of producing abortions by the use of such instruments as catheters, probes, quills, knitting-needles, etc., and thought it was the duty of every physician to warn his patients of the danger arising from the operation. He attributed it in some cases to the influence of popular lectures to women.

### EPITHELIOMA OF THE CERVIX UTERI.

DR. J. L. LUDLOW presented a specimen of epithelioma of the cervix uteri. The patient from whom the specimen was

removed had suffered from intense pain and hemorrhages, and had been treated with various applications, such as nitric and carbolic acids, etc., without relief. He removed the proliferating mass in fragments with considerable loss of blood, and made an application of a mixture of equal parts of carbolic acid and a solution of bromine 3 j. to 3 vii. of water. He then dressed the parts with the extract of the root of the *phytolacca decandra*.

The patient improved under this treatment, and in three weeks there was no trace of the disease left.

Dr. Ludlow remarked that he had been led to use bromine in this manner by the remarks of Dr. Wynn Williams before the London Obstetrical Society. He also instanced another case similar to the former, in which the patient improved so much under two or three applications of the carbolate of bromine, that she is now pregnant. He thought the mixture well worth trial if only to remove the extreme fetor.

Dr. PARRY said that the specimen was from a case of villous cancer of the os uteri. He had had several cases of the kind, and was thoroughly impressed with the advantages of the method of treatment of Prof. Simon, of Heidelberg, viz. scraping away the diseased mass. If the tissue of the uterus is not involved the simple removal of the growth will restore the patient temporarily to health. He had already reported one case to the Society which he treated in this manner with the effect of affording complete relief for a time.

Dr. LUDLOW thought that in these cases of cancer of the uterus he had noticed a stale, albuminous odor which he thought characteristic.

Dr. CLEEMANN referred to a case which he had treated by scraping, with the effect of stopping the hemorrhages. He always used a strong solution of tannin in glycerine for arresting the hemorrhage from the operation.

#### RENAL DISEASE WITH EARLY FŒTAL DEATH.

Dr. A. H. Smith gave the history of a case of renal disease, with fetal death, occurring at an early period of pregnancy.

In April, 1871, he was called to a patient who presented symptoms of labor at about six and a half months of pregnancy. She was twenty-three years of age, had been married two years, and had previously had two abortions, one at two and a half months, and the other at four months. She was not able to assign any cause for these. He found her in quite active labor, the pains occurring at short intervals. There were nausea, violent pain in the head, slight tendency to convulsions, and a full and frequent pulse. He attempted at first to arrest the



labor by opium suppositories, with the effect of lulling the pains, but they recurred with increased activity.

In thirteen hours she was delivered of a child of about five months' development. The placenta was atrophied, and presented in several places large organized clots. The patient recovered slowly, requiring stimulation to bring her up again to a normal condition.

The urine at the time of delivery was albuminous, but there were no tube-casts or oil-globules. The albumen gradually diminished, and the urine returned to a healthy condition after delivery.

In 1873 he saw her again, suffering from menstrual irregularity. A week later nausea, etc., set in, and he informed her that she was again pregnant. In the third month, examination of the urine revealed a small amount of albumen, but no tube-casts. The albumen increased in amount until the sixth month, when the urine contained it so largely as to become of a semi-solid consistence, on the application of the usual tests.

At four and a half months the patient quickened; at the fifth month she was seized with cramps of a peculiar character. Upon placing the hand upon the abdomen at certain times, the uterus was found to harden, and was thrown violently forward with great tension of its tissues. These cramps occurred perhaps three or four times a day. He had noticed this peculiarity in three cases of placental disease.

Symptoms of uræmic poisoning had existed during the whole of the pregnancy.

There was no œdema present, but nausea at times caused the rejection of all her food. He treated her actively, to prevent the development of œdema. On December 10th, 1873, the motion of the child ceased, and symptoms of constitutional poisoning from the presence of a dead fœtus set in. She went on to full term, and on the 22d of January, 1874, fell into labor, and was delivered of a child of about six months' development. The placenta was atrophied, softened, and contained organized coagula. The fœtus was soft and the bones cartilaginous in character.

He examined the urine frequently after the death of the child, and found the albumen gradually diminishing. The patient menstruated one month and then became pregnant again. During this pregnancy she was quite well, with no symptoms of uræmic poisoning. She was of good color, and in full muscular vigor. The pregnancy advanced favorably and without bad symptoms, until October 15th, 1874, when the motion of the child ceased. Fearing the bad symptoms of the previous pregnancy, he induced premature labor. He used a bougie for

that purpose without the slightest effect. Hot and cold douches were also used, and large doses of ergot. The latter brought on labor, and the child was born in the sixth month of gestation, but only of about four months' development.

The patient is now well (December 3d, 1874), and has a good color, good digestion and appetite.

The urine now shows again diminution of the albumen. In this last pregnancy there were also the same cramps as before, and the placenta had the same characteristics as in the other pregnancies.

DR. SMITH remarked further that there were some peculiar features in this case. The condition of pregnancy was the exciting cause of the condition of the kidneys. This did not occur as in ordinary cases of albuminuria, but was manifested soon after pregnancy.

The albumen in the urine steadily diminished between each delivery, and in the next pregnancy oedema was absent during the whole history of the case. There was no puffiness of the eyelids or oedema of the feet or face. The character of the cramps was also peculiar.

DR. ELLWOOD WILSON remarked that the case seemed to be one of syphilitic taint. The difficulty could be remedied by the continued use of iodide of potassium. He did not think the presence of albumen in the case necessarily showed the existence of congested kidneys; he had seen in similar cases the urine thick with albumen.

DR. SMITH replied, that he had frequently seen cases in which repeated abortions were due to syphilitic disease, and a few years ago he should have considered this case as one of syphilis. He had, however, every reason to believe that there was no taint whatever in the case. The father he had known for years as a strong vigorous man. He had seen cases from time to time, before he had examined into the condition of the placenta, in which he had supposed the condition to result from syphilis. In this case he had found only an atrophied condition of the placenta, without any evidence of degeneration. He had seen cases in which young men who have had syphilis have married and had healthy children. Because renal disease often results in poisoning of the system, there was no reason to regard this case as one of syphilis.

DR. JOHN S. PARRY remarked, that he had had the urine of many cases of syphilitic pregnancy examined, and his experience would lead him to conclude that these cases were not more subject to albuminuria than others. He had repeatedly seen women impregnated with syphilis, carry their children to term in a well-developed condition, but the children showed evi-

dences a few weeks after birth of constitutional disease. He agreed with Dr. Smith that we are too prone to regard syphilis as a cause of abortions. He agreed with him also, as to the influence of syphilitic fathers on their children. He had known healthy children begotten by a syphilitic father. According to some authorities, the mother does not become infected through the child, but is inoculated by contact with the husband's secretions, etc.

DR. CURTIN remarked, that the German writers recognize syphilitic albuminuria, and its peculiarity is its amenability to treatment.

DR. PARRY said he hoped that Dr. Curtin did not think that he had not recognized the condition of syphilitic albuminuria referred to. He believed in syphilitic disease of every organ of the body.

DR. E. WILSON could not believe in the possibility of a healthy child being born of a diseased father. He instanced several cases in support of his views, in which young men who had contracted chancres, followed by little or no constitutional symptoms, married after the lapse of years and had syphilitic children.

He believed also that a succession of pregnancies will eliminate syphilis from the father.

#### VIABILITY OF FÆTUS AT SIX MONTHS.

DR. PACKARD reported the following case:

Mrs. H.—, æt. about 20, has one child less than a year old. Was unwell in June, after missing several periods; again in July, and again in August. In September she had what her mother said amounted to flooding for nearly or quite three weeks, and in October she again had a slight bleeding, which she thought was her regular menstrual flow.

On the 25th of November, having suffered from abdominal pains through the day, she had, as she supposed, a call to stool, and sitting on the commode, was suddenly delivered of a child, which fell into the vessel, and was taken out by the grandmother, who was present. The child lived, kicking vigorously, and crying, but not opening its eyes, for fifteen minutes.

Mrs. H. had no idea that she was pregnant; had had no quickening, and had felt no motion.

Similar instances were reported by Drs. E. Wilson and Cleemann.

#### ABORTED OVUM—ABSENCE OF FÆTUS.

DR. DE FOREST WILLARD presented an aborted ovum which presented some interesting features. The patient was a young,

healthy woman, nursing a child of 16 months of age, and did not know she was pregnant. She had had severe pains for two months, until, after fulfilling a dentist's engagement, the pains increased, and she was delivered of an ovum apparently of two months of age. On opening the chorion he found that the amniotic sac was dropsical. He could not find any evidence of a fœtus. Under the microscope the villi of the chorion were beautifully shown, but there was not the slightest sign of a fœtus.

Dr. A. H. SMITH asked whether there was any evidence of vesicular degeneration of the chorion.

Dr. WILLARD replied that the chorion was perfectly normal.

#### RARE FORM OF DEATH FROM CANCER OF THE UTERUS.

Dr. JOHN S. PARRY exhibited a specimen which illustrates rather a rare mode of death from uterine cancer. The woman from whom the specimen was taken had been treated in various hospitals for hemorrhage from the vagina, but there was nothing peculiar in her case until three weeks ago. Careful examination had revealed that the cervix and half the body of the uterus were destroyed by cancer, and that the vagina communicated with the bladder by a small orifice.

Three weeks ago she complained that her urine was arrested. There were symptoms of uræmia, such as headache, vertigo, profuse vomiting, but no anasarca. In a few days the secretion of urine reappeared; in a little while it was suppressed, then again appeared, and a few days ago it was arrested for the last time. She died with symptoms of uræmia.

On post-mortem examination there was found an opening between the bladder and the vagina. The cervix and part of the body of the uterus, and the adjoining part of the vagina, were destroyed.

There was a thickened mass to the right of the bladder and uterus. The right kidney and ureter were greatly distended with urine. The obstruction existed at the entrance of the ureter into the bladder, and was caused by inflammatory thickening. The left kidney was natural. Dr. Parry had seen three cases of uræmia from cancer of the uterus.

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STATED MEETING, JAN. 7, 1875. DR. J. L. LUDLOW, VICE-PRESIDENT, IN THE CHAIR.

The following officers were elected for 1875:

*President*—Dr. A. H. SMITH.

*Vice-Presidents*—Dr. JOHN S. PARRY, Dr. EDWARD WALLACE.

*Secretary*—Dr. J. V. INGHAM.

*Treasurer*—Dr. D. MURRAY CHESTON.

*Curator*—DR. WILLIAM F. JENKS.

*Publication Committee*—DR. WM. GOODELL, DR. J. H. PACKARD, DR. W. F. JENKS, DR. R. G. CURTIN.

*Council*—DR. LEWIS D. HARLOW, DR. R. P. HARRIS, DR. ELWOOD WILSON, DR. J. F. WILSON.

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STATED MEETING, THURSDAY, JAN. 7, 1875. DR. A. H. SMITH, PRESIDENT, IN THE CHAIR.

DR. GOODELL read a letter from Dr. A. Douglass Hall, presenting to the Museum of the Society a lancet case containing four lancets which were used by Prof. James in his daily practice.

#### CONVENIENT METHOD OF EXAMINING OF THE BRAIN IN INFANTS.

DR. R. G. CURTIN described on a cadaver a convenient method of making a post-mortem examination of the brain, only applicable, however, to cases of infants. The scalp is divided in the usual manner, and instead of taking off the calvarium he divides the sagittal, fronto-parietal and lambdoidal sutures, and in this manner is able to separate the parietal and occipital bones sufficiently to allow the brain to be removed. When the brain is replaced the bones spring back to their usual position. The benefit of this method is its convenience, and the absence of disfigurement of the head by the mobility of the separated calvarium.

#### ABORTED OVUM.

DR. ELLIOT RICHARDSON exhibited an aborted ovum of about eight weeks' gestation, as nearly as could be ascertained. On examination of the walls of the sac a thick portion composed of clot apparently between the membranes was found. There was a small sac of delicate membrane attached to the larger one. The large cyst contained a muddy fluid, but there was no trace of a foetus.

The specimen was carefully examined with the microscope, and the presence of the villi of the chorion proved it to be a product of conception.

DR. E. RICHARDSON also presented a specimen of a uterine myoma.

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## QUARTERLY REPORT ON OBSTETRICS AND DISEASES OF WOMEN AND CHILDREN.

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### REPORT ON DISEASES OF WOMEN FOR THE YEAR 1874.

By PAUL F. MUNDÉ, M.D.

(Read before the Medical Library and Journal Association, June 25th, 1875.)

MR. PRESIDENT AND GENTLEMEN :

As I had expected, when I accepted the invitation extended to me by your Secretary to prepare a report on the progress of gynecological science during the past year, I have found this task an interesting but withal a somewhat difficult and laborious one, principally for the reason that the amount of valuable and instructive periodical literature in this department is so great, greater probably than in any other branch of medical science, except that of surgery, that it was extremely perplexing to choose from this superabundance what was likely to prove most desirable for a Report of this kind. I have endeavored, while making no claim to completeness, to mention all those subjects at greater or lesser length, which appeared most interesting and important to me, and I trust the selection will meet with your approval. I have particularly laid stress on those points in which actual progress seems to me to have been made, or which I thought might be less generally known or appreciated than others. For the sake of method, I have followed the plan usually adopted by the text-books, and have arranged the subjects in the order from within outwards, commencing at the vulva and concluding with the ovaries and the general system, endeavoring, when practicable, so to connect and blend the different topics as to deprive the Report of the monotony it might otherwise possess. After this introduction, I will proceed at once to the discussion of the first subject in order—the *diseases of the female uro-genital organs*.

The *gradual dilatation of the female urethra by sponge-tents, laminaria, and the passage of the finger into the bladder*, after having been proposed by Sir Astley Cooper for the purpose of extracting vesical calculi, and revived by Huguier (*Sur les allongements hypertroph. du col. de l'utérus*) in two cases in 1860, and Pippingsköld, of Helsingfors (*Berlin Beitr. z. Geb. u. Gyn.*, III., 2), in three cases in 1874, with the object of preserving the bladder from injury during the amputation of the cervix uteri, has been supplanted principally during the past year by the equally safe and thorough and much less tedious method of *rapid and forcible dilatation*, by means of instruments or the finger. Doubtless some physicians, both at home and abroad, may have practised this operation now and then; but prior to the year 1874 I have been able to find only two publications on the subject, both in 1872, by Hybord, of Paris (*Des calculs de la vessie chez la femme*), and Simonin, of Nancy (*Mém. Soc. de Méd. de Nancy Compte-rendu*, 1871-72), the former of whom employed the dilatation for the extraction of stone.

CHRISTOPHER HEATH, of London (*Med. Times and Gaz.*, April 11, 1874), was the first to publish an article on forcible urethral dilatation, in 1874; he introduces first the little finger on a director, and then the index finger, in contracted urethrae beginning the dilatation with a pair of dressing forceps run along a curved director. Heath, like the majority of those acquainted with this method, employed it only for the removal of urethral caruncles and organic or spasmodic stricture of the canal, for hyperæsthesia of the urethra and vesical neck—tenesmus—and in chronic vesical catarrh. SIMON of Heidelberg (*Letter from Dr. Nathan Bozeman, N. Y. Med. Rec.*, Vol. IX., p. 603, 1874), SPIEGELBERG of Breslau (*Berl. M. Woch.*, No. 16, 1875), THOMAS, NOEGGERATH, and PERRY, of New York (*Meeting N. Y. Obst. Soc.*, Feb. 16, 1875), appear to be the only gynecologists who, besides dilating the urethra in the cases mentioned above, have introduced the finger into the bladder for the purpose of detecting tumors or other pathological conditions of that organ, who have, indeed, practised the *vesical touch* proper. At the meeting of the N. Y. Obst. Society referred to, Dr. Noeggerath reported a number of cases in which he had extended the range of the vesical touch, he having used the finger in the bladder as a means of palpating the uterus and adnexa, an adaptation of this method entirely new and original with him, and one likely to have a brilliant future. His paper appeared in the *Am. J. of Obst.* for May. I believe Simon has almost simultaneously with Noeggerath recommended and practised the same manipulation. Simon and Spiegelberg have both detected and removed tumors of the bladder by this method, and thus cured old, previously inexplicable cases, and the latter has punctured a hæmatometra which was inaccessible through the vagina on account of complete closure of the canal near the normal entrance, through the bladder with complete success. As regards the instruments used in dilatation, Simon employs graduated bougies or special bivalvular specula, first incising the border of the meatus; Spiegelberg uses Busch's bivalvular uterine dilator, without the upper blade; Noeggerath dilates with graduated steel bougies and the little and index fingers; Thomas with a forceps similar to a dressing forceps; a useful instrument is Goodwillie's wire-spring nasal speculum. In all cases the dilatation occupies but a few seconds or minutes, and is never followed by permanent incontinence. All operators agree in this statement. Heath says, that in all cases of rapid dilatation the mucous membrane is split beneath the pubis, and that the incontinence rarely lasts longer than twenty-four hours. The operation is always performed under anæsthesia. Its benefit is usually instantaneous and generally permanent in well-chosen cases.

DR. WILLIAM GOODELL has an excellent clinical lecture on "*Affections peculiar to the Female Urethra.*" in the *Phil. Med. Times*, for Oct. 24th, 1874. Strictures are rarely met with. One of the most common disorders is urethral caruncle. These intensely vascular and exquisitely sensitive growths are very common and occur more frequently in married than in single, especially in middle-aged, women; they do not appear to be due to gonorrhœa, unclean habits, or frequent coition, but are usually accompanied, perhaps preceded, by some form of uterine disease; although at times perfectly painless, they are often the cause of the most excruciating pelvic and vesical pains, and in course of time bring about alarming symptoms of general debility, nervousness, and depression, apparently quite out of proportion to the size of the tumor. The caruncles may occupy any portion of the urethra, and are most difficult of treatment when situated near the bladder and sessile in their attachment. They may be destroyed by caustics, such as crystallized carbolic acid, nitric or chromic acid, or be removed by the scissors (followed by the immediate application of nitric acid,

nitrate of silver or the actual cautery, to check the usually profuse hemorrhage), or by the galvano-caustic loop, or its less expensive substitute, a red-hot knitting-needle or hair-pin. Forceful dilatation of the urethra will sometimes be found necessary to prevent the return of the tumor and effect a permanent cure. The after-treatment should consist in the application twice a week of the undiluted commercial carbolic acid, until the surface has skinned over. Other less common affections of the urethra are: *Granular erosion of the mucous membrane*, a very painful affection, but generally easily relieved by the application of the undiluted carbolic acid, on a pine stick, once a week, followed by the introduction of olive oil. *Prolapse of the mucous coat* is more common in children, although occasionally seen in adults, and is distinguished from a caruncle by not being very sensitive, and not bleeding readily, and by being circular in shape; it may be cured by snipping off a piece of the mucous membrane, or by touching it with nitric acid in a narrow streak around the circumference. *Polypus and cancer of the urethra* are rare affections, and are treated on general principles.

A rare and obscure form of urethral disease, *fissure of the urethra*, near the neck of the bladder, has lately been described by SPIEGELBERG (*Berlin kl. W.*, 16, 1875), and cured by hyper-dilatation and cauterization with lunar caustic. DR. C. H. F. ROUTH, of London, reports eight cases of *urethral caruncle* (*Obst. J. Gr. Br. and Irel.*, Dec., 1874), in six of which he excised the growth and applied either tincture of steel or nitric acid to the bleeding surface (he prefers the latter caustic, because more effectual and less likely to be followed by pyemic poisoning, a case of which he saw in the practice of a colleague). In the two other cases, both in *bona vivantes*, exceedingly stout women, the tumors appeared to be due to the mode of life of the patient; in one case the caruncles were found to have vanished spontaneously after several months of enforced regularity of living; in the other, the growth was excised, allowed to bleed freely, and never returned.

*Idiopathic catarrhal urethritis in females* is described by DR. A. W. SAXE, of Santa Clara, California (*Pac. Med. and Surg. Jour.*, April, 1874), as a notoriously common disease in California during the changeable seasons of the year, resulting from exposure to cold and moisture. It occurs in females from the ages of fifteen to that of seventy-five years, and may be accompanied by more or less cystitis. The origin and nature of an attack of catarrhal urethritis is similar in such cases to that of a simple nasal catarrh.

The symptoms are: excessive pain and tenesmus preceding the evacuation of the bladder, paroxysms returning after five to fifteen minutes, as soon as there is slight urinary accumulation, pulse contracted, tense, slightly accelerated, extremities cold, skin dry, tongue furred; urine unchanged or slightly excessive. The tendency of the disease is to a spontaneous cure in from two weeks to three months. Structural changes in the membranous walls of the urethra, complete occlusion of the canal, partial or temporary paralysis of the sphincter, chronic urethritis and cystitis, are among the results of protracted local inflammation. There is no muco-purulent discharge—an important differential sign from gonorrhoea. The treatment used by Dr. Saxe in preference to remedies acting specifically on the bladder and urine consists in, 1, horizontal position in bed; 2, hot applications to the feet, with hot diluent drinks, so as to induce diaphoresis if possible; 3, the administration every two hours of the following: R Hydrarg. chlor. mitis, gr. xxxvi.; pulv. ipecac, gr. viij. M. Divide in chart, No. xii. Sig. dose, one powder in a little syrup every two hours until six are taken. Then follow with decoct. sennæ, q. s. to

move the bowels freely. Diet, light and simple, mucilaginous drinks, slippery elm or flaxseed tea, etc. The only sedative necessary is a cold wet napkin to the vulva, frequently changed. Opiates are injurious. If there is no decided improvement after twelve hours, give the remaining powders. Keep the bowels open by saline laxatives, and a sure and speedy recovery is certain.

The local treatment of cystitis in the female sex has been specially recommended by BRAXTON HICKS (*Brit. Med. Jour.*, July 11, 1874), who, while acknowledging the favorable influence of internal remedies combined with rest and tonics on the disease, believes that severe cases are more readily cured by local applications, that is, injections. The character of the urine, whether it is acid or alkaline, will determine the nature of the fluid to be injected, which, in the former case, will be composed of alkalies, in the latter of some mineral acid, both combined with opium. A severe case of acute cystitis with great pain, tenesmus, and scalding during micturition, in which there is an elevation of temperature and the urine contains pus and mucus, should be treated as follows: The urine is first examined as to its acidity or alkalinity; usually the latter is found to be present. An elastic catheter, open at the point and not at the sides, is passed into the bladder; the urine evacuated, and the catheter withdrawn to the neck of the bladder to prevent the mouth of the instrument from being occluded by the vesical mucous membrane; slightly acidulated warm water is then injected until the bladder is cleansed of all mucus, and a solution of one grain of morphine to one ounce of water injected, the catheter removed, and this solution retained as long as possible. It is very rare to find even this first topical application unproductive of at least a slight degree of benefit. In severe cases it should be repeated twice daily. Frequent catheterization is not as injurious as the repeated voluntary passage of the irritating urine. The quantity of morphine may be increased, but in case of bloody discharge, the fact must be borne in mind that this is probably due to an abrasion, and that consequently the morphine is more readily absorbed. Unless the urine be exceedingly alkaline, the solution of morphine may be injected at once; usually, however, it is best to precede it by an injection of warm water or strongly diluted liquor potassæ. As soon as the acute symptoms subside astringent injections should be employed, such as solutions of two to three grains of tannic acid or three to four drops of the tincture of the muriate of iron to the ounce of warm water, to be followed by the usual morphine injections. In a fortnight the disease will have been much alleviated; unusual sensitiveness of the urethra is relieved by light canterization with lunar caustic or perchloride of iron, or a bougie dipped in mucilage and sprinkled with tannic acid is introduced into the canal. Tepid sitz baths, rest in bed, proper diet, and regular defecation will aid the local treatment above mentioned; alcoholic drinks of all kinds should be avoided.

In chronic cystitis, injections of a solution of nitrate of silver (grs. 6-10 to  $\frac{3}{4}$  j.) are very serviceable; they are to be repeated once a week, if necessary. Solutions of perchloride of iron are also beneficial. The irritable condition of the bladder remaining after acute and chronic cystitis is greatly relieved by the morphine injections mentioned above.

This exceedingly obstinate affection—*chronic cystitis*—in aggravated cases formerly defied all the perseverance of the patient and ingenuity of the physician, and resisted all remedies, both local and general, until the operation of creating an artificial vesical fistula, to permit the free drainage of urine from the bladder, was proposed by Mr. Guthrie, of London, and subsequently performed by Sir James Y. Simpson, Drs. Emmet, Bozeman, Parker, and others. This operation proved to be of the greatest benefit to the patients,



particularly those of the female sex, in whom the vesico-vaginal septum offers a favorable spot for the opening. There are, however, some difficulties attending this method, one of which is the unwillingness occasionally felt by both patient and physician to submit to and perform an operation of this kind, and another, the difficulty experienced in keeping open the artificial vesical fistula. The latter objection has been removed by Dr. Emmet, who with his well-known ingenuity constructed a hollow button of glass or hard rubber, with its upper and lower edge everted, which is grasped and held fast in the opening by the vesico-vaginal septum, and prevents the fistula from closing. Still, efforts have been made to obviate the necessity of creating an artificial vesico-vaginal fistula, and Dr. HUNTER MCGUIRE, of Richmond, Va. (*Virginia Medical Monthly*, April, 1874), relates a case of chronic inflammation and ulceration of the bladder, of eight years' duration, which had been subjected in vain to all known local and general remedies, and which he cured by the introduction into the bladder through the urethra of about four inches of fine rubber tubing, perforated with holes about half an inch apart. The tubing was passed into the bladder through a silver catheter, the opening of which was at the end and not the side, and attached to the thigh of the patient by a rubber band; the open end was connected with a rubber urinal. For six weeks the tubing was retained in the bladder, being expelled occasionally or removed to be cleansed or replaced by new material. During this time the patient had kept her bed; afterwards she was allowed to walk about and attend to her household duties, still wearing the tubing until it was removed at the end of four months. Some incontinence of urine then remained, but gradually the bladder regained its power, and eight months after the drainage was commenced the patient was entirely cured.

Dr. J. GOODMAN, of Louisville, Ky., reports a case (*Amer. Jour. Obst.*, Nov., 1874) of chronic cystitis of four years' duration, in which he introduced a self-retaining hard rubber catheter of peculiar shape designed by him, which was worn by the patient, an unmarried Irish girl, for about twenty months (being removed and cleaned every three or four days) until she was finally induced to give up wearing it, the bladder having fully regained its normal condition. The catheter was worn four or five months longer than necessary, because the patient was unwilling to give up what had been a source of so much comfort to her. The bladder had regained its normal retentive power, and no diminution was apparent in its capacity. Dr. Goodman says that his experience leads him to believe that any case of chronic cystitis, uncomplicated by disease of the neighboring viscera or serious constitutional dyscrasia, may be cured by drainage, unaided by local or general treatment.

At a meeting of the London Obstetrical Society (*London Obstetrical Journal*, Dec., 1874), eleven cases are spoken of in which *recto-vaginal and vesico-vaginal fistulae* were caused by the wearing of various pessaries. It was generally conceded that all pessaries should be removed at least once a month, the parts bathed in a douche and permitted to recover their tone. In some cases of fistula there had been no evidence of trouble until the mischief was done.

A case of cure of a vesico-vaginal fistula by local applications is related by Dr. WM. GOODELL (*Amer. Journal of Obstetrics*, Nov., 1874), as follows: On the tenth day after a difficult labor he found the urine dribbling from a patient. On examination he found a slough as large as a silver quarter near the neck of the womb, and in it an opening communicating with the bladder. He cauterized the whole slough with fuming nitric acid, repeated this application three days later, and on the intermediate and following days applied a solution of one drachm of nitrate of silver to one ounce of



water. For five days all the urine escaped from the vagina. It then began partly to be passed from the urethra, in very small quantities at first, but daily in increasing amount. At the end of five days more the opening closed entirely.

A new method of *operation for recto-vaginal fistula* is described by DR. SUSAN DIMOCK, of Boston (*N. Y. Medical Record*, vol. ix, p. 458). She separated the vaginal and rectal mucous membranes with the scalpel, obtaining thus a surface three-quarters of an inch in width without encroaching on the scant vaginal walls, and united first the rectal mucous membrane by five silk sutures tied in the rectum and allowed to slough away, and the vaginal mucous membrane by seven double wire sutures fastened on quills of French bougie. The cure of the large fistula, which after paring its edge was three inches in length, was complete.

I cannot refrain here from adding my share to the obituary of Dr. Dimock, published by Dr. Mary Putnam Jacobi in the *Medical Record* of May 22d. and from paying a well-merited tribute to her intelligence and medical attainments, and the uniformly amiable, unobtrusive, and womanly manner with which she pursued her studies, under the not always pleasant and often exceedingly trying circumstances which a woman, who chooses medicine as a profession, must necessarily encounter. Meeting her almost daily in the wards and at the gynecological operating table during the last five months of my stay in Vienna, I learned to admire her sterling qualities, and think it but just that I should add my name to those who sincerely regret her untimely death by the wreck of the ill-fated *Schiller*.

The application of galvanism in the form of *galvanic sutures*, for the purpose of inducing the union by first intention of poorly nourished and imperfectly organized surfaces, has been, I think, first made by PROF. PIPPINGSKÖLD, of Helsingfors. In Vol. III., No. 2, of the *Berlin Beiträge zur Geburtshülfe*, 1874, he reports two cases of vesico-vaginal fistulæ, which had resisted repeated previous attempts at an operative cure, and in which the surroundings of the fistula were composed chiefly of cicatricial tissue, the vitality of which is extremely low. These fistulæ he united, after paring the edges, by means of alternate sutures of silver-plated copper and of iron wire, with the object of stimulating the low vitality of the parts, and in both cases with perfect success. That the supposed galvanic action of these sutures is not purely imaginary, was demonstrated by his colleague, Prof. Hällstén, who found that when a silver and an iron wire suture were connected with the galvanometer, the maximum deflection of the needle of that instrument was 90°, the average deflection 35°; when, on the other hand, the galvanometer was connected with two iron or two silver-wire sutures, the maximum deflection was 15° to 20°, the average about 0°. This latter result would prove that every metallic suture, when bathed in the vaginal and vesical fluids, acquires a certain degree of galvanic power. Only a series of operations performed on cases which had previously withstood all attempts at cure by the ordinary suture would, of course, absolutely prove the value of this method.

*Tampons of tow soaked in coal-tar saponine* are recommended by DR. SIREDEY as very successful in the treatment of *vaginitis*. The tampon is removed after twenty-four hours, and reapplied on the third day, injections being used in the meanwhile.—(*Jour. de Méd. et de Chir. Prat.*)

DR. SAINT-GERMAIN uses medicated *sachets* in the treatment of *ulcerations of the cervix, uterine catarrh, and leucorrhœa*. They are made of gauze, five or six centimetres in length, somewhat larger than the thumb, and are filled with dry linseed-meal. These sachets are smeared with tanno-glycerine 1:8, mixed with belladonna 1:12, if there be pain, and having been introduced into the vagina, are left there for three or four days.

In profuse leucorrhœa without lesions he prescribes simply sulphurous baths and copious injections of the bath water.—(*Gaz. delle Clin. and Gaz. Med. Ital. Prov. Venet.*, No. 2, 1874.)

A very simple, ingenious, and efficient substitute for the common astringent vaginal injections usually prescribed in cases of *endocervicitis*, *erosion of the cervix* and *leucorrhœa*, and the inefficiency of which is well known, is proposed by DR. WILLIAM GOODELL, of Philadelphia (*Some Practical Hints for the Treatment and Prevention of Uterine Disorders*, 1874). He recommends that the tannin or other dry astringent powder be projected by the woman herself upon the cervix, through the nozzle of one of the familiar tin roach-powder syringes; or else, after the plan of Dr. E. L. Duer, of Philadelphia, a teaspoonful of glycerine containing five grains or more of tannin, acetate of lead, sulphate of zinc or alum, may be poured into a hollow pressed by the thumb into the centre of a thin sheet of cotton wool, not quite so broad as one's palm. The edges being now gathered up and securely tied, there will be found a small and dry tampon, which the woman, after getting into bed, can herself push up against the cervix. This tampon can be removed by a string attached and left hanging outside of the vulva. The advantage of applying agents to the cervix and vagina in this manner, and also in the form of vaginal suppositories, is obvious. The physician is thereby relieved from the tedious duty of daily making the simple local application himself, and the patient derives more benefit from the long contact of the remedy with the diseased surface than is ever the case with injections. I have frequently employed the substitutes mentioned above, and have found the "roach-powder syringe" very useful, as also a tampon smeared with lard and sprinkled with the astringent powder, which is easily introduced every day by the woman herself.

*Air-cysts in the vaginal mucous membrane* have been observed by PROF. KARL SCHROEDER (*Deutsches Arch. f. kl. Med.*, XIII., 4), who, while examining a woman in the ninth month of pregnancy, noticed the exquisite granular, nodular condition of the vaginal mucous membrane, which exuded a thick, purulent secretion. No syphilis. In the speculum the greater portion of the vaginal wall was covered by broad, closely-arranged protuberances (compared by Winckel, who found fluid in these cysts, to the appearance of an ear of corn), which generally appear bright-red, but frequently pellucid, evidently cystic in character. On puncturing these cysts, which are of the size of a pea, a crackling sound is heard, and the walls collapse completely, without the discharge of a drop of fluid. A microscopical examination by Zenker showed that these cysts are lined by tessellated epithelium cells of unequal size, among which appear quite large spherical cells with finely granular protoplasm and glistening nuclei. These air-cysts doubtless arise in the course of the hyperæmia and hypertrophy of the chronically inflamed mucous membrane of the vagina during pregnancy. The possibility of their developing during any long continued inflammation of the vaginal mucous membrane outside of pregnancy, as in chronic leucorrhœa and granular vaginitis, leads me to mention the subject here. The mode of development of these cysts is still doubtful. Schroeder believes that they may originate in the mucous follicles described by some anatomists, as Luschka and Henle, and that the clear fluid observed in them by Winckel undergoes a gaseous transformation. According to VOX PREUSCHEN (*Centralbl.*, 40, 1874), these cysts are retention-cysts, developed by the closure of the efferent ducts of the glands, which in some vaginæ were found by Preuschen in large numbers, in others less numerous, and in most vaginæ are exceedingly scarce. Some cysts are found in the sub- or para-vaginal tissue, and project into the canal; these are not vaginal cysts proper, and differ from those just described.

In a paper on "*Arrest of Development of the Uterus, Vagina, and Vestibule*" (*Wien. Med. Woch.*, 1874, Nos. 36 and 37), PROF. KARL BRAUN, of Vienna, states that absence of the uterus is generally accompanied by absence of the whole vagina: in 11 such cases observed by Braun, 3 showed urinary fistulae caused by impetuous coition. Deformities of the uterus are congenital or acquired, the latter especially after confinement at too young an age. The former, known as uterus duplex and unicornis, originate in abnormal changes of the ducts of Müller during embryonic life. Uterus bicornis, in the extreme degree of which the double cervix projects into the single or double vagina, is very rare, and distinguished from the uterus didelphys in non-vital children by its two halves appearing to be united externally in at least their lower portions. The variety termed uterus bicornis unicollis generally does not interfere with pregnancy or labor, although occasionally transverse positions may be habitual in such cases. In simple uterus septus without divergent horns, conception, pregnancy, and labor are possible, and generally normal; hæmatometra occurs in atresia of the internal os, under favorable circumstances; also hæmatocolpos. To facilitate the diagnosis, the method of high rectal exploration, according to Simon, is advisable.

The *rapid and forcible dilatation of the cervical canal of the non-pregnant uterus* in preference to the slow and not always innocuous expansion with sponge-tents and laminaria, has been much practised and recommended within the last two years, especially during the year 1874. A number of new dilators have been invented, of which Ellinger's, of Stuttgart; Palmer's, of Cincinnati; Miller's, of New York; Vanderveer's, of Brooklyn; and Ellwood Wilson's, of Philadelphia, are constructed of steel, with two diverging branches worked by a screw in the handle (Ellinger's act only by manual compression); and Molesworth's, of New York, of rubber surrounding a metal conductor, the expansion being produced by pressing air or water into the rubber tube by means of a piston.

At a meeting of the N. Y. Obstetrical Society, held June 2d, 1874, I exhibited the dilator of Ellinger, and stated that I had used it repeatedly in over 30 patients, suffering principally from antelexions, also from retroflexions, or simple constriction of the cervical canal, with or without dysmenorrhœa, endometritis with profuse catarrhal secretion, and in two cases of miscarriage, where it was necessary to dilate the cervical canal sufficiently to introduce the finger and curette, and remove remnants of placenta. I also found it very useful in dilating the cervix before and while introducing the cotton-wrapped applicator, to prevent the fluid saturating the cotton from being rubbed off. It thus serves as a substitute for Wylie's cervical speculum, a very useful and valuable instrument for this manipulation. I found the benefit from the forcible dilatation to be often temporary, it is true, but almost invariable, the distressing symptoms accompanying the above affections being usually relieved for some time after the operation. I have since used the instrument in an additional number of cases, always with good results. In one case of severe dysmenorrhœa from acute antelexion, the first painless menstruation which the patient had had for 11 years came on the day after the dilatation, and was perfectly normal in character; this patient has since menstruated twice without appreciable pain. Palmer's and Miller's dilators are superior to Ellinger's, because the screw action admits of more equal and gradual dilatation.

ELLWOOD WILSON, of Philadelphia, has used his dilators, of which there are four sizes, for several years, with great success, and told me, only a few weeks ago, that he now never employs the sponge-tent or laminaria, finding that he could procure all the dilatation required with his dilators. Dr.

Wm. Goodell, of the same city, agreed with Dr. Wilson on this point, and said that he had cured several cases of dysmenorrhœa, antelexion, and retroflexion.—(*Trans. Phil. Obst. Soc., Am. J. Obst., Aug., 1874.*)

At the meeting of the N. Y. Obst. Society above referred to, Dr. BYRNE said that he uses Peaslee's steel dilating bougies with great satisfaction, completing the dilatation with Molesworth's rubber instrument, which, however, is of no use in the primary dilatation of very narrow cervixes.

A novel method of curing retroflexion is reported by Dr. ELLINGER (*Note, p. 489, Am. J. Obst., Vol. VII.*). He replaces the retroflexed uterus with his dilator at the house of the patient, keeping her in bed for 3–8 days thereafter. The reposition and dilatation produces a slight degree of parametritis, which retains the uterus in the erect position, where it is also supported for a few weeks by a Hodge pessary. I confess to entertaining serious doubts as to the justifiability of intentionally causing pelvic cellulitis to cure a deformity, ordinarily so amenable to mechanical treatment by pessaries, as retroflexion; also, whether subsequent contraction of the exudation may not displace the uterus again more than ever.

In an article on "*Laceration of the cervix uteri as a frequent and unrecognized cause of disease*" (published in the *Am. Jour. Obst. for Nov., 1874*), Dr. THOMAS ADDIS EMMET again calls attention to the importance of this accident, and the necessity of recognizing and remedying it before the uterus can be expected in such cases to become amenable to treatment, and regain its normal condition. He states this accident to be exceedingly common (in which opinion Drs. Sims and Hanks concurred, the latter mentioning, during the discussion following the reading of Dr. Emmet's paper at the County Med. Society, that out of 229 strictly uterine cases seen by him at the Demilt Dispensary during the five months ending Aug. 31st, 1874, there were 19 lacerations of the cervix, or over 8 $\frac{1}{2}$  per cent., 16 of which cases suffered directly from the effects of this injury), and to be still little understood, notwithstanding that, during the 12 years which have elapsed since he first recognized this lesion as a cause of uterine disease, he has repeatedly drawn the attention of the profession to the necessity for surgical interference in these cases. Subinvolution, eversion of the lips of the cervix, eversion and ulceration of the cervical mucous membrane of the everted portion (indeed, many a case of this kind has doubtless been, and still may be, by some taken for nothing more than a large ulceration of the cervix, and is treated accordingly, of course in vain), metro- and menorrhagia, endocervicitis and endometritis, ovarian neuralgia and inflammation, hyperplasia and displacements of the uterus—these with their local and constitutional symptoms are the chief consequences of a well-pronounced injury of this character. The operation is simple, although usually not easy of execution. The cervix being fully exposed in Sims' position, the edges of the laceration are freshened and united by wire sutures. Before proceeding to the operation, it is well to prepare the parts for a month or two beforehand, by hot vaginal injections, support of the uterus by pledgets of cotton soaked in glycerine, or the rubber air-pessary, and the application of tannin or subsulphate of iron to the lacerated surface. After the operation the parts, if assisted by a suitable pessary and by general tonic treatment, will usually in a short time recover their normal tone and condition, almost without active local treatment. Although, as stated above, this lesion was recognized and the operation for its cure and the relief of its consequences devised as long ago as 1862, its importance was never before described and published in such detail by Dr. Emmet, he having only demonstrated it at the Woman's Hospital, where and in private practice he has performed the operation over 200 times. I consider it to be one of the greatest achievements, perhaps the greatest which has been brought pro-



minently before the profession in the field of uterine surgery during the year 1874.

In an article on "*The Philosophy of Uterine Disease, with the Treatment applicable to Displacements and Flexures*" (*New York Med. Journal*, July, 1874), Dr. EMMET states, as the result of his experience, that local congestion and inflammation are the causes of the most common forms of uterine disease, producing hypertrophy, hyperæsthesia, versions, and flexures. As he removed the congestion the hyperæsthesia disappeared, the flexures lessened in degree, and there was, with less hypertrophy, a great improvement in the versions. This result having been attained, he found that there was a stage in almost every case where mechanical support was indispensable. Further on he expresses his conviction that a flexure of the body of the uterus cannot be relieved by a lateral division of the cervix, even if it be extended to the vaginal junction. On the other hand, in flexure at the vaginal junction, the division of the posterior lip backward in the median line is a useful, safe, and justifiable operation, preferable in every sense to the lateral incision. It has been performed by Dr. Emmet sixty times in the Woman's Hospital (up to 1872), and forty-nine times in his private hospital, and was followed in four cases only by cellulitis with recovery, and in two cases by general peritonitis (from imprudence of the patient) and death. The stem-pessary is characterized by Dr. Emmet as "the device of the Evil One himself." Each case of displacement requires a pessary especially fitted to the peculiar conformation of the soft parts and the degree of displacement. Dr. E. is "ignorant of any instrumental means, safe or reliable, for correcting the position of an anteverted uterus." The success of a physician in treating displacements depends entirely on his capability of appreciating the individual peculiarities of each case, and his possession of the mechanical skill necessary to adapt the mechanical means used to these peculiarities. As a rule, if the patient is physically conscious that she is wearing a pessary, it either does not fit or she is not in condition to wear one; it therefore does harm, and should be removed at once. The copious hot vaginal douche, administered in the recumbent posture, is considered by Dr. Emmet to be an indispensable and invaluable remedy in congestion and hypertrophy of the uterus. Contrary to the opinion formerly entertained, hot water does not relax the vessels, but constricts them, although the first effect of heat may be to cause dilatation, but the water must be used hot, at least 98°, not tepid. These hot injections are infinitely preferable to cold-water injections, and without them the treatment of this class of uterine disorders would be incomparably more tedious and unsatisfactory even than it is at present.

At a meeting of the London Obstetrical Society, held October 7th, 1874, Dr. JOHN WILLIAMS read a paper "*On the Relation between Congestion of the Uterus and Flexion of the Organ*" (*Obst. J. Gr. Br. and Irel.*, Nov., 1874). Different, and even opposing, views are held on this subject. Some believe that congestion is the primary morbid condition of the uterus, and that flexion follows as its consequence; others maintain that flexion is the primary morbid state, and that congestion is brought about by it; others, again, assert that the two conditions bear no relation of cause and effect to each other, although they frequently exist at the same time. After discussing the various opinions, Dr. Williams summarizes his conclusions as follows: "There is no evidence to show that a physiologically increased flow of blood through the uterus occurring periodically, or that erections of the uterus favor or cause chronic congestion of the organ. Exposure to cold during a menstrual period is not a common cause of congestion of the uterus. Simple congestion is a rare affection of the virgin uterus. Flexion, or flexion accompanied by congestion, is not an uncom-



mon affection of the organ in its virgin state. The effects of congestion on the uterus are, at first sight, enlargement through distention of its vessels, then slight softening from exudation into its tissue, and lastly, enlargement of the organ and induration of its tissue. The increase in weight of the body of the virgin uterus, arising directly from congestion, is probably equal to about the weight of two drachms of blood. The effects of congestion on the uterus are such that it is not possible for such a small force as the weight of two drachms of blood to produce flexion of the organ. The condition of the uterus from the time of impregnation to the fourth month of gestation militates strongly against the view that congestion is a cause of flexion. The effects of flexion on the uterus are occlusion of its canal, leading to dilatation of its cavity and congestion and thickening of its walls; just as obstruction to the exit of material from all hollow muscular organs causes dilatation and hypertrophy of those organs. The increased flow of blood through the flexed uterus just before menstruation does not diminish, but increases, the flexion. Simple flexion of the uterus gives rise to congestion and hypertrophy of the cervix by compressing the venous plexus around the insertion of the vagina into the uterus. In retroflexion the body of the uterus and the veins of the broad ligament may be grasped by the sacro-uterine ligament, and thus become greatly congested.

DR. TILT and DR. BARNES agreed with Dr. Williams, that in many cases the flexion preceded and was the cause of the congestion, but believed that there were probably as many others in which the flexion was caused by the congestion, and removed by the cure of the latter. Dr. Tilt thought that to accept flexion as the common cause of uterine congestion was to make it the keystone of uterine pathology, and to establish a doctrine dangerous to sound practice and treatment; that it were better to remove the other morbid conditions of the womb before resorting to mechanical means to remedy the flexion.

DR. BRAXTON HICKS had also seen congestion as a product of flexion; especially when this relation apparent when flexion was present and congestion accidentally occurred; the latter was then kept up and aggravated by the flexion. DR. PLAYFAIR agreed with the above gentlemen, and said that simple, uncomplicated flexion often existed for years (as in cases where it is the cause of sterility) unknown to or without causing any inconvenience to the patient.

A valuable contribution to the *etiological statistics of cancer of the uterus* is furnished by DR. LOTHAR MEYER, of Berlin (*Berlin Beit. z. Geb. u. Gyn.*, III. 1). He has examined the records and clinical material of the Berlin City Hospital as regards the nature of the exciting causes of the primarily always local cancerous degeneration, whether they are local or general, external or constitutional, and arrives at the following conclusions:—

*Age.*—Cancer of the uterus hardly ever appears before the twentieth year, most frequently between forty and fifty.

*Hereditary disposition.*—1. The hereditary nature of the disease is not always confined to the same organ; it frequently skips generations. 2. In families with a hereditary tendency the oldest and strongest children are oftenest attacked. 3. The hereditary quality increases with each generation. 4. The disease is more frequently transmitted by the mother. All these assertions are, however, difficult of proof, and should be accepted with proper caution.

Whether and what kind of *local irritation* produces cancer, whether *excessive sexual indulgence* and *menstrual disorders* predispose to the disease, are still doubtful points; at all events, prostitutes are not known to be attacked by cancer more frequently than other women.

The influence of *childbearing* is generally acknowledged; it being found that cancer is more common in fruitful than in barren women; that the disease frequently develops itself soon after confinement; that the fertility of women with cancer of the uterus is above the average; and that sterile married women are attacked in about the same proportion as single women.

Among the *local diseases*, inflammatory processes, especially papillary proliferations and catarrh, exert an undoubted predisposing influence.

As regards *constitutional tendency*, Meyer found that brunette women, with so-called bilious temperament, are by far more liable to cancer; also those living in cities.

*The local application of the liquor ferri perchloridi in cancerous ulceration of the uterus* is of little use when the course of the cancer is rapid, but when the disease is purely epithelial, or chronic, or rodent in character, it relieves or sometimes seems to cure bad cases. Its application rarely causes pain. All discharges should be washed away from the cancer, and the breech of the patient raised to prevent any overflow of the solution over the vulva. Cotton wool, saturated with the chloride, is then applied, and any superabundant solution which a slight pressure of the wool causes to flow out is to be sucked up by a sponge from the bottom of the vagina. The wool is to be retained in its place by a loose plug of tow in the vagina, and the vulva oiled before the patient rises from the couch.—(*Lancet*, Dec. 12, 1874.)

The local use of chloral in suppositories of fifteen grains each, introduced into the vagina, is said by DR. C. PAUL, of Paris, to modify and correct the fetor of the discharges of *cancer of the uterus*, and to produce sleep where morphine had failed. Lint dipped in a solution of chloral and applied to the cancer appeared to cleanse the ulcerating surface, and greatly relieve the local distress.

HENNEBERG (*Allg. Wien. Med. Ztg.*, Oct., 1874) recommends the application of the alcoholic solution of bromine (1:5) to *cancer of the cervix uteri*, and also its injection into the tissues. Cancerous tissue placed in the bromine solution for forty-eight hours was found on removal to consist only of traces of connective tissue, with spindle cells; in one case the cancerous mass disappeared almost entirely.

In the *American Journal of Obstetrics* for February, 1875, PROF. HILDEBRANDT, of Königsberg, Prussia, again discusses the subject of the *treatment of uterine fibroids by the hypodermic injection of ergotine*, and publishes a new series of observations with the method originally devised by him. This paper was contributed already in August, 1874, and therefore properly belongs in this Report. He enumerates 6 new cases in which the symptoms produced by the tumor were removed, and the uterus was reduced in size; 11 in which the symptoms were greatly improved, but no diminution in size of the uterus took place; and 2 which were in no manner influenced by the treatment. The injecting fluid used by him consists of R Extr. aq. secal. cornut. (a preparation similar to the semi-solid aqueous extract of ergot of Squibb), partes 3; glycerini, partes 4; aquæ, partes 12. M. Generally a whole syringe-ful, containing about 44 drops of the above solution (equal to  $2\frac{1}{2}$  grains of the extract of ergot), is injected into the abdominal integument near the umbilicus, the point of the needle being plunged perpendicularly to the depth of one-half to one inch into the fold of skin pinched up between the fingers. Thus administered, he himself has never seen cellulitis or suppuration follow the injection, which he has performed in this manner at least 1,000 times.

This treatment is most likely to be attended by favorable results:

1. Where the tumor is richly provided with muscular tissue and possesses the consistence and feel of a tense elastic cyst.

2. When the tumor is submucous.

3. When the walls of the uterus are sound, capable of vigorous contraction, not too much attenuated by dilatation, or stiffened by exudation in their substance, and when there is no pelvic peritonitis or cellulitis.

4. As soon as the pelvic cellulitis and chronic metritis, which frequently accompany fibroid tumors, have been removed by proper preparatory treatment, when the previously mentioned conditions again come into force.

5. When the tumor is unprovided with a capsule and merges directly without a boundary into the peculiar tissue of the uterus, which anatomical relation of uterine fibroids may be considered most favorable to their complete cure by absorption.

DR. THEOPHILUS PARVIN (*Am. Pract.*, May, 1874) records 3 cases of uterine fibroid in which marked benefit followed the hypodermic injection of ergotine. DR. REEVES JACKSON (*Chic. Med. J.*, June, 1874) reports 5 cases of uterine fibroids, in 3 of which he had very favorable results from the same treatment; he uses a solution composed of 50 grains of Squibb's extract in 250 minims of water, with 50 minims more added to the filtered fluid, of which each minim contains one grain of ergot; he injects into the deltoid region in preference to the abdomen, and finds no pain, inflammation, or suppuration follow the injection. Five additional cases of decided benefit from the hypodermic use of ergot are reported by Drs. Ethridge, Merriman, and Fisher, of Chicago. Others are published by Emmons, Atthill, Scanzoni, and other physicians, sufficient in number to place the great value of the method beyond all doubt, and to show that its application is not so severe as to exceed the powers of endurance of the majority of patients afflicted with, and anxious to be relieved of, such an incurable and distressing disease as fibroid of the uterus.

*Fibro-Cystic Tumors of the Uterus.*—DR. OSWALD HEER, of Zürich (*Inaug. Diss.*, Zürich, 1874), gives a very thorough résumé of the history, anatomical relations, etiology, symptoms, diagnosis, prognosis, and treatment of these growths, which have of late become objects of greater interest than formerly on account of a number of successful operations for their removal by gastrotomy. The history of these tumors begins with a case reported by John Hunter in 1793, and concludes with the dissertation of Oscar Schröder, in 1873; 70 cases have been collected during this period.

*Anatomical relations.*—Fibro-cysts, as a rule, spring from the external layer of the uterine muscular tissue, either with a pedicle or a broad base, generally from the fundus or posterior wall (31 times in 62 cases). The pediculated tumors exert but a slight influence on the size of the uterus; those with a broad base, however, cause enlargement, hypertrophy of the lips, considerable dilatation of the os and elongation of the cavity, and may attain a weight of eighty pounds. They are composed partly of fluid, partly of solid substance, the latter of which generally forms the bulk of the growth or incloses the fluid as in a sac, or divides the cavities into compartments. In some tumors the solid matter predominates, in others the cysts. The latter may communicate; their shape varies; their internal surface shows projections and striæ similar to the cardiac trabeculæ; it is smooth, reddish, like mucous membrane, but devoid of epithelium. Adhesions between cystic tumors and adjacent parts are frequent, especially if the former are voluminous. Pediculated tumors contain one large cavity, and their walls may become so thin as to threaten to burst. The size of the pedicle varies from that of a thumb to that of three fingers. The interstitial, sessile tumors are multilocular, with many small cavities, and consist principally of solid tissue; their contents are brownish, sanguineous, or bright yellow, and in many cases coagulate rapidly when exposed to the

air. The cystic fluid contains principally albumen, blood corpuscles, fat globules. Koeberlé found cholestearine, Routh, uric-acid crystals, also pus, in it.

*Etiology.*—A hereditary tendency is not apparent. Uterine fibro-cysts occur principally between the ages of thirty and fifty years, at the time of the greatest sexual activity; married women are more disposed to them than unmarried females; of 44 cases 31 were in married women. A tendency to fibroid tumors is a predisposing agent; a connection between fibro-cysts and sarcomatous tumors is not improbable. According to Frankenhäuser, anæmic, poorly nourished women are more disposed to fibroid tumors than strong, well nourished females. Fibro-cysts are not very uncommon; compared with ovarian tumors their frequency is 1.83 per cent.; among 36 fibroids they were found twice. According to Péan and Urdy subperitoneal fibro-cystic tumors are more common than interstitial.

*Symptoms.*—These are similar to those of ovarian tumors, when the contents of the fibro-cysts are chiefly serous; so those of fibroids when the solid character predominates. The growth of the albumen first attracts the attention of the patient; sensations of tension and pressure, and pain particularly during menstruation, œdema of the lower extremities if the tumors are very large, occasionally hydrorrhœa, are symptoms generally observed. Active peritonitis rarely occurs, although extensive adhesions are not unfrequent. The menstrual flow is profuse only in sessile tumors; at times it ceases entirely for a greater or lesser period. Some patients pretend to notice a periodical enlargement of the tumor. Vascular murmurs are not regularly audible. Large tumors interfere with the functions of the bladder and rectum. Sterility is rare, 5 times in 31 cases, but miscarriages are common. If a pregnancy should go to term, fibro-cystic tumors of the uterus may interfere materially with the delivery of the child; in 4 cases the mothers and children both died in consequence of the dystocia. Cystic fibroids grow very slowly during the first years of their existence; later they increase very rapidly, and may attain enormous dimensions. Their duration is from two to ten years.

The *Diagnosis* is difficult and uncertain. Koeberlé and Péan twice made a positive diagnosis: in 3 cases they were uncertain, in 18 cases the diagnosis was incorrect, and, instead of fibro-cysts, ovarian cysts and fibroids; of the latter four were discovered. Frankenhäuser was more successful, and made a correct diagnosis in 4 cases out of 5; so also Sims, Beatty, and Atlee by tapping the tumor, and examining the fluid removed, which in ovarian cysts is thick and glutinous, in fibro-cysts of a watery nature. The puncture of fibro-cysts is frequently, however, attended with dangerous consequences, and at times elicits nothing but a drop of blood. Small tumors are usually recognized most readily, by means of combined and rectal examination. The diagnosis generally wavers between myoma or cysto-myoma, more rarely between these growths and a tumor adherent to the uterus or an ovarian tumor imparted in Douglas's cul-de-sac. If an ovarian cyst is adherent to the uterus by a large surface it will be found situated to one side of the uterus; if it is not adherent it glides into Douglas's pouch and is palpable per vaginam as an elastic, occasionally grape-like, tumor, which does not change the configuration of the intravaginal portion of the cervix, but may displace the womb. Fibro-cystic tumors are hard, nodular at the base, and fluctuate more or less towards the apex; ovarian cysts are uniformly elastic. Fibroids frequently accompany fibro-cysts. *Dermoid cysts* are more doughy than fibro-cysts, because they consist of adipose tissue. Large pediculated fibro-cystic tumors are distinguishable from ovarian tumors by the firm tissue near the pedicle,



the cystic wall above being very thin. If the fibro-cysts sprang from interstitial myoid tumors the uterus itself is found to be hypertrophic. The fibro-cyst is longitudinal, with a uniformly rounded contour; the ovarian tumor is broad and, if multilocular, nodular. Mistakes are possible with sarcoma, hæmato- and hydrometra, intra- and extra-uterine pregnancy, hæmatoma of the broad ligaments, echinococci, pelvic cellulitis, renal and tubal dropsy.

*Prognosis and Treatment.*—Only rapidly increasing fibro-cystic tumors of the uterus threaten life; the prognosis is, therefore, more favorable than that of ovarian tumors. Tapping of the tumor, however, is detrimental, because the unyielding walls do not collapse, air enters, the contents decompose, and septicæmia readily occurs. Gastrotomy is by no means always fatal; the removal of pediculated tumors is especially favorable. Before 1869 there were 20 fibro-cysts removed by gastrotomy, of which 6 terminated favorably; since 1869 there were 7 recoveries out of 13 operations. The transection and separate ligature of the various portions of the pedicle appears preferable to the clamp. When it becomes necessary to remove the internal genital organs, as is the case in sessile tumors, it is advisable to secure the broad ligaments with the ovaries by two ligatures.

In an article on "*The Diagnosis of Cystic Myomata of the Uterus and their Intraperitoneal Enucleation; A New Method of Operation*" (*Arch. f. Gyn.*, VI., 3, 1874.), PROF. SPIEGELBERG, of Breslau, describes a case of supposed ovariectomy, in which the ovaries were found normal, but the uterus enlarged to a point 12 cm. above the umbilicus by a large fibro-cystic tumor, which was drawn out, ligated and removed with the knife. The edges of the peritoneal envelope of the tumor (and uterus) were united by 18 deep silk sutures, which were carried out through the abdominal wound, and a drainage tube was passed from the latter through the recto-uterine pouch into the vagina. The patient nearly died from septicæmia on the second day, rallied, and died suddenly on the sixteenth day, probably from pulmonary embolism; the autopsy was not permitted. This is the first case in which the intraperitoneal suture of the wound left after the removal of a uterine tumor of such size has been employed. Spiegelberg suggests its use also in solid tumors of the uterus after their intra-abdominal enucleation and removal through an abdominal wound. Catgut would be preferable to silk for the sutures *perdues*. Spiegelberg avows himself an enthusiastic admirer of the system of vaginal drainage recommended and practised by Sims, and believes that this case, as well as the 8 cases of ovariectomy he has lost from septicæmia, would have been saved if proper drainage and disinfection of the abdominal cavity had been employed.

Much has been said, written, and especially done within the past five years on the subject of the *operative removal of large fibroid tumors of the uterus*. Indeed, this is one of the chapters in which a decided progress has been made during the last year.

There are two chief methods of removing uterine fibroids—that by *enucleation* through the *vias naturales*, when the tumor is submucous or intramural and its size does not present an obstacle, and that by *gastrotomy*, when the growth is subperitoneal or involves the uterine tissue to such an extent that even its extraction piecemeal through the pelvis is impracticable. The operation by *enucleation*, although at first much opposed on account of the dangers of hemorrhage and septicæmia attending it, has now, by the efforts of Danyan, Amussat, Maisonneuve, Bérard, Langenbeck, and more recently Duncan, Meadows, McClintock, Spiegelberg, and especially Atlee, Thomas, and Sims, become a perfectly well understood and frequently practised measure. In an article on "*The natural and artificial*



*elimination of sessile (intraparietal) uterine fibroids*" (*Vierteljahrsschr. f. d. prakt. Heilk.*, 1874), Dr. MÄNNEL, of Dresden, after relating a number of instances in which such tumors were enucleated and voluntarily expelled, "sloughed out," after accidental or intentional injury by the examining finger, instruments, or during confinement, gives a compilation of all the cases of intentional enucleation of fibroids of the body of the uterus, published in literature, numbering 49 in all, 32 of which terminated favorably, 17 fatally. Doubtless many others, principally with fatal issue, have never been published; indeed these cases are getting so common that the figures given could easily be enlarged by looking over the literature of the last year, and I have necessarily been obliged to omit many names in the list given above, as deserving of mention in connection with this operation. In an exceedingly interesting paper on "*Intrauterine Fibroids*" (*N. Y. Med. Jour.*, April, 1874), Dr. SIMS relates six cases of large interstitial uterine fibroids successfully removed by him, and two other cases of death after the operation, and vividly describes his method of enucleation, which is briefly as follows: Two main rules are, 1, the cervical canal must be freely open; 2, the tumor must be free from its capsule. The first requirement is obtained by sponge-tents and, if necessary, by crucial incision of the cervix. The presenting portion of the tumor having been brought into view in Sims's speculum, and seized with a strong hook, the capsule is opened with scissors at its attachment to the posterior and lateral wall of the cervix, the finger and then the enucleator (a blunt-pointed, rectangular hook) passed in and swept around up to the fundus, detaching the tumor from its capsule in all directions; at the same time a powerful double hook is hooked in further and further up, and strong traction made on the tumor, which is finally rolled out of its uterine bed and removed. The careful packing of the uterine cavity with iron-cotton, and the frequent cleansing of the cavity by carbolyzed warm-water injections, are essential precautions to prevent the two great dangers—hemorrhage and septic infection.

*The removal of a large uterine fibroid by gastrotomy* was first practised by Clay, of Manchester, in 1843, who, as is usually the case, was obliged to remove a portion of the uterus with it; the patient recovered. No one had the boldness to follow Clay's example, notwithstanding the favorable result, until Storer some ten years ago repeated the operation, also successfully. Still, although occasional successful cases were reported, by Fletcher in England, Burnham, Kimball, Boyd, and Darby in this country, no one operator had ventured to repeat the operation a number of times, or had endeavored to compare it with ovariectomy as regards success. Indeed, the general opinion was that the operation was unjustifiable, because too rarely followed by recovery. The profession were, therefore, surprised and startled by the appearance, towards the close of 1873, of an essay on this subject by J. PÉAN and L. URDY, of Paris, in which nine cases of removal of uterine fibroids by gastrotomy were reported, seven of which recovered. To these are to be added 4 cases of recovery out of 6 operations performed by Koeberlé of Strasburg, a total of 15 operations and 11 recoveries—that is 73 $\frac{1}{3}$ %. According to Péan and Urdy, this operation has since Clay been performed in England 11 times, 2 cured, 18%; in America 11 times, 4 cured, 36%; in France 18 times, 11 cured, 61%. These successes have induced Sims, as stated in the paper quoted above, to modify the unfavorable views he had hitherto entertained on this subject. Two operations of this kind performed by Drs. Thomas and Sims during the past year in this city terminated fatally, both patients dying rapidly from shock. At a meeting of the Royal Med. and Surg. Society, Oct. 27th, 1874 (*Obst. J. Gr. Br. and I.*, Dec., 1874), Mr. LAWSON TAIT reported the successful removal of a large fibro-myoma of the uterus by gastrotomy; the tumor weighed 11 pounds,

and the whole uterus above the internal os was removed with it; the clamp came away on the eighth day. During the ensuing discussion, Mr. Spencer Wells stated that he recollected only three cases in which this operation was successful; in one case he removed a tumor weighing 20 lbs., because it appeared to be killing the patient, who is now well. As a rule, this operation should never be attempted unless the life of the patient is endangered by bleeding or by pressure on the intestines or uterus. The mortality will always be more serious than from ovariectomy. In answer to an objection by Mr. Holmes, that the solid nature of large uterine fibroids tends to the formation of adhesions, and thus increases the risk of the operation, Mr. Tait said that in all the cases where he had seen the abdomen opened before and after death in the presence of a large uterine tumor, he had never seen an adhesion, whilst in ovarian tumors their absence is exceptional. This is certainly an important point in favor of this operation.

A case of *successful amputation of an inverted uterus*, eight years after the accident, is reported by Dr. SINCLAIR, of Dublin (*Dubl. Path. Soc.*, April 11th, 1874. *Obst. J. Gr. Br. & L.*, October, 1874). Taxis was ineffectual. A strong whipcord was passed around the neck of the tumor with Gooch's double canula, and tightened as much as possible, this operation of tightening being repeated morning and evening for four days, when the uterus was in a state of decomposition, and was removed with the uterine scissors, a short distance below the ligature. In three days the ligature came away, in twelve days the patient sat up, and in two months she was discharged in perfect health. On examining the amputated uterus, it was found that the inverted peritoneal cavity of the organ was almost obliterated by strong adhesions, and nearly all traces of the appendages had disappeared. The danger of using much force in attempting to reduce these very old inversions is thus apparent; the presence of the firm adhesions will prevent the reduction, and their laceration will produce peritonitis.

Dr. A. GUERIN, in *Le Mouvement Medical*, says: "There is no physiological condition so nearly resembling disease as that which produces every month in an adult woman a change so profound, that it has been looked upon as the expression of a morbid condition." This sentence induces me to refer prematurely to a startling and remarkable theory advanced by Dr. A. F. A. King, of Washington, professor in the University of Vermont, to the effect that the generally accepted physiological function of menstruation is really a pathological condition, which in Biblical, almost prehistoric times did not exist, but has gradually become a fixed habit of the female sex in consequence of the vitiating influence of civilization. Dr. King bases on this view a new theory of uterine pathology, and defends his opinions by arguments, logical and historical, in an article to appear in the August number of the *Am. J. Obst.*

In an article by Dr. PRADIER, on the *employment of baths and medicinal agents during the menstrual period* (published in the *Tribune Médicale*, 33, 1874), the author arrives at the following conclusions: 1. According to several authorities, some strong medicines, such as bleeding, emetics and purgatives, may be administered without inconvenience during menstruation. 2. Hydropathic treatment and sea-baths have been given under the same circumstances for a long time without evil effects. 3. Several physicians do not hesitate to prescribe tepid baths in the same condition. 4. As to mineral baths, one of the author's friends has permitted them in like circumstances for seven years past, without having seen the least accident occur.

Dr. MARY P. JACOBI (*Med. Record*, Jan. 2) makes the following contribution to the exhibition of *Belladonna* and *Nitrite of Amyl* in *Dysmenorrhœa*: By clinical observation and physiological experiments she shows that clonic and tonic contractions of the uterus may be produced by anæmia

of that organ. In spasmodic dysmenorrhea we have no other cause but changes in circulation in the uterine walls to account for the contractions. Believing that the pain of dysmenorrhœa is due to the spasmodic contraction of blood-vessels, resulting from irritation of vaso-motor nerves, the use of belladonna and nitrite of amyl are suggested.

To get the secondary or dilatory effect of belladonna it is given in ordinary doses for several days previous to the menstrual flow. When the pain comes on, nitrite of amyl is administered by inhalation in from two to six drop doses. In three cases spasmodic dysmenorrhœa was speedily relieved by the above treatment.

DR. DANLOS, in a thesis published in Paris during the past year, discusses the subject of the *influence of menstruation on skin diseases*, and arrives at the following conclusions: There exists a manifest sympathy between the utero-ovarian sphere and the integument. This sympathy shows itself either by eruptions coincident with menstruation or the development of uterine disease, or by the disappearance of a pathological condition of the skin, when puberty arrives or disorders of menstruation cease. The urticaria-like mobility and hyperæsthetic character of these skin affections indicates their nervous nature, and justifies us in classing them with the reflex neuroses. The well-known close connection of the genital and nervous systems, and the dependence of derangement of the latter on disorders of the former, render this assumption extremely plausible, indeed almost certain.

A case in point was related at the November meeting of the *Leipzig Obst. Soc.*, by DR. GERHARD LEOPOLD, who saw first a violent hysterical paroxysm follow the application of three leeches to the cervix uteri, and then a severe general urticaria arise after eight leeches to the sacral region for intense sacralgia. The eruption was of the most violent character, the face became livid and hard, large blotches gradually spread from the lower extremities over the whole body, and the vascular and febrile excitement rose to an alarming degree, the pulse going up to 122 beats, the temperature up to  $40^{\circ}\text{C.} = 104^{\circ}\text{F.}$ , on the third day. After this the eruption gradually waned and disappeared without desquamation on the sixth day. The phenomenon of urticaria after leeches to the cervix uteri has already been witnessed and described by Scanzoni in 1860, and referred to subsequently in his works on Chronic Metritis and Diseases of the Female Sexual Organs. The exact nature of the connection between an irritation of the uterine nerves, such as is caused by the biting and sucking of the leeches, and the intense vascular and nervous excitement of the general integument, as witnessed by Scanzoni and Leopold, is still open to explanation. The name "reflex-neurosis," used by Danlos, expresses our ignorance on this point, as well as any other. This unpleasant accident, although neither dangerous nor very common, will still be an additional reason in favor of employing scarification of the cervix uteri in preference to leeches, the sucking of which, according to Schroeder (*Handb. d. Krankh. d. weibl. Geschlechtsorg. Ziemssen, Leipzig. 1874, p. 106*), increases the determination of blood to the uterus, instead of diminishing it. The difficulty of correctly estimating the amount of blood drawn by leeches, the inconvenience attending their use, and the repugnance of the patient to their employment, should besides effectually banish them from the region of the cervix uteri, in cases of chronic enlargement of the womb, where simple scarification with a bistoury would answer the purpose much better.

SLAVJANSKY (*Allg. Med. Centr. Z.*, 54, 1874), whose name is well known by his researches on the *physiology and pathology of the ovaries*, sums up the following results obtained by him:

1. The Graafian follicles develop themselves from the primordial follicles, and are growing towards maturity from the first month of birth to the fortieth year.

2. The larger number of follicles do not mature, do not rupture, do not discharge their contents, but pass over into a condition of atrophy, which is analogous to the formation of the corpora lutea.

3. The development and ripening of the Graafian follicle does not take place periodically in a regular manner, and there is no connection between it and menstruation.

4. Menstruation is a physiological phenomenon unconnected with the development and ripening of the Graafian follicle.

5. The rupture of the more or less ripe follicle is associated with congestion of the genital organs, and is, as yet, an unexplained matter.

6. Certain diseases, such as fevers, poisoning, etc., cause atrophy of the follicles, where there has previously been a parenchymatous inflammation of the ovary.

In a paper on "*The Structure of the Mucous Membrane of the Uterus and its Periodical Changes*" (*Royal Society, London; Obst. Jour. Great Britain and Ireland*, August, 1874), Dr. JOHN WILLIAMS reports his observations made on the uteri of nine women who had died in different stages of the monthly period. The deductions he draws therefrom are the following:

"Menstruation appears essentially to consist, not in a congestion, or a species of erection, but in growth and rapid decay of the mucous membrane. The menstrual discharge consists chiefly of blood, and of the débris of the mucous membrane of the body of the uterus. The source of the hemorrhage is the vessels of the body of the uterus. The mucous membrane having undergone fatty degeneration, blood becomes extravasated into its substance; then the membrane undergoes rapid disintegration, and is entirely carried away with the menstrual discharge. A new mucous membrane is then developed by proliferation of the inner layer of the uterine wall, the muscular tissue producing fusiform cells, and the groups of round cells inclosed in the meshes of the muscular bundles producing the columnar epithelium of the glands."

Dr. ENGELMANN, of St. Louis, differs from Dr. Williams in various points, and says very truly, that Dr. W.'s deductions are not always correct, because they are based on observations of uteri of women who died during some acute inflammatory disease, typhoid fever, pneumonia, etc., by which the uterine mucous membrane was more or less influenced. Engelmann, whose paper appeared in the *Amer. Journ. of Obstet.* for May of this year, believes, from his numerous and thorough observations, that the uterine mucous membrane merely "increases in thickness and succulence as the time of menstruation approaches, that this tumefaction is most marked during the period itself, and gradually decreases after the cessation of the catamenial discharge."

*Nidation* is the term used by Dr. J. H. AVELING (*Obstet. Journ. of Great Britain and Ireland*, July, 1874) to designate the periodical development of the mucous membrane lining the interior of the body of the uterus. The membrane thus developed he calls *nidal decidua*, instead of menstrual decidua, that being a misnomer, because it is formed independently of menstruation. The nidal decidua is developed in the intermenstrual period. Nidation probably commences with the reproductive life of women, and recurs with regularity, according to the periodic life of the individual, until that life ceases. During the week preceding menstruation, it appears to proceed more actively than at any other time. The *duration of the nidal period* varies in different women; it usually lasts three or four weeks,



and corresponds with the so-called intermenstrual period. *Nidation is dependent upon ovulation* for its being, for without an ovary there is no reproductive life, and therefore no nidation. Once established, however, nidation proceeds independently, as shown by its continuance after the destruction by disease or the removal of both ovaries. If the nidal decidua is still unprepared when the ovum is matured, or *vice versa*, the conception will be rendered abortive. It being certain that ova are discharged from the ovaries at irregular periods, and not, as hitherto believed, at or near the time of menstruation, it would be interesting to know at what period of nidation the uterus is in the most fit condition for impregnation; probably when the process of nidation is most active the uterus is best prepared for the reception of the ovum, but also least in condition for insemination, the swollen mucous membrane preventing the entrance of the seminal fluid. Impregnation would therefore be most likely to take place during the second week preceding menstruation.

*Nidation may occur independently of impregnation*, for Mondat discovered the nidal decidua in two sterile women who had never menstruated, and Courty removed it from the vagina of a girl whose hymen was perfect.

Nidation is frequently attended towards its end by a sympathetic swelling of the breasts, which disappears when denidation has taken place. During lactation, nidation is probably suspended, but is re-established when the breasts cease to furnish nourishment.

*Denidation.*—While nidation has been likened to gestation, denidation may be compared to parturition. When the nidal decidua has reached its full development, and no impregnated ovum arrives to demand from it protection and sustenance, it speedily becomes degenerated, and is thrown off, generally in minute shreds, sometimes as a triangular sac. This process is probably completed during menstruation, which act is determined by denidation, the menstrual flow coming from the denuded uterine surface left after denidation. Although the menstrual molimina may occur where there is no uterus, and consequently no nidation, still, as a rule, denidation and menstruation are generally contemporaneous, and menstruation must in most cases be controlled in its periodicity by nidation, to which latter function it is always secondary.

There is a variety of *disorders of nidation*, such as:—*Painful nidation*, occurring usually in hyperæmic conditions of the uterus, ten days to a week before menstruation. *Hypernidation*, excessive growth of the nidal decidua, occasionally foreshadowed by distinct symptoms, as in two cases of Scanzoni, and terminating in the expulsion of the unaltered nidal sac, not necessarily productive of sterility; *subnidation*, imperfect formation or absence of the nidal decidua, occurring in cases of disease or weakness, and entailing sterility; *abortive nidation* or *nidal abortion*, produced by morbid, mechanical, chemical, traumatic, and physiological influences, probably followed by too early recurrence of menstruation.

*Difficult denidation* is divided into two periods, the separative and the expulsive, and is better known under the name of membranous dysmenorrhœa (dysdenidation). The pain of this disorder and the difficulty experienced in curing it make it one of the stumbling-blocks of gynecologists.

Impregnation arrests denidation, the decidua continues to be developed in the well-known manner, until the full development or death of the fetus renders the nidal decidua no longer necessary, and *gravidal denidation*—the determining cause of labor—takes place.

Any operations on the uterine cavity should be avoided during the latter days of nidation, but depletion will be most effective at that period. Tents and stems should be inserted early in the nidal period.

I have been thus explicit in reviewing Dr. Aveling's article, because, as



you see, he differs from the views held by Dr. Engelmann and agrees with Dr. Williams, and because this exceedingly interesting subject of the exact nature of the hitherto always accepted *physiological* phenomenon of menstruation, and the relation between it and ovulation, is one which has attracted much attention during the past year, and bids fair to puzzle investigators for some time to come.

At the meeting of the *British Medical Association* last August, Dr. MATHEWS DUNCAN said, in the Obstetric Section, that he differed from the view entertained by most authors, that a *pin-hole os uteri* was a *frequent cause of dysmenorrhœa*. He had found the pin-hole os as rare as an acephalous fœtus. It was not a cause of mechanical dysmenorrhœa, because a woman might bleed to death through it. Mechanical dysmenorrhœa is a disease of a spasmodic character, and its symptoms are often fearful, such as are never produced by a pin-hole os uteri; the cervix may be amputated in a case of dysmenorrhœa with pin-hole os, and still the dysmenorrhœa continue.

*Catelectrotonus of the ovaries in a case of amenorrhœa* was induced by Dr. ALTHAUS (*Med. Times and Gaz.*), by placing the negative electrode of the constant battery alternately on the right and left ovarian region, putting the anode alternately to the lumbar spine and to the os uteri by means of an insulated sound. A current of from fifty to sixty cells of Daniell's battery was used for fifteen minutes at a time, giving the patient exactly the sensation—persistent uterine pain and right hemicrania—which she had always felt before the period came on. The current was used in all sixty-three times, with three intervals—one of four months, the other two of ten and twenty-two days respectively; after the fifty-fifth application the menses came on normally, and eight more sittings being taken, returned punctually and normally on the same day of the following month. The patient was then discharged.

A case of *successful transfusion of unprepared human blood for menorrhagia* is reported by M. BÉHIER (*Annales de Gynécologie*, Mai, 1874). There have been but few transfusions performed in gynecological practice, probably for no other reason than that the necessity does not so often present itself as in obstetrical cases. Still, there are doubtless many cases—such as anæmia from metrorrhagia in fibroid tumors, and metrorrhagia from various causes, prostration after ovariectomy and other major operations, etc.—in which this, in itself harmless, operation might be indicated and life-saving.

Dr. J. K. BLACK, of Newark, Ohio, strongly recommends the administration of *bromide of ammonium in non-structural catamenial excesses* (*Cin. Lancet and Obs.*, May, 1874), and says that he is not more certain of arresting an attack of ague by quinia than he is of controlling the forms of menorrhagia referred to by the bromide of ammonium. To be efficacious, however, it should be given at least ten days before the expected period, not alone during the flow itself; only when thus administered will its sedative influence on the ovarian and uterine vascular engorgement preceding menstruation make itself properly felt.

The *muriate of ammonia* is reported by Dr. R. D. WINSETT (*Nashville J. of Med. and Surg.*, Jan., 1874) to have cured a case of *menorrhagia and dysmenorrhœa* combined, in a woman forty-six years of age, the mother of four children, the youngest of whom was twenty years old. The menstrual flow would last from six to eight weeks incessantly, and had completely prostrated the patient. After almost the whole materia medica had been exhausted, the muriate of ammonia was tried and accomplished a cure, the menses now reappearing regularly every four weeks without pain.

DR. A. CORDES, of Geneva, reports a case of *irregular menstruation* (*Obst. J. Gr. Br. and Irel.*, April, 1874) in a girl fifteen years of age, who had menstruated every three weeks since the appearance of the catamenia a year previously, and who became perfectly regular, menstruating every four weeks, after the administration of *bromide of potassium* (grs. iv.) twice a day during the eight days preceding the expected period. This treatment was continued during six months with the same favorable result. As soon as the bromide was omitted, however, the menses again reappeared every three weeks, becoming regular again on the resumption of the drug.

*The mobility of tumors connected with the internal genital organs.*—DR. PAUL GRENSER relates a case (*Arch. f. Gyn.*, VII. 1) in which a large tumor connected with the genital organs, a fibroid of the right ovary, as he thinks, changed its position at certain times, usually shortly before and during the menstrual period. Before the flow the tumor presses against the right costal arch and causes severe symptoms of oppression; it is also pushed more to the left side of the abdomen, and its apex is 3 cm. nearer the ensiform process than usually; the heart and liver are dislocated upwards. As soon as menstruation commences the symptoms of oppression cease. The pulse falls from 112 to 90–80 beats, and in the course of ten or twelve days the tumor has sunk to its lowest point, where it does not remain, however, but in a few days again rises to about midway between the two positions. When the tumor occupies the middle portion of the abdomen, halfway between the ensiform process and the pelvic brim, the condition of the patient is endurable; as soon as it rises above this point, the symptoms of oppression already mentioned come on, and when it sinks down in the pelvis the signs of pelvic pressure and obstinate constipation predominate. Thus the patient enjoys a condition of comparative comfort only for 8 to 10 days out of each intermenstrual period. Grenser attributes this mobility of the tumor partly to the distention and collapse of the intestines, partly to a regular menstrual congestion of unusual intensity, which latter circumstance has been already referred to by Peaslee (*Ovarian Tumors*, 1872). All observations on the motion of ovarian tumors have hitherto had reference only to the axis-torsion of the pedicle.

At a meeting of the Strasburg Medical Society, May 7th, 1874 (*Ann. de Gyn.*, II., Dec., 1874), PROF. KOEBERLÉ discusses the subject of *torsion of the pedicle of the ovary*, just referred to. This accident, he says, happens gradually or suddenly: in the former case the symptoms are vague and indistinct: in the latter, on the other hand, the sudden intense pain, and the rapid development of an ovarian cyst, are diagnostic signs. The torsion is most liable to occur near the menstrual period, when the ovary is congested and heavy, and when some quick movement of the individual causes its pedicle to become twisted, producing sharp, constant pain in the iliac and renal regions, occasionally vomiting, and a sensation of numbness of the corresponding thigh. The degree of torsion varies: in the worst cases seen by Koeberlé, there were two, and even two and a half, turns. In one of these cases the pain was so intense and uncontrollable as to induce Koeberlé to remove the ovary by abdominal section, which revealed a tumor of nearly the size of a child's head. The cysts are usually unilocular, their contents are brownish, hemorrhagic, occasionally coagulable, in contradistinction to serous and albuminous cysts, the contents of which do not coagulate. In one instance the torsion was so complete as to entirely detach the ovary from its pedicle, but subsequent adhesions had attached it to the surrounding parts and permitted its further growth. Complete separation of the ovary from all attachments has not yet been observed. Koeberlé, at the same meeting, reports having seen four cases of torsion of fibrous tumors, and in the three last cases he succeeded in diagnosing the accident.

Prof. Schützenberger also, on the same occasion, mentions having seen a case of complete torsion of the uterus and vagina, following a rapid movement of the patient.

A third case of *normal ovariectomy*, and the second in this city, has been performed by DR. T. T. SABINE, at St. Luke's Hospital (*N. Y. Med. Jour.*, Jan., 1875). The patient, an unmarried girl, twenty-five years of age, had suffered for eight years from the most intense dysmenorrhœa and ovarian neuralgia, for which numerous operative and other measures had been tried in vain; the left leg (the left ovary was the one affected) was unable to bear the weight of the body, and the patient finally became so anxious for relief from the constantly returning, harassing ovarian, uterine, and pelvic pains as to earnestly desire the removal of the ovary, which operation was performed August 8th, 1874, in the usual manner. The patient recovered without difficulty; one month after the operation all ovarian pains had left her; she could walk easily, and the second menstruation since the operation was entirely normal and painless. The ovary was found to be perfectly normal.

The first operation of this kind was performed by Dr. Battey, of Georgia, several years ago; the second by Dr. T. G. Thomas, at the Woman's Hospital, in this city, about one and a half years ago (both ovaries being removed, and both having undergone the changes of interstitial inflammation and fatty degeneration); both operations were undertaken for pretty much the same symptoms as those enumerated above, and both were followed by success, not only as regards the recovery of the patient from the operation, but also touching her relief from the ovarian distress, which finally called for the removal of the ovary. The relief thus obtained would seem to justify the adoption of this fearful alternative of unsexing a woman whose ovaries are not vitally degenerated or unfit for their physiological functions, as is the case in cystic disease of the organ, even though as yet there may not be found many operators willing to take the responsibility, or able to overcome the scruples as to the justifiability of the operation still entertained by probably the majority of the profession.

DR. WHEELHOUSE, in the *Brit. Med. Jour.*, March 21, 1874, gives his views on *ovariotomy*, discussing only those points on which opinions still differ. He agrees with those operators who expect a better result from the operation when the peritoneum has already become toughened and thickened by chronic or intercurrent inflammation, because the membrane is then less likely to absorb deleterious substances, and the danger of pyæmia and septicæmia, as well as of traumatic peritonitis, is much diminished.

As to whether the cyst should be tapped before operating, he says that if it is a monocyst, yes, for the following reasons: 1. For the sake of diagnosis (also specially recommended by Spiegelberg); 2. Because a radical cure is possible; 3. Because, occasionally, the cyst fills very slowly. He relates a case in which only two tapplings were required in nineteen years, and three cases in which the cysts did not refill after the first paracentesis.

A polycyst, however, should never be tapped before operating, or if this is done, one must be prepared to perform ovariectomy at a moment's notice, because, in these cases, inflammation of the cyst is very liable to follow the puncture, and a rapid removal of the tumor is the only chance of saving the patient. (This view perfectly coincides with that expressed by Dr. Peaslee at a recent meeting of the New York Obstetrical Society.)

The incision should be rather too long than too short, for a wound of six inches heals as easily as one of three, and the advantages of having more room to detach and extract the cyst, arrest hemorrhage and cleanse the abdominal wound, are incalculable. The pedicle should not be treated after a fixed plan, but be either attached or dropped, ligated or seared, etc., in accordance with the requirements of each case.

The successful results gained after ovariectomy by Spencer Wells, Peaslee, Thomas, Kimball, and Sims, by the introduction of the *drainage tube* and the *washing out of the peritoneal cavity* through it, induced Nussbaum, in Munich, to adopt the same plan, and, like Spiegelberg already mentioned, he declares his unqualified support of the practice. He states, that out of seventy-eight cases of ovariectomy he has lost twenty-eight by septicæmia, and he feels confident that the majority of these would have been saved if he had used drainage. He has employed the drainage-tube and intraperitoneal injections in five cases, and goes so far as to say that a continuous stream of some mild antiseptic fluid may prove a necessity in certain cases, and that he has no fear of the influence of air and water on the peritoneum and the bowels (*Aertzl. Zeit. Bl. Memorab.*, 3, 1874). Now, all who have seen ovariectomies performed at the Woman's Hospital, in this city, are aware of the wide difference in the method of drainage, as originated and until lately practised by Sims, and that employed by Thomas and Peaslee. A simple curved glass or metal tube, introduced into the lower corner of the abdominal wound, and reaching only as deep as the bottom of Douglas's cul-de-sac, used only in case of need as a canal for the injection and return of antiseptic fluid, kept tightly corked in the interval, and removed after the first sixty to ninety-six hours, if not required longer (the method of Thomas and Peaslee), is quite another and certainly less severe procedure than the passage of an *open* rubber drainage-tube through the abdominal wound and roof of the vagina, which seems to me likely to produce the very inflammation and infection which it was the object of the distinguished inventor of this form of drainage to prevent.

At a meeting of this Association, held Nov. 6th, 1874, during the discussion of a paper, by DR. DAWSON, on "*Special Points in Spencer Wells' Ovariectomy Operations.*" DR. PEASLEE said that in every case of ovariectomy the abdominal cavity should be hermetically sealed; that, if a drainage-tube be used, all its connections in every particular should be air-tight; that every *open* tube introduced into the peritoneal cavity is an invitation to septicæmia. A closed tube does not possess the vital objection of admitting air to the peritoneal cavity.

At present, Drs. Thomas and Peaslee employ the drainage-tube, as described above (Dr. Thomas, of glass; Dr. Peaslee, of metal), only in cases where there is likely to be oozing from large adhesions; when once thought necessary, it is never removed before ninety-six hours, and then only when it is evident that it is no longer required. If there is oozing or intraperitoneal fluid of any kind, it is retained as long as such fluid continues to be formed. Dr. Peaslee makes intra-abdominal injections through the tube, when indicated; Dr. Thomas has entirely given them up since last fall, no matter how much intraperitoneal fluid there may be.

Recently Dr. Sims himself has greatly modified his views on this subject, for at the meeting of the American Medical Association, held at Detroit in June, 1874, he frankly stated that he had much to take back on the subject of ovariectomy: 1. As regards vagino-abdominal drainage, he was now satisfied that it is not at all necessary to puncture the vaginal cul-de-sac, that drainage from the lower end of the abdominal walls by a glass tube passed down behind the uterus, and resting in the lower angle of the external wound, after the manner of Thomas and Peaslee, is just as efficient, and that by the latter method the danger of leaving a few drachms of bloody serum at the very bottom of the cul-de-sac is avoided, an accident which he has seen occur in vaginal drainage; besides, the often very difficult operation of puncturing the vaginal cul-de-sac is thus done away with. 2. Although he still holds to his former opinion, that the mortality in ovariectomy depends mainly on septicæmia, still he admits that death



may occasionally occur from pure peritonitis, especially in a vitiated atmosphere, a possibility which he formerly denied. He had lately seen two cases of death from pure peritonitis.

On the whole, I believe that the majority of ovariologists now condemn this method of vagino-peritoneal drainage. I am afraid that Nussbaum and Spiegelberg (who were evidently not aware of the change of opinion by Dr. Sims when they followed his teachings), may still live to deplore their unqualified adoption of this practice.

During the discussion on the paper by Dr. Dawson, just mentioned, Dr. Sims remarked (what, indeed, he had already said in substance, on the occasion above referred to) that, after having followed the plan of securing the pedicle in ovariectomy with silver wire, for twenty years, he had been induced to abandon this practice, because he had seen two cases of secondary hemorrhage from the pedicle following its use. Adopting, then, Dr. Peaslee's method of double ligation by silk or catgut, and dropping the pedicle into the abdominal cavity, he found the stump in a sloughy condition at the post-mortem. His opinion now was, that the best and safest plan was to secure the pedicle with iron wire, twisted at both sides, and keep it outside. He had much to retract of his former views concerning the clamp. Ultimately he thought that Baker Brown's method of searing the pedicle would prove to be the best, especially if good instruments for applying the cautery were constructed. He would have a clamp with a gutter made, so that when the pedicle was cut off even with the upper surface of the clamp an exclusive surface would be left behind for the application of the cautery, and when such a large surface of the stump had been well seared, it could be safely returned to the abdominal cavity.

DR. Pallen said, on the same occasion, that he would first thoroughly crush the vessels of the pedicle and then apply the actual cautery, a plan which he believed would effectually prevent secondary hemorrhage.

In this and a previous paper the use of the *bichloride of methylene* as an *anæsthetic in ovariectomy*, in preference to ether, now commonly employed, was recommended by Dr. Dawson. He claimed, with Spencer Wells, who uses it exclusively, that it was as safe as ether (much more so than chloroform), and that it very rarely produces vomiting, one of the great objections, as is well known, made by many operators to ether, particularly in ovariectomy. Dr. Sims did not concur in the views of Dr. Dawson regarding methylene, considering it no safer than chloroform, although he had no valid cases in support of this opinion.

DR. PEASLEE said that he had never seen vomiting ensue when food had not been taken within three hours previous to the administration of the ether. Still, he felt disposed to try the methylene.

DR. GEORGE A. MURSICK reports a case of successful *ovariectomy* (*Am. J. Med. Sc.*, Jan., 1874), which differs from ordinary cases in the following particulars: 1. The removal of the tumor by enucleation (detaching it from its adhesions and vascular investment with fingers and scalpel; no hemorrhage, no ligatures); 2. The establishment of free drainage per vaginam by the seton, the vaginal and abdominal ends of the silk strands being tied together; 3. The administration of medicine by subcutaneous injections, in consequence of excessive gastric irritability; 4. The support of the patient by nutritive enemata; 5. The value of quinia in the prevention and cure of septicæmia.

MR. SPENCER WELLS reports an interesting and unusual case of *successful ovariectomy in a girl eight years of age* (*Brit. Med. J.*, March 14, 1874). The operation was performed in the usual manner (the diagnosis having been confirmed by tapping the cyst three weeks previously), the pedicle was tied with silk and dropped. The patient made a good recovery, and



sailed for New York, en route for San Francisco, her home, twenty-five days after the operation. The tumor consisted of a solid portion and a cyst, the former weighing three ounces, the latter containing twenty ounces of fluid. The solid portion was composed partly of true bone (shown to be such by the microscope) covered by true skin, from which grew a quantity of long, light hair, rolled into balls and matted together by sebaceous matter. The period when the tumor was first observed could not be ascertained.

Another equally interesting and successful case of *ovariotomy in a girl 7½ years of age*, is reported by DR. GEORGE CUPPLES, of San Antonio, Texas. It is, as he says, the tenth case of ovariectomy before puberty, which he has been able to discover in literature. The tumor was first noticed 3½ months before the operation, and was supposed to have been caused by the falling of a heavy piece of lumber on the abdomen three years before. The tumor was a polycyst of the size of a cocoanut, and was removed in the usual manner, the pedicle being tied by a single catgut ligature, and returned to the abdomen. The little patient was able to leave for home on the 14th day. The tumor consisted of sebaceous matter, and a considerable quantity of fine, loose hair. Its weight was 15 oz., the solid part 3¾ oz.; the fluid contents were very viscid and dark in color.

*Solid Tumors of the Ovary.*—DR. LEOPOLD (*Arch. f. Gyn.*, VI. 2) writes a long paper on this subject. He has arranged in a tabular form 56 cases; 43 of these have been collected from various sources, and the remaining 13 are published by him for the first time. Solid tumors of the ovary are rare; according to L. they are found only in the proportion of 1.5 per cent. of all tumors of the ovary. Externally, solid tumors of the ovary somewhat retain the natural shape of the ovary, and thus are to be distinguished from fluid tumors, which have an irregularly rounded form. Their consistence varies; they may be so soft as to give the suspicion of fluid, or even as hard as a stone. The thickness of the external coat varies much, and this, Dr. L. thinks, is a point of some importance, as regards the more or less rapid development of the tumors. A point of value in diagnosis is that, when the pedicle is short, you may find the tumor wedged down between the uterus and the rectum. Histologically, tumors of the ovary may be fibroma, enchondroma, sarcoma, or carcinoma. The fibromas are simple or complex, fibro-myoma, fibro-sarcoma. To these two others more rare must be added. The first, described by Waldeyer, presents lacunar blood-spaces, besides the fibroid tissue, and is very analogous to certain blood-tumors. This variety may help to explain some rare cases of true ossification of the ovary. The second has been described by Spiegelberg as areolar fibroma, and consists of a framework of fusiform cells, with large, cavernous vessels in places, resembling sarcoma. Enchondroma of the ovary is rare; sarcoma of the ovary, although scarcely mentioned in the text-books, has been frequently seen and examined by L., who also describes a new tumor, "lymphangioma kystomatosum," characterized by the cystic formation, the dilatation of lymphatic vessels, and a proliferation of the stroma. Ovariectomy was performed eight times for these solid tumors of the ovary, and only three times successfully.

Referring to the condition of menstruation in women suffering from solid or cystic ovarian tumors, L. repeats the fact, now well known from numerous instances, that menstruation may go on when both ovaries are degenerated, and even after they have been removed, and inclines to the belief that menstruation has no direct relation with ovulation.

In "*The Hastings Prize Essay on the Pathology and Treatment of Ovarian Diseases*," from which I regret not to be able to quote more, MR. LAWSON TART says that he has never met with a unilocular ovarian tumor, and

believes that all unilocular tumors in the neighborhood of the ovary are not ovarian, but of parovarian origin. In every truly unilocular tumor, Mr. Tait has found the ovary unaffected, though on several occasions he has seen it stretched over the cyst-wall. This opinion of Mr. Tait's does not coincide with the view generally held, that true unilocular tumors of the ovary, although rare, and not usually growing larger than a man's head, still do occur.

DR. ALEXANDER MILNE, in a paper "*On the occasional arrestive and discutient influence of pregnancy over pelvi-abdominal tumors*" (*Edinb. Med. Jour.*, Aug., 1874), combats the commonly accepted doctrine, that pregnancy not only accelerates the growth of ovarian cysts, but excites inflammation, adhesions, and suppuration. He cites three cases in which ovarian tumors of the size of an orange or the closed fist, were entirely discussed by pregnancy and parturition, or very much reduced in size. Although he admits the possible risks of pressure, inflammation, suppuration, and bursting of the cyst, still he does not think, judging from these three cases, that the danger is as great as usually supposed. He even counsels any of his readers who may have a maiden patient, both betrothed and the possessor of a unilocular ovarian, not to say nay, when asked whether this patient may marry; especially if the growth is small and clearly a monocyst, marriage may be a probable journey towards a cure.

The method of exploring the pelvic and abdominal cavities with the hand in the rectum, the *rectal touch*, first practised, developed, and recommended by SIMON, of Heidelberg, some three years ago, has frequently been put into execution during the past year. Dr. Leale, of this city, has endeavored to resuscitate a patient from syncope by touching the solar plexus with the hand in the large intestine; Dr. Thomas, Dr. Weir, and other physicians here and abroad have practised the manipulation with success and ease, and all agree as to its supreme utility in many and entire innocuousness in most cases. Care should be taken to pass only four fingers, never the whole hand, above the sigmoid flexure, and to be very cautious as regards the force used, and the distance the hand is introduced, if it measures more at the knuckles than 20 cm. (8") in circumference. By passing his hand, which had this measure, above the sigmoid flexure, Simon caused a rupture of the peritoneal coat of the intestine. As a rule, a hand which does not measure more than 25 cm. at its largest point, can be passed up to a point 6 to 7 cm. above the anus, where, according to Simon, the rectum measures from 25 to 30 cm., and by then elevating the bowel, a proceeding permitted by its natural mobility, the fingers alone may safely palpate the abdomen as high up as the umbilicus and even ensiform process; the introduction of the large portion of the hand, the knuckles, above a point 17 to 19 cm. (7") above the anus is, however, always unsafe. The great value of rectal exploration in the diagnosis of uterine and ovarian tumors is already sufficiently appreciated.

Under the title, "*Remarks on the employment of hydrotherapeutics in gynecology, and special indications for this method of treatment in chronic female diseases, particularly uterine affections*," DR. JOLY, "*Directeur des Thermes de la frégate, la Ville de Paris*," reports his experience as follows (*Gaz. Obstet. de Paris*, Dec. 20, 1874):

*Chloro-anæmia, dyspepsia, disorders of menstruation*.—During ten years Joly has treated, in his establishment, over 1,000 cases coming under this category, and asserts that all who regularly and methodically took the general douche once or twice a day, were cured within three months, or sooner. The action of the douche was to stimulate the capillary circulation, thereby improving assimilation and tissue-metamorphosis.

*Uterine displacements*.—The cold douche, general and vaginal, and hip

baths in running water, will, by their tonic and reconstructive power, more readily cure displacements of the womb than bandages, supporters, or pessaries of any kind. Of about 170 cases of this kind, Joly was able to verify a cure in about one-half, the remainder not having followed up the treatment with sufficient regularity or perseverance.

In 91 cases of recent or old *metritis*, and 53 of *congestion* or *engorgement of the uterus*, with or without ulceration, Joly saw 95 cures.

*Disorders of menstruation* in young girls, or at the climacteric, are almost always relieved by hydropathic treatment. If the first catamenial period is slow in arriving, the general douche will quickly bring it on and remove all the nervous troubles incident to its retardation. In amenorrhœa—or spanenorrhœa, insufficient menstruation, as Heywood Smith calls it—the douche applied to the lower end of the body will draw the blood to the uterus; in menorrhagia the douche to the upper portion of the body relieves the congestion of the uterus; in mere irregularity the douche to the whole body, general douche, is indicated; indeed, the general douche is to be considered the regulator, *par excellence*, of menstruation.

DR. A. TRIPIER, of Paris, gives us, in the *Archives of Electrology and Neurology*, Vol. I., No. 2, an excellent paper on “*Disorders of nutrition and displacements of the womb, and their treatment by faradization.*” Proceeding from the view that congestion and stasis are cured soonest and with most certainty by causing a transient hyperæmia and muscular contraction of the diseased organ (a plan, he says, superior to repeated local depletion, *anémiation*, which is always followed by another congestion), Tripier employs local faradization for the cure of congestion and engorgement, especially if associated with displacement of the uterus. In order to reach the affected tissues themselves, he has constructed various electrodes, *excitors*, as he calls them, which he carries into the bladder, uterus, and rectum, or applies to the abdominal walls or the loins. In *antelexions* or *versions* he uses the *uterine* and *rectal*, in *retroflexions* or *versions*, the *uterine* and *vesical* excitors, passed in to the point of flexion or body of the uterus, with the object of localizing the contractions in the posterior or the anterior wall of the womb, and thus remedying the displacement. In cases of *congestive obstruction*, *recto-vesico-uterine* or *recto-vesical* faradization is used; in *amenorrhœa*, the *sacro-pubie*; in *prolapsus uteri*, the *bi-inguino*; in *relaxation* and *prolapsus of the vagina*, the *bi-inguino-vaginal* and *vagino-uterine* faradization, in which the vaginal excitor is a solid speculum, have been found to be efficacious. In virgins the *abdomino-rectal* and *abdomino-vesical* methods will be found to answer, although but imperfectly, compared with the other more thorough applications.

During the first month the operation should be, if possible, daily, commencing five or six days after the cessation of the menses; later, three applications a week will suffice.

Flexions require longer treatment than versions. Tripier has never seen evil results of any kind follow this treatment, but I do not find that he mentions the length of time which he required to effect a cure, or that he did actually cure one of the affections mentioned by electricity.

His suggestion to administer the electrical treatment immediately after childbirth in a case which suffered from uterine displacement before pregnancy, seems to me worthy of attention, the condition of the uterus at that period offering, as it does, the best chances for the radical cure of the displacement.

The subject of *Intercostal Neuralgia in Women* is discussed by Dr. J. MILNER FOTHERGILL (*Obst. J. Gr. Br. and Irel.*, April, 1874), who says that the affection is found almost exclusively in weak, feeble, anæmic women, generally suffering from constipation, leucorrhœa, and severe uterine de-

rangement, or else nursing beyond the usual limit. The pain in the side, usually the sixth intercostal space, and the back between the shoulders, is almost characteristic. As a rule, the disease occurs among married women and hard-working, careless servant-girls; it is rare to find it after the menopause. The prognosis is good, although the progress towards cure is often tedious.

Fothergill combines a stimulating and tonic treatment, such as the carbonate of ammonia, with the ammonio-citrate of iron in an infusion of quassia; or later, sulphate of quinia, muriate of iron and quassia. If gastric symptoms predominate, the well-known mixture of gentian and rhubarb, after Ferriar. Belladonna and mustard plasters locally, cold hip baths, and vaginal injections, laxatives, are the other remedies generally found necessary and useful. The wearing of skirt-supporters is highly recommended.

DR. J. ASHBURTON THOMPSON, of London, reports three cases of *hysteria* and mental depression in young girls, aged sixteen, eighteen, and twenty years respectively, and one case of *epilepsy* in a female child four years of age, which he treated successfully by the administration of *free phosphorus*,  $\frac{3}{16}$  gram for the child, and  $\frac{1}{2}$  grain for the adults, in the alcohol and glycerine mixture, three times a day, the mixture being continued in the last case for five months, in the three other cases fifteen days.—*Obst. J. Gr. Br. and Irel.*, June, 1874.

"*The Influence of Posture on the Health of Women*," is the title of an article by DR. J. H. AVELING, commenced in the November number of the *Obstetrical Journal of Great Britain and Ireland*.<sup>6</sup> It treats of a subject intelligible as well to our female patients and the weaker sex at large as to ourselves, and the importance of which is but too generally overlooked. There can be no doubt that a large proportion of those uterine disorders, which are not developed after parturition, are due to faulty and pernicious habits of posture and dress, among which the beloved rocking-chair and lounge, the heavy skirt supported from the hips, the high-heeled shoes, and that bulwark against rational medical counsel, the corset, are only some of the principal agents. The continuation of the article in the January number of this year, and the fact that it is not yet concluded, absolves me from further consideration of it in this Report. I thought it too interesting and important, however, to pass over entirely.

A few modifications of operations have been introduced or are still on trial at the Woman's Hospital. DR. THOMAS has several times tried the plan of joining together the sides of the longitudinal fold of mucous membrane detached from the posterior wall of the vagina in the operation for rectocele and ruptured perineum by means of a long steel pin, which is then enclosed in the perineal sutures; this pin is withdrawn when the sutures are removed. Its object is to cause firmer union of the sides of the vaginal fold, and obviate the usual gradual return of the narrowed vagina to its former abnormal size; the measure is still *sub judice*.

DR. THOMAS has also modified the ordinary operation for ruptured perineum, where there is redundancy of the posterior vaginal wall, by lifting up this redundant vaginal tissue with the finger in the rectum, paring it on a level with the denuded surfaces on the labia, and bringing it into the wound by the same sutures which unite the lateral surfaces of the perineal body. The rectocele is thus made to constitute the posterior wall of the restored perineum, the floor of the vagina being on a level with the new posterior commissure, and the original normal C curvature of the posterior vaginal wall is restored instead of the exaggerated S form presented by it in every case of complete rupture of the perineum. This operation has been performed successfully, I think, four times, and appears to



be a decided improvement on the old method. An additional advantage it possesses is, that during union all vaginal fluids are obliged to flow over the intact floor of the vagina and out at the posterior commissure, instead of working their way between the edges of the new wound and interfering with union, as in the ordinary operation.

DRS. EMMET and PEASLEE have lately been practising a similar operation, differing from that of Dr. Thomas in that the denuded surface on the posterior vaginal wall is less broad and more pointed, and that only the first or uppermost perineal suture catches the tongue of the vagina projecting into the perineal rent.

Since his return from Europe last Fall, DR. THOMAS has been using Richardson's serrated scissors in all operations about the genital organs, where much parenchymatous hemorrhage is to be expected, as in laceration of the cervix, removal of a fold of vaginal mucons membrane in rectocele or vesicocele, and finds that the crushing action of the scissors, while usually effectually preventing severe hemorrhage, in no way interferes with union by first intention.

The special points in which Gynecology appears to me to have made a more or less decided progress during the past year are the following:

1. The investigations into the nature of menstruation and its relation to ovulation.

2. The hypodermic injection of ergotine in uterine fibroids, the number of cases of which successfully treated by this method is constantly increasing.

3. The perfection of the operations of enucleation *per vias naturales*, and of gastrotomy in large fibroid tumors of the uterus.

4. The differentiation of the lesion of laceration of the cervix from ulceration of that part, for which it has hitherto usually been and doubtless frequently still is taken; the appreciation of the importance of that injury and of the necessity for its operative cure.

5. The rapid forcible dilatation of the urethra and the recognition of the numerous disorders of that canal and the bladder, in which it is of paramount utility.

6. The vesical touch.

7. The rectal touch, both of which methods are daily winning new supporters and advocates; and, if I may be permitted to style as a progress what on its face is a retrograde step.

8. The gradual return from the doctrines of the extreme operative to those of the more moderate school of uterine therapeutics, which uses the knife only when the operation is based on rational scientific principles, and offers a fair chance for the recovery or ultimate benefit of the patient.

This Report has attained dimensions so far beyond those I had marked out, that I find myself obliged to confine my remarks on the several new gynecological works which have made their appearance during the past year, to a mere notice. The deservedly popular text-book on *Diseases of Women*, by DR. T. GAILLARD THOMAS, which reached its fourth edition in 1874, five years after the appearance of the first, is probably in the libraries of most of the gentlemen present, many of whom perhaps also possess the other in many respects equally valuable work by DR. ROBERT BARNES, of London, which entered on its first edition in the early part of 1874. Those gentlemen who read German will derive much useful information from a work on *Operative Gynecology*, by HEGAR and KALTENBACH, Professors in the University of Freiburg, in Baden. It is the only work in existence solely devoted to and embracing the whole surgery of the female genital organs. A large text-book in two volumes, on the *Diseases of the Female Sex*, by HERMAN BEIGEL of Vienna, known here and abroad as the tran-



slator into German of Sims's "Uterine Surgery," and recent ovariectomy pamphlet, and of Graily Hewitt's "Diseases of Women," has also appeared during the past year, and has been the subject of much criticism in Germany. Still, it, like almost every new book, contains many new and excellent points, and I know some gentlemen who prefer it to any of the existing German works on Gynecology. The diagrams are numerous and excellent. A short essay on "*The Parasites of the Female Mammary Gland*" is published by DR. HAUSMANN, of Berlin, as a continuation of his former pamphlet on "*The Parasites of the Female Sexual Organs.*" It is worthy of perusal.

In conclusion, let me call the attention of those gentlemen who desire to study up many points of interest in the etiology and treatment of uterine diseases and to learn numerous little knacks and "dodges," which are usually acquired only by practice or a schooling such as it is the good-fortune of but a few to enjoy, to the two excellent papers already referred to by DR. EMMET, on "*The Philosophy of Uterine Disease, etc.,*" and DR. WM. GOODELL, "*Some Practical Hints for the Treatment and Prevention of Diseases of Women,*" *Med. and Surg. Rep.*, Jan. and Feb., 1874. The latter, especially, contains more useful information and more "practical hints," such as are not usually found in the text-books, than any paper on the subject which has ever come to my notice.

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## REVIEWS AND NOTICES OF BOOKS.

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CYCLOPÆDIA OF THE PRACTICE OF MEDICINE. Edited by DR. H. VON ZIEMESSEN, Prof. in Munich. AMERICAN TRANSLATION. Edited by ALBERT H. BUCK, M.D. New York: Wm. Wood & Co., 27 Great Jones Street. 1875.

VOL. I. *Acute Infectious Diseases*: Typhoid Fever and The Plague, by Prof. Liebermeister; Relapsing Fever, Typhus Fever, and Cholera, by Prof. Lebert; Yellow Fever, by Prof. Haenisch; Dysentery, by Prof. Heubner; Epidemic Diphtheria, by Dr. Oertel. Pp. 708.

VOL. II. *Acute Infectious Diseases*: Varicella, Measles, Rubella, and Scarlet Fever, by Prof. Thomas; Small-Pox, by Dr. Curschmann; Erysipelas, Miliary, Fever, Dengue, Influenza, and Hay-Fever, by Dr. Zuelzer; Malarial Diseases, by Prof. Hertz; Epidemic Cerebro-Spinal Meningitis, by Prof. von Ziemessen. Pp. 751.

VOL. III. *Chronic Infectious Diseases*: Syphilis, by Prof. Bäumler; Infection by Animal Poisons, by Prof. Bollinger; Diseases from Migratory Parasites, by Prof. Heller. Pp. 672.

The first two volumes of this admirable work have already received so many favorable notices from the medical press as to render it almost superfluous for us to say anything more in their favor. Still, as we were unfortunately prevented by

press of matter from reviewing the work at the time of the appearance of the first two numbers, we feel that we ought at least to say a few words of sincere endorsement of all that has since been written in its praise. Judging from the three volumes already before the profession, the *Cyclopædia* will be, without doubt, the most comprehensive and thoroughly scientific medical work in existence. A glance at the indices of these three volumes will suffice to convince the reader that nothing short of an actual monograph could give more on each subject than is found here. The authors are all Germans, men still in the prime of life, and engaged in active research in or thoroughly acquainted with the departments which each one has undertaken to discuss; but the authorities consulted by them, as shown by the list at the head of each chapter, are by no means exclusively German, but widely cosmopolitan, the names of American, English, French and Italian authors appearing quite as frequently as those of writers nearer home. An objection, which we have heard made against this so-called "German" work, is thus effectually disposed of. The names of the authors and the positions they have held and still hold are sufficient guarantees for the excellence of their mental productions.

When the fifteen or twenty volumes of the *Cyclopædia* will all have appeared, as is to be the case in three or four years, the whole work will be an ornament to every medical library and an inexhaustible source of information to its possessors. To the progressive, hard-working physician of the present day it is, indeed, an indispensable acquisition.

The translation, thanks to the care of the translators and in a great measure to the untiring supervision of the American editor, is very good, and reads on the whole as easily as though it were original English.

The presswork, paper, and binding of the three volumes are a credit to the printers and the publishers, to the latter of whom the English-speaking part of the profession owe a large debt of gratitude for having undertaken the stupendous and more or less hazardous experiment of translating and publishing so large a work.

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DISEASES OF THE RESPIRATORY ORGANS, HEART AND KIDNEYS,  
By ALFRED L. LOOMIS, M.D., Professor in the University of  
the City of New York, etc. New York: Wm. Wood & Co.,  
27 Great Jones St. 1875. Pp. 549.

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A MANUAL OF DIET IN HEALTH AND DISEASE. By THOMAS  
KING CHAMBERS, London. Philadelphia: Henry C. Lea.  
1875. Pp. 310.

THE AMERICAN  
JOURNAL OF OBSTETRICS  
AND  
DISEASES OF WOMEN AND CHILDREN.

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ORIGINAL COMMUNICATIONS.

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THE GENESIS OF AN EPIDEMIC OF PUERPERAL FEVER.

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By WILLIAM T. LUSK, M.D.

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DURING the early months of the year 1874, the Lying-in Department of the Bellevue Hospital, in this city, was visited by an epidemic of puerperal fever of singular intensity and violence, which led finally to the transfer of the obstetrical service from the Bellevue to the Charity Hospital. It has seemed to me that it would be a profitable task to trace, so far as it is possible, the origin, progress, and growth of the epidemic, to serve for future guidance and warning.

A glance at the casualties of the second half of the preceding year, will, perhaps, serve to bring into sharper contrast the distinction between ordinary puerperal accidents, common, alas, to both civil and hospital practice, and the raging of a monstrous pestilence, for which large hospitals, viciously conducted, alone furnish the atmosphere, the soil, and the means of expansion.

It is important, however, at the outset, not to be misled into regarding so-called hospitalism as a distinct entity, instead of a sum of ponderable factors. It is not to be gotten rid of by childish expedients. Holy water will not exorcise it. The latest vagary in hospital construction fails to solve the problem

of its extinction. Mastery of hospital evils must come from acquaintance with not one, but all the elements out of which such evils are generated.

During the summer months of 1873 the hospital enjoyed an almost complete immunity from puerperal disorders. From the 11th of May to the 26th of August there were 136 confinements, without a single death. On the latter date Mary G., a primipara, single, after a severe labor lasting thirty-seven hours, was delivered by forceps by the house-physician at that time on duty in the obstetrical service. The os is reported as having been rigid at the time of the operation, and the dilatation only partially completed. The perineum was badly ruptured. The patient subsequently died of peritonitis. The peritonitis was of a purely traumatic character, and though terminating fatally, did not perceptibly influence the health of the service.

On the 12th of September Mary S., single, æt. 21, primip., was delivered by forceps, after a prolonged labor of fifty hours. A rigid cervix demanded the preliminary employment of Barnes's dilators. Six days later this patient's temperature went up to  $103\frac{3}{4}^{\circ}$ . With this exception she did well, and made a rapid recovery.

On the same day (September 12th) Bridget K., married, æt. 38, sixth pregnancy, was delivered by forceps on account of an attack of puerperal convulsions. The child weighed 10 lbs. 14 oz. The patient developed on the third day slight tenderness along the femoral vein, and was transferred to a medical division, but made a good recovery.

On the 13th instant Kate R., single, æt. 29, primip., was delivered naturally after a labor lasting nearly thirty-five hours. On the 15th instant she was transferred to a medical division on account of tenderness in the left iliac fossa. Recovered.

On the 17th inst. Mary H., mar., æt. 35, sixth preg., had a high temperature soon after delivery, and was accordingly transferred to a medical division, where she speedily recovered.

Though as yet no deaths had occurred, these four cases excited so much uneasiness on my part that I directed the closure of the confinement ward, and the room next adjacent in which the puerperal women were ordinarily retained during the first four days following parturition. Meantime Medical

Ward 24 was placed by the warden, Mr. Brennan, at my disposal for subsequent confinements. The necessity for this action was demonstrated by the fact that Ann R., the last patient confined in the old ward, likewise had for two days a temperature of  $104^{\circ}$ , and a rather slow convalescence.

On the 21st inst., *i.e.*, after the change had been made, six patients were confined. Of these, Hannah H., married, æt. 32, tenth preg., suddenly exhibited symptoms of collapse, which proved to be the result of spontaneous rupture of the uterus. She died at 7 A.M. Two of the six patients were confined before her death took place, and two after the corpse had been removed from the room. All these did well. One, however, Jennie T., single, æt. 22, primip., was in labor at the time of Hannah's death, and witnessed the painful scenes that closed her life. This patient manifested at the time intense mental distress. On the third day she developed symptoms of peritonitis, and died October 2d. In this case it would be absurd to refer the death to a hospital poison. The rapid recovery of four other patients, confined on the same day, and by the same attendants, demonstrates this. She died precisely as any young, nervous, unmarried primipara would have done in civil practice, after having been subjected to a similar shock during labor.

On the 29th of Sept. there were two confinements. One, Margaret W., had an attack of cellulitis, following rupture of the perineum. The other, Mary H., single, æt. 27, primip., had an easy confinement. She had, however, entered the hospital some months previous, to conceal herself from her friends, whose anger she greatly dreaded. The day after her delivery she was accidentally discovered in the lying-in ward by her sister, who came to her bedside, and there cursed her, and loaded her with reproaches. This sister's language was so violent that it became necessary to summon the hospital police, and expel her from the precincts by force. Soon after this occurrence the patient became maniacal, developed symptoms of peritonitis, and died on the 5th of October.

Thus among the patients confined in the month of September one died from spontaneous rupture of the uterus, and two from peritonitis apparently arising out of excessive mental emotion.



The month of October was singularly free from metritic disorders, and yet there were three deaths. 1st, Mary C., brought into hospital three weeks *after* confinement, with cerebral embolism. 2d, Ellen Mc., who had uræmic convulsions *after* delivery. All attempts to excite a sufficient urinary secretion proved ineffective. 3d, Laura B., sent into hospital after labor had commenced. This patient was in the last stages of consumption, so that it was found she would hardly live to the completion of labor. In fact she expired a few hours after the birth of her child.

Besides these cases, in the same month Ellen C., a prisoner, sent to the Hospital to be confined, previous to her transfer to the Penitentiary on Blackwell's Island, developed puerperal mania.

On the 1st of November the regular ward of the obstetrical service, which had been closed from the 20th of September, for purposes of disinfection, was once more reopened.

The first patient, Mary C., married, æt. 38, fourth preg., was sent to the hospital from a so-called "private asylum" in the city. She was, on her arrival, in a weak and anæmic condition. The house-physician, Dr. Fleming, found the cervix, on digital examination, in a soft and dilatable condition. On withdrawing his finger he found it covered with dark, grumous blood. I was at once summoned, and on introducing my hand found that it passed directly through an extensive laceration in the posterior uterine wall into the abdominal cavity. I at once extracted the child, which proved to be in an advanced state of decomposition. The patient died twenty-four hours afterward of shock. She did not in any way affect the health of the service, as shown by the fact that the succeeding seventeen cases had not one dubious symptom.

The eighteenth case, Jennie M., married, æt. 33, eighth preg., was delivered Nov. 10th, after a labor lasting forty-nine hours thirty min. The first stage was very tedious, lasting forty-nine hrs. ten min. The anterior lip of the cervix became very œdematous. The expulsion of the child followed, however, five minutes after the rupture of the membranes. In her three previous labors she had been delivered by forceps of still-born children. On the afternoon of the third day (12th inst.) her temperature rose to  $104\frac{3}{4}^{\circ}$ , P. 140, R. 34. The day following the

record shows T.  $106\frac{1}{2}^{\circ}$ , P. 126, R. 36. On the 14th inst., T.  $102\frac{1}{2}^{\circ}$ , P. 120, R. 126. Patient was feeling more comfortable. On the 15th inst., the patient being well under the influence of opium, the respirations fell to 18, P. 132, T.  $103\frac{1}{2}^{\circ}$ . The abdomen was tympanitic, and tender on pressure. The skin was moist and the face flushed. On the 16th inst. she was transferred to Med. Ward 23, where her temperature rose to a high point, not specifically recorded, the pulse became rapid and feeble, and the abdomen excessively tympanitic. Six hours after her transfer the patient died. The autopsy was made by Dr. Delafield, who simply reported, "No lesion found except slight endometritis. No peritonitis."

I have been thus specific, because this patient, in contrast to the others whose cases terminated fatally, exercised for a long period of time a most pernicious influence upon the obstetric department. Indeed it is a question whether we may not fairly ascribe to her the starting-point of the epidemic which subsequently assumed such terrific proportions. Yet at the time of her confinement the health of the service was excellent. All the patients were doing well. No change had been made in the attendance. There was nothing but the long labor to apparently account for the fatal issue.

One point in her history, however, deserves to be noted. My attention was drawn to the patient in the month of July previous, when she claimed to have reached the ninth month of pregnancy. The question then arose as to the advisability, in view of the death of the three previous children during parturition, of inducing premature labor in the interest of the unborn child. On examination, however, in spite of her positive statement, the pregnancy proved to be not further advanced than the fifth month. The patient was very rebellious when informed of the fact, and persisted in remaining in the wards devoted to the puerperal women, where she was employed as a helper by the nurses, up to the time of her confinement, four months afterwards.

The effect of this case of fever did not at once manifest itself, for one patient confined on the 11th inst., one on the 14th inst., and two on the 16th inst., all made good recoveries. It was, too, some time before a single fatal case occurred. The prevalence of elevated temperatures, accompanied by a quickened,

or what is euphemistically termed "a hospital pulse," gave, however, the warning of impending danger. It will be remembered that Jennie M. was the eighteenth patient confined during the month. The 23d, the 24th, the 26th, the 29th, the 30th, the 31st, the 34th, the 37th, the 38th, and the 40th cases, or 10 cases out of 24, had chills on the third day.

Of these, one died, viz., Mary B., single, æt. 22, primip., confined Nov. 27th, and attacked with pelvic peritonitis the following day. The case terminated fatally Dec. 25th, probably from rupture of abscess into peritoneal cavity.

CASE 38. Lizzie Stern, single, æt. 19, primip., possesses special interest, as a year later she died suddenly from shock, a short time after confinement, in the boat-house, at the foot of East 26th street, where she had been carried *en route* to the obstetrical service of Charity Hospital.

CASE 39. Katie G., single, æt. 18, primip., was delivered by forceps, Nov. 29th, after a painful labor lasting fifty-four hours, and died on the fourth day from exhaustion.

On the first of December Fannie G., married, æt. 28, primip., had a labor, of which the first stage lasted ten hours, the second stage, five hours and fifty minutes. During the long second stage febrile symptoms were developed, and it was thought best to terminate the labor by means of forceps. The child weighed 9 pounds 2 ounces. The forceps' delivery was unattended by difficulty. As the febrile disturbance continued after delivery, it was thought best to remove the patient immediately to a medical ward. On the third day the pulse reached 140, R. 28, T.  $104\frac{1}{2}^{\circ}$ . During her subsequent illness, which terminated fatally on the 2d day of January, the patient made no complaint, except toward the end, of either pain, tenderness, or headache. The temperature ranged from  $102^{\circ}$  to  $105^{\circ}$ . During the last hours of her illness it ran up to  $107^{\circ}$ . However, under the influence of sponging, quinine, alcohol, and tinct. of gelseminum, the temperature frequently reached a nearly normal point. Thus on Dec. 12th, it was  $99^{\circ}$ ; on the 13th,  $99^{\circ}$ ; on the 14th,  $99^{\circ}$ ; the 17th,  $100^{\circ}$ ; the 18th,  $100^{\circ}$ ; the 21st,  $98\frac{1}{2}^{\circ}$ ; the 22d,  $100^{\circ}$ ; the 25th,  $100^{\circ}$ ; the 27th,  $100^{\circ}$ ; the 29th,  $99\frac{1}{2}^{\circ}$ . But even on the most favorable days the evening exacerbations ranged between  $103^{\circ}$  and  $105^{\circ}$ . The pulse was for the most part persistently rapid, only twice (on the 17th and 21st) sinking be-

low 100 (viz., 96 and 92). The ordinary range was between 116 and 144, becoming 160 toward the end. For the most part of the time the patient said she "felt splendid." The vaginal discharge was not offensive. A slight laceration of the perineum became covered with grayish exudation, noted as cleaning up on the 22d, and as looking well on the 26th of Dec. The appetite was repeatedly reported excellent. Perspiration was always excessive. The sleep was generally disturbed at night by unpleasant dreams. Active delirium set in on the 29th of Dec. The bowels were moderately tympanitic. Slight pain was elicited by pressure of the fundus uteri. During the last two weeks of life chills occurred nearly every day. Pain and swelling of the joints, especially in the left shoulder, now occurred. Phlegmasia dolens was developed in the left leg Dec. 29th. On Jan. 2d, P. 160, R. 30, T. 107, mouth sore, tongue red and tremulous, subsultus tendinum, sighing respiration, face yellow, breath sweetish. Patient finally became unconscious, and death followed. There was no autopsy, but the case may be fairly set down as one of metrophlebitis, followed by purulent infection.

The fact that Fannie G. was transferred directly after confinement to a medical ward, and did not enter the puerperal wards at all, together with the fact that her fever came on prior to delivery, is evidence that the infection must have been derived either from the nurses, the physicians in attendance, the *armamentaria* employed, the beds used for confinement, or the condition of the confinement ward itself. Of course the long second stage rendered her peculiarly vulnerable to any existing pernicious influences. Some light, perhaps, may be obtained regarding these points from subsequent occurrences.

From the 2d to the 6th of December, inclusive, seven patients were confined. Of these all except one had a chill, and more or less pain and tenderness on the third or fourth day. In each case the uterus was noted as large, and the discharge as offensive. Six of the seven, however, made rapid recoveries.

In the seventh case, Agnes C., single, æt. 25, second preg., confined Dec. 6th, the record of the fourth day showed, pulse 132, resp. 28, temp.  $104\frac{1}{2}^{\circ}$ . On the tenth day there was muttering delirium, the eyes were sunken, the pulse 150, R. 33, T.  $102\frac{1}{2}$ . On the eleventh day death took place, and the autopsy

showed the usual lesions of puerperal peritonitis. Was there any reason why this case should have proved fatal, not common to those already mentioned as having recovered? There is this comment in the history book: "Patient worrying greatly at giving birth to a second illegitimate child. Crying all the time." It is puerile folly to deny to mental emotions a prominent place among the causes of puerperal disorders.

On the 8th inst. it was determined once more to close up the confinement ward and shift the patients to another locality.

The first effect was happy. Of five cases confined four had not a single bad symptom. The other, the third in point of time (Dec. 8th), was violently agitated the second day after her confinement by the screams of a patient brought into the hospital from the street in the last stages of labor. From a perfectly normal condition her pulse became 148, the resp. 40, the temp. 105°. The febrile disturbance was maintained for two days following, and then subsided. How far this disturbance was due to moral causes it is impossible to say. A patient, confined the following day in the pavilion outside the hospital, where she was conveyed *en route* to the Charity Hospital because of the sudden intervention of labor, presented a parallel history, though the medical attendant, the nurses, and the locality were at that time all free from the slightest suspicion of taint.

A patient confined in Dec. 10th, died on the fifth day following of phthisis. So far, then, it was by no means clear whether simply a change of wards, but with a continuance of the same physicians and nurses, had had any special practical effect in the way of improving the service. On Dec. 12th, however, the answer came. Bridget Brophy, single, æt. 36, primip., had a long severe labor. The stage previous to the rupture of the membranes lasted fourteen hrs. forty-five m. The subsequent stage continued twelve hours, during the last six of which the patient seemed greatly exhausted. Finally she was delivered by forceps. The operation proved simple enough, but symptoms of shock ensued. The next day the pulse was 150, the resp. 32, the temp. 102°. The uterus was relaxed. In the afternoon, in consequence of the free administration of stimulants, the storm subsided. On the 15th inst., though her temperature was 99° only, and the patient said she "felt splendid," she was trans-



ferred to a medical ward on account of the offensiveness of the lochia. On the 17th she became delirious, the respirations irregular, the pulse feeble, the breath sweetish, the discharges continuing very offensive. A large diphtheritic patch, three inches in diameter, was discovered at the same time, covering the entire posterior wall of the vagina. On the 23d inst. she died, with the symptoms of intense septicæmia. Thus it was seen that a mischievous element had been imported into the freshly opened ward, which had only needed a sufficiently severe case of labor to assert its influence. During the time that elapsed between the 12th and the 19th inst. there were ten cases of confinement, of which six had more or less febrile disturbance, while four escaped unscathed.

On the 19th of December the service passed into the charge of Dr. I. E. Taylor, who at once restored the patients to the ward which had been closed by my order on the 8th of December. During the remainder of the month following the transfer, there were twenty confinements. Of these eight manifested disturbance of greater or less severity. Ten cases are reported to have done well, and in two instances no record is given. Thus far, then, it will be seen that no special results were obtained by exchanging one set of wards for another.

It is proper, perhaps, here to observe that many of the cases marked as manifesting a febrile reaction did not differ materially from a class of cases common enough in private practice, where the brevity of the attack frees the physician from serious apprehension. But when an aggregation of such cases occur in hospital practice, they are like the mutterings in the clouds which betoken the impending storm.

According to a hospital rule, two *internes* of the house staff were selected in order, and allowed the privilege of attending the labor cases of the obstetric service for one month. The beginning of each month, therefore, witnessed a change in the physicians. This change, taking place on the 1st of January, enables us to narrow down still further the sources by which the contagion was spread. Changing wards, it has been observed, did no good. Let us see whether with a change of physicians the patient fared any better.

The first two patients in January had no questionable symptoms. In the next case, however, the temperature rose on the

third day to  $103^{\circ}$ . On the fifth day the fever fell. On January 17th the patient was allowed to sit up. On January 21st she complained of abdominal pain, and of not feeling well. The next day the patient was pale, weak, and anæmic. The face looked pinched and emaciated, the eyes lost their lustre, the abdomen became tympanitic, and by 9.30 A.M. the register showed P. 160, R. 36, T.  $100\frac{1}{2}^{\circ}$ . In the afternoon the pulse was jerky and almost imperceptible, the extremities were cold and clammy, while at the same time the patient professed to feel very comfortable. At 10 P.M. she died. At the autopsy the abdomen was found filled with sero-purulent fluid, while lymph glued together intestines, liver, omentum, uterus, the abdominal and parietal walls. The uterus was  $6\frac{1}{2}$  inches long. That the peritonitis should have been so far overlooked in this case as to have led to the extension of permission to the patient to sit up on the 17th inst. is not a little singular. The evidences of adhesive inflammation within the abdominal cavity certainly show that the trouble must have existed long prior to the 21st inst., when the alarming symptoms were first noticed. The latency of the symptoms up to this point make the case a very remarkable one.

The fourth patient, who was delivered on the 6th of January, had moderate febrile disturbance on the fifth, sixth, and seventh days following confinement. She was discharged from the hospital on the 24th, but returned Feb. 2d, to be treated for subinvolution of the uterus.

The fifth patient, delivered Jan. 6th, had a short labor of three hours. Everything went on well, apparently, until the 13th inst, when slight abdominal pain was complained of, and the temperature rose to  $103\frac{1}{4}^{\circ}$ . On the 15th inst. the temperature fell to  $98\frac{3}{4}^{\circ}$ . Vomiting came on. The pulse, however, did not fall below 116. On the 17th the patient was weak, collapsed, and cyanosed. The pulse became imperceptible, and in the afternoon the patient died. In this case the temperature alone afforded no clue to the severity of the attack. Yet the post-mortem examination showed the peritoneum to be filled with purulent fluid and lymph. There had existed, likewise, pericarditis and pleuritis, limited to the lower surface of the lungs and the corresponding part of diaphragm. The uterus and vagina were normal. In this instance the morbid element

whatever might have been its nature or origin, evidently was disseminated by the lymphatics through the system.

Susan M., single, æt. 22, primip., confined on same day as foregoing (Jan. 6th), had a chill come on during labor. The placenta was retained, and was removed by myself two hours after the birth of the child, as I happened at the time to be accidentally in the hospital, though not on duty. The delivery of the placenta was accomplished by the introduction of two fingers into the cervix. This patient died Jan. 12th, with all the symptoms of general peritonitis.

The three following cases (viz., the 7th, 8th, and 9th, consecutively) all had fever, pain, and abdominal tenderness, though all recovered.

Thus it will be seen that the change in the physicians wrought no miraculous transformation in the sanitary aspects of the service.

During the remainder of the month there were sixteen confinements, of which eight had, in various degrees, febrile and pelvic disturbances, and eight completed the period of convalescence without a single abnormal incident. For this slightly improved condition of affairs I think we must look in part to the reduced number of confinements during the month (there were forty-six in December and twenty-five in January), and partly to efforts to promptly remove fever cases from the wards so soon as the first symptoms of trouble were developed. It might be well here to state, in anticipation, that, in my opinion, the transmission of the disease from patient to patient was mainly accomplished through the agency of the nurses. It was to have been expected, therefore, that a diminution in the number of patients confined, and the removal of those infected, would lessen, while it would not entirely destroy, the capacity of the nurses for evil.

The first patient confined in February had moderate fever on the fifth, sixth, and seventh days. The second patient did perfectly well. The third patient, however, a married primipara, 37 years of age, confined Feb. 5th, had on the afternoon of the second day the following record: R. 24, P. 112, T. 103°. By Feb. 8th the abdomen had become tympanitic and tender, there was thirst and anorexia, the tongue and lips were dry, and directions were given to remove her to the third medical divi-

sion. Feb. 10th the patient was delirious. Feb. 12th, R. 44, P. 156, T.  $101\frac{3}{4}^{\circ}$ , face livid, failing pulse, and death at noon.

The volcano had been for a time quiescent, but was not yet extinct.

The fourth, fifth and sixth patients all did well. The seventh patient had fever on third day, subsiding on the sixth day, and returning once more on tenth and eleventh day.

The eighth patient, Sarah L., single, æt. 19, primip., delivered Feb. 11th of a premature child, after a labor lasting nineteen hours, twenty minutes. On the third day developed abdominal tenderness, on the sixth day was delirious, with dry tongue, insomnia, blue lines about eyes, flatus, involuntary passage of urine, and great tenderness over right inguinal region. Notwithstanding the severity of the symptoms, she passed a quiet night and awoke in the morning convalescent. She was discharged well, March 12th. There was no apparent reason for this exceptionally severe attack, except that from the outset "she was very despondent, shedding tears when spoken to."

CASE 9 had chill, abdominal tenderness and fever ( $104^{\circ}$ ) on third day, but no further trouble.

CASE 10. Mary R., single, æt. 17, primip., confined Feb. 13th. Previous to delivery had oedema of legs, and labia externa, but no albumen in urine. There was post-partum hemorrhage, and the placenta was extracted manually. She was transferred immediately after confinement to Med. Ward No. 24. The lochia became offensive, a diphtheritic ovoid ulcer appeared on each labium on third day, fever set in, but the patient eventually recovered.

CASE 11 had cellulitis.

CASE 12 made a good recovery.

CASE 13 had an attack of cellulitis, which kept her in bed until April 21st.

CASE 14. Nellie M., married, æt. 21, third pregnancy, confined Feb. 20th, had a perfectly normal labor; died sixty hours after confinement, with symptoms of intense septicæmia.

CASE 15 had an attack of cellulitis, which kept her in bed until May 1st.

CASE 16 had pneumonia, but no pelvic symptoms.

CASE 17 had no trouble.

CASE 18 had fever on the third, fourth, and fifth days.

CASE 19 had febrile symptoms of sufficient severity to lead to her transfer on the fourth day to a medical ward.

CASES 20 and 21 had each moderate febrile disturbance for two or three days after confinement.

CASE 22 had œdema pulmonum, as a complication of general anasarca. The patient had no puerperal symptoms, and recovered in a month's time.

CASES 23 and 24 both did very well.

CASE 25. Mary C., married, æt. 28, fifth pregnancy, confined after a long labor (twenty-three hours), Feb. 23d; had a high temperature the following day, profuse fetid lochia, and great nervous prostration. These symptoms subsided in two weeks, but on March 10th the patient had a severe chill, followed by a temperature of 105°. A second chill followed the succeeding day. On the 14th inst. pneumonia developed, and on the 15th the patient died. The autopsy revealed decomposing pus in the sinuses of the uterus and around the ovarian veins, peripheral abscesses, pyelonephritis, and pyæmic (gangrenous) abscesses in the lungs. There were no peritoneal symptoms in life, nor any lesions of the peritonæum found after death. The patient had primarily puerperal phlebitis, followed by metastatic abscesses.

During February there were twenty-five confinements. Three patients died. Twelve had more or less febrile disturbance. One had pneumonia, one œdema pulmonum, and eight had no abnormal symptoms.

Thus, during the months of January and February, out of fifty patients confined, there were six deaths, and only twenty-four of the entire number escaped without febrile symptoms of more or less severity.

From Feb. 23d to March 1st there were, fortunately, no confinements. On the 1st of March I resumed my duties as attending physician to the Obstetrical Department. It soon became apparent that the sanitary condition of the service had passed into a new stage of development. Whereas, heretofore, the change of the patients to a healthy part of the hospital had accomplished but little in the way of preventing the continuance of febrile outbreaks, the attacks came on three or four days after delivery, and were probably due to the neglect of ordinary precautions on the part of ignorant and irresponsible nurses. But by this time the aggregation, for a period of



ten weeks, of infected patients in the same locality, had poisoned the wards themselves. This was evidenced by a prolongation in the duration of labor, the frequency of post-partum hemorrhage, and the great difficulty of getting the uterus to contract after parturition had been completed ; that is to say, an abnormal performance of a whole series of acts which were completed prior to the period when the patients naturally fell to the charge of the nurses. It would in all likelihood have been well to have, at this time, once more removed the patients to a healthy locality, but this was, owing to the overcrowded condition of the hospital, an impossibility. During the month of March it will be seen that, while the aggregate amount of sickness was great, the death-rate was small ; a result in large measure due to the untiring exertions of the *internes* in charge.

In the first two cases there was very mild febrile movement. Case 3 had pelvic cellulitis and phlegmasia, but recovered.

CASE 4. Mary S., married, æt. 27, primip., confined March 3d, had a long labor, lasting forty-eight hrs. Following the delivery of the placenta there was considerable post-partum hemorrhage. The following day the uterus was still soft, and the fundus reached as high as the umbilicus. Pain and tenderness were complained of in the right leg, and were followed by swelling, and finally by the development of phlegmasia in both extremities. By the 20th inst. the pain and swelling were mostly confined to the right trochanter. As fluctuation was detected the patient was transferred to Med. Ward 18, with directions to make a free incision to let out the pus. Upwards of three pints of fluid were evacuated. The patient eventually succumbed to the protracted suppuration which followed. The *autopsy* revealed nothing to account for the lesion. The veins were pervious and healthy. There were no traces of pelvic cellulitis or peritonitis. The uterus was small, and normal in appearance. The lymphatics were unaffected. Whatever the process may have been which led to the phlegmasia, it left nothing behind to stamp its character.

CASE 5 had no trouble except that the uterus did not contract down well.

CASE 6 had no trouble.

CASE 7 had a slight attack of cellulitis, but made a good recovery.

CASE 8. Kate M., married, æt. 21, second preg., confined March 7th, had post-partum hemorrhage. A recurrence of hemorrhage took place March 12th, when the uterus was found distended by clots and reaching to the umbilicus. A half-basinful of clots were removed from the vagina and uterus by hand. The patient, however, was discharged well April 2d.

CASE 9 had some pain on third day, with profuse lochia, slight fever, and some diarrhœa. Her condition was aggravated by the fact of her fretting greatly over the loss of her baby, which she gave away for adoption.

CASE 10. Julia P., single, æt. 20, primip., confined March 8th, was of unusual interest. Two years previously she had had syphilitic sores upon the inner surface of the labia, for which she had been treated at Charity Hospital. On admission a hard syphilitic indentation was found, involving the labia, perineum, and lower part of the vagina. The inguinal glands were enlarged, and there was a worm-eaten ulcer on the right leg. The first stage of labor lasted twenty-nine hrs., the second stage seven hrs. thirty m., when I delivered the child with forceps, rupturing to some extent the indurated perineum. There was some post-partum hemorrhage. On the day following, on making the afternoon visit, I found the respirations 40, the pulse 168, and the temperature 104°. There was pain in the abdomen, the patient was delirious, and on examination a large diphtheritic patch was found on the vulva. This I ordered to be cauterized with *aa tinct. iodi comp.* and *liq. fer. persulph.* The next day the more serious symptoms subsided. The ulcer gradually cleaned up and the patient recovered. Curiously enough, in one little point not reached by the caustic, a hole was eaten completely through the labium internum.

From this time onward diphtheritic ulcers in the genital passage became a feature common to nearly all the cases seriously affected. Following the patients in the order of confinement, these patches were found in Cases 11, 12, 14, and 15. In Cases 13, 16, and 17 the diphtheritic appearances were not observed, but in them all it was nearly impossible to procure contraction of the uterus. In Case 16, sixteen grains of ergotine were injected hypodermically before any effect was produced. In Case 17

ice alone produced any effect, pressure and ergotine failing completely.

In a report made to me by Dr. J. L. Perry, House Physician, and Dr. Robt. A. Murray, Senior Assistant, who were indefatigable in the performance of their duties during the month's service, they state regarding these and similar cases which occurred:

"We have often held the uterus for several hours, and in one case Dr. Perry was obliged to hold the uterus all night. Otherwise the uterus would rise above the umbilicus, and hemorrhage would occur. Dr. Perry found Squibb's ergot so powerless, even in  $\frac{3}{4}$  ss. doses, that he almost abandoned its use. Quinine, on the contrary, appeared to possess a marked efficacy.

CASE 18. Bridget B., single, æt. 28, second pregnancy, confined March 16th, presented some want of uterine contraction, and a few hours after confinement had a flushed face, dry tongue, sordes in the mouth, and a tympanitic abdomen. The next day a diphtheritic patch appeared on the posterior commissure, and the lochia became offensive. Three convulsions occurred on the 19th inst., and the patient became comatose. At 2 o'clock A.M. on the 20th, another convulsion occurred, and the patient died. At the autopsy the liver and spleen were found soft and congested; there was no peritonitis, but the internal surface of the uterus was coated with grayish diphtheritic sloughs, the inner surface of the uterus was torn and gangrenous, and large gangrenous patches coated the inner surface of the vagina. It was specially noted in this patient that she was, prior to, and following her confinement, the victim of intense terror for fear lest something should befall her.

CASE 19 had flaccidity of the uterus.

CASE 20 had moderate fever lasting three days.

CASE 21 had albuminous urine with granular casts, œdema of the labia, and convulsions following delivery. Three days later diphtheritic patches appeared on the right labium. By April 1st patient was convalescent, and sitting up.

There were thirty-six patients in all confined during March. Of the cases from No. 22 to 36, six had no trouble. In three cases the disturbance was very slight. In one case diphtheritic trouble was developed, and in the remaining four the principal features were profuse lochia, abdominal tenderness, sweating, chills, and

oftentimes retention of urine. They seemed to be mild cases of cellulitis and pelvic peritonitis, terminating in recovery after no very long period of time. One of these latter was a patient confined in a police station-house, and afterward brought to the hospital by ambulance. It was noticeable in her case that the serious symptoms, instead of breaking out upon the second or third day, were delayed until the ninth day after delivery. Thus of the women delivered during March, there were two deaths, one from phlegmasia and one from diphtheritic conditions of the genital organs.

During this month too high praise cannot be awarded to the untiring zeal and fidelity of Drs. Perry and Murray. So soon as premonitory symptoms were observed, such as a flushed face, a headache, a slight chill, a change in the lochia, pain in the abdomen, everything was done to anticipate the threatening storm. Where patches appeared upon the vulva, quinine was given in from ten to twenty grs. per dose. The patients were syringed every three hours with carbolized water. Drs. Perry and Murray state: "Every patient had her own catheter and syringe, which were put in carbolic acid after being used. No sponges were allowed. The patches were touched with a mixture of iodine and persulphate of iron. To be sure that the patients were properly cared for, all those who had high temperatures, tender abdomen, or lacerations, were cleaned, syringed, and had their urine drawn by ourselves. We likewise administered the medicines, as it was impossible to confide these important matters to ignorant and irresponsible nurses. The wards were visited night and day every two or three hours by one or both of us, and for most part of the time one of us remained constantly in the ward. Bed and bedding were changed either by our own hands or in our presence, to be sure that the patient was comfortable. . . . All rags used were destroyed. Nothing used on one patient was employed upon a second, if possible. As far as could be, new cases were put in fresh, clean beds. So many critical cases soiled a great deal of linen, and the amounts which were found ordinarily sufficient proved inadequate for the emergency. Napkins and blankets therefore gave out, and for a little while blankets which had been used over cases with high temperatures had to be used for new cases. The warden, Mr. Brennan, and the matron

Mrs. Riddle, however, speedily furnished a new supply of napkins and blankets. . . .

"When the puerperal cases were at their worst, only one of us attended them, while the other attended the confinements. Our hands were washed in carbolic acid and glycerine, or Labarraque's solution, before going from one infected case to another, and before confining any case. Carbolic acid was sprinkled over the floor. The patients were warmly covered, and the windows let down several inches. . . . We constantly felt that there were too many patients in each ward. Patients with high temperatures were often in the ward into which a freshly confined case had to be brought, though we made it a point to send out all such cases when it was possible to procure places for them. The severer cases were transferred to the medical divisions. Had there been only four to six cases to each ward, our nurses could have taken much better care of them. . . . Owing to one of the nurses going away and becoming intoxicated, one of the wards was left for about ten days without a second nurse. The remaining one was civil and obliging, but had no memory or care. One of the waiting women had to be brought up to assist this nurse. The duties of the nurses were numerous, and no one could care for from twelve to sixteen beds when full, as was the case sometimes during the month. Sometimes six confinements took place in one day, and the amount of old rags, linen, and blankets needed was considerable, and even with the utmost economy a sudden demand would completely exhaust the supply."

Thus we have vividly presented to us a not exaggerated picture of the inadequacy of the provisions made for the obstetrical service of the Bellevue Hospital. It is important to dwell upon this subject. Such conditions ought never to exist again in any civilized community. It is cheap talk to blame the "tainted hospital," but by similar management it would have been possible to have made a pesthouse of the garden of Eden. It would be useless to ascribe the state of affairs to any particular individuals. The whole arrangement was radically wrong and vicious from the beginning. Any one familiar with hospital affairs is aware of the serious drain upon the pecuniary resources of an institution which results from small leakages. Where appropriations are limited, a wise economy cannot



afford to overlook losses liable to be thus incurred. For this reason everything had to be kept upon a peace, instead of a war footing. Thus space, ventilation, nursing, blankets, napkins, and all the armament in use, were amply sufficient for all ordinary emergencies. In times of peril, when new wants were suddenly created, the deficiencies became miserably apparent.

Three wards were assigned to lying-in patients. These contained an aggregate of 75,120 cubic feet of air. The average number of patients at one time in the wards was upwards of thirty-two, which would give about 2,348 cubic feet of space to each patient. The French system was adopted in the arrangement of the service. The first room was fitted up with *lits de misère*, beds upon which the patients were confined. Here the bedding was frequently changed, but the bedsteads never. After confinement the women were transferred to the adjoining room, where they remained for from four to five days. They were then transferred to the second ward, where they were kept until they were able to sit up. Finally they passed into the third ward, in which they continued until strong enough to be discharged. Now it is well known that nearly all puerperal troubles occur during the first five days after delivery. Past this period puerperal inflammations are rare. The granulation of wounds affords a barrier to the absorption of septic materials. The reduction of the uterus offers a safeguard against the dreaded affections of the veins and lymphatics. Common-sense policy would dictate, therefore, not the aggregation of the newly confined into one apartment, but either complete isolation, or their distribution among patients not liable to contamination. This is recommendable, not only on account of the vulnerability of the newly confined, but for the same reason that it is thought desirable to isolate or dilute the influence of surgical cases with open wounds.

The opposite policy prevailed at the Bellevue Hospital. The room assigned to fresh cases contained 15,970 cubic feet of space. The number of occupants averaged from six to eight. In the winter months, when the ventilation was imperfect, it was always merely a question of time as to when the sanguineous and purulent discharges of these patients would load the atmosphere with morbid elements to such a degree

as to exercise a deleterious influence. In this ward all puerperal disorders invariably had their origin. It was the rule, when a case of fever occurred, to transfer it at once to the medical side. In spite of this precaution a winter rarely passed without its becoming necessary to close the ward for a time, and subject it to thorough disinfection. During the panic of 1873-4 there were in this city large numbers of unemployed poor. As a consequence the hospital was overcrowded, and the transfer of obstetric cases to the medical wards became a matter of difficulty. In the interests of economy it was made the duty of the midwife attending cases of labor to likewise syringe and care for the patients in the adjoining room. Yet it is hardly possible to conceive of a more ingenious arrangement for keeping alive disease, when once generated, than this. A patient with fetid discharges is syringed out by the midwife, who perhaps a few moments after is called upon to cleanse the genitalia of a patient just confined. During the month of March the house physicians themselves did much of this sort of work, because of the difficulty of getting their injections carried out by others. They strove, however, to protect themselves from acting as carriers of contagion by incessant and thorough ablutions with disinfecting fluids. As bearing upon the question of the portability of the puerperal poisons, it is interesting to note that, while during the entire epidemic I spent upwards of one to two hours daily in the hospital, I had not a single bad symptom developed among patients whom I attended in private practice.

It is interesting here to inquire whether it was possible at this time to control the mischievous influences at work in the hospital. The following facts speak for themselves: From April 1, 1873, to April 1, 1874, 434 women were confined. Of these 23 died. Eight of these fatal cases were due to causes in no way referable to the condition of the hospital (pneumonia, phthisis, cerebral embolism, uræmia, etc.), leaving 15 cases of metria, or about 3.7 per cent. Now let us compare the Bellevue Hospital with the Lying-in Institution in Dresden for the years 1872-3. In the year 1872 there were in the latter 991 confinements and 52 deaths, a rate somewhat exceeding 5 per cent. During the first nine months the death-rate was 6.5 per cent., whereas it fell in the succeeding three months to 2.8

per cent. This favorable change was not due to the workings of a special grace, but to the energetic measures inaugurated by Winckel, who assumed the control in October, 1872. These measures were in substance as follows: The locality of the confinement ward was changed. Every patient was continued upon the bed in which she was confined. When removed to an adjoining ward the bed was transported with her. Each ward containing puerperal patients was emptied once a year in turn, and kept vacant for from three to four weeks. A distant wing of the hospital was set apart for erysipelatosus or other cases requiring to be separated from those who were in a normal state. An additional nurse relieved the midwife in attendance upon confinement cases from all charge of the puerperal patients. To determine the carriers of infection only one female pupil at a time was allowed to take charge of the confinements. So soon as a single case of fever occurred among the patients delivered by her, she was at once prevented from examining either pregnant or parturient women. Similar regulations were enforced in the case of the medical attendants.

In the year 1873 the month of January began badly. From the 1st to the 9th, inclusive, 26 patients were confined. All, without exception, were taken ill, and five, or 19 per cent., died. This formidable state of affairs was coincident with a change in the staff of nurses. Whereupon the following additional orders were issued: Examinations of parturient women to be forbidden to all those who had previously been in the habit of making them. Removal of the midwife in charge of the labor cases. The pupils and midwives were forbidden to wash the genitalia of the puerperal women, but the latter were compelled to perform their own ablutions. All the instruments and apparatus in the lying-in ward were either destroyed or subjected to white heat. Each patient was provided with her own catheter and injection tube. Temperatures were taken in the axilla, in place of the vagina. The ulcerations about the vulva and vagina were touched with the liq. fer. perchlorid. This duty was performed by Winckel himself. As a result of the enforcement of these rules, from the 10th of January to the 7th of July, out of 510 births there were but three deaths. But of these three, one was due to rupture of the uterus, one to nephri-

tis, and one only, or 0.2 per cent., took place from metria, a result scarcely possible in a similar number of confinements among the poorer classes in their homes. In the months of August and September six patients died. At the end of October and the beginning of November four died. In both these small epidemics the communication of the disease was traced directly to the midwives in charge. They ceased the moment the removal of the latter was secured. The year 1873 witnessed 1,011 confinements, and 18 deaths from metria, or 1.8 per cent.

In referring to the reforms introduced into the hospital management, Winckel states: "Although these changes increased the annual expenditures, not hundreds, but thousands of dollars, they were at once allowed without red tape (ohne Umstände), and in the shortest possible time, by the Royal Department of Internal Affairs." A request under similar circumstances for an addition to the force of nurses at Bellevue Hospital, with increase of pay, and consequent improvement in the quality, was refused. Is it too much to hope, that a time may yet come, when an intelligent regard for great trusts may likewise govern the actions of those, who administer the "government of the people, by the people, for the people"?

However, relief was promised from another source. The State Charities' Aid Association offered to supply the Lying-in Department with competent workers from the "Training School for Nurses," which was an outgrowth from their organization. This offer I would gladly have accepted. Unfortunately, however, a dispute arose between the members of the Medical Board and the managers of the Training School, regarding certain points of jurisdiction. As a result of this controversy the acceptance of the offer made by the State Aid Association was delayed until the 1st of May. Meantime a rumor of coming changes reached the ears of the nurses at the time on duty. The most complete demoralization at once ensued. The small sense of responsibility which had previously adhered to them now departed. The tables furnished for April show the further development of the pestilence.

Out of 35 cases confined there were 27 cases of puerperal disease and 9 deaths, leaving thus 8 cases only which present-

Number.	Date.	Age.	Social Condition.	Number of Pregnancy	Duration of Labor.		Character of Labor.	Diseases.	REMARKS.
					First Stage (hours).	Second Stage (hours).			
1	1st	28	M.	3	.....	.....	.....	Cellulitis.	MARY Y——. Disease terminated by abscess formation, which discharged through Douglas's cul-de-sac into vagina, April 27th, whose symptoms were at once relieved.
2	1st	20	S.	1	.....	.....	.....	Cellulitis.	JESSIE S——. Cellulitis of mild type. Patient discharged well, April 27th.
3	3d	22	M.	1	47	2	.....	Slight endometritis.	KATE M——. Up April 10th.
4	4th	32	M.	3	.....	.....	.....	Pelvic peritonitis.	MARGARET C——. Chill second day. Severe pelvic peritonitis, with formation of pelvic tumor in right iliac fossa. Phlebitis of right leg.
7	6th	28	S.	1	30	5	.....	Slight cellulitis.	ELIZA MC——. Patient was sitting up on tenth day.
13	8th	20	S.	1	.....	.....	.....	Diphtheritis of vulva.	EMMA R——. Up on the 19th of April. Treated with tinct. iodini co. and liq. ferri persulph.
16	12th	23	S.	1	.....	.....	For- ceps.	Diphtheritis of vulva.	CHRISTINI A——. Second day R. 44, P. 144, T. 105° Fahr. No pain nor abdominal tenderness. Recovery under local treatment.
17	13th	27	Wid	1	.....	.....	.....	Cellulitis and pulmonary embolism.	CHRISTINA O——. Discharged well April 30th.
19	14th	40	M.	7	.....	.....	.....	Puerperal rheumatism.	SARAH D——. Symptoms of cellulitis, beginning with chill on the third day. Up April 27th.
22	15th	22	S.	2	55	30	.....	Bright's disease.	MARY R——. The kidney affection complicated pregnancy, which it antedated; no convulsions.
23	15th	33	M.	4	53 $\frac{5}{6}$ 6 0	5	.....	Diphtheritis of vulva.	MARY M——. Offensive lochia. Albuminous urine. Discharged well May 10th.
25	19th	25	S.	2	33 $\frac{3}{6}$ 6 0	44 $\frac{4}{6}$ 2 0	.....	Slight cellulitis.	ANNIE T——.
27	23d	25	M.	1	7 $\frac{7}{6}$ 0 0	2 0	.....	Diphtheritis of vulva.	SARAH C——. Septic symptoms. Local treatment. Verat. viride, quinine, whiskey.
30	26th	24	S.	1	15 $\frac{9}{6}$ 0 0	16 $\frac{6}{6}$ 0 0	.....	Cellulitis.	ALICE U——. Discharged May 27th.
33	28th	22	S.	1	.....	.....	.....	Diphtheritic patch on vulva.	on MARY S——. Long septic fever, terminating in recovery.
34	30th	24	M.	2	.....	.....	.....	Cellulitis.	MARY P——. Hour-glass contraction.
35	30th	34	M.	4	.....	.....	.....	Mild septic fever.	BRIDGET H——.



ed no unfavorable symptoms<sup>1</sup> (Nos. 6, 8, 9, 10, 12, 15, 20, 26). On the 1st of May the managers of the Training School were invited to take charge of the nursing of the Lying-in Ward. Up to this time the obstetrical service occupied the upper story of one wing of the hospital. It was now removed, on suggestion of Warden Brennan, to the story immediately below, which had always enjoyed an excellent sanitary reputation. It should, however, be stated, that this floor, like all the other Medical Divisions of the hospital, had received during the epidemic, a number of cases of puerperal fever. A lazaretto was established in a remote part of the hospital for the reception of cases who manifested symptoms of a threatening character. It was expected at the time of this removal that the nursing would pass at once into the hands of the probationers of the Training School. Unfortunately, however, the managers of the Training School were not prepared to assume at once full charge, on account of the deficiency in the numbers of the pupils under their control. Their arrangements were not completed until the 20th of May. Two nurses were, however, detailed from the force on the 1st of May, to aid, so far as lay in their power, in the performance of the very onerous duties of the department. But even this inadequate change was not without some beneficial result. Thirty-five cases were confined during the month. There were 17 cases of disease, with 10 deaths.

But there were eighteen patients who had no bad symptoms, whereas in the previous month, with the same number of confinements, only eight had a similar good fortune. It is surely not at all impossible that, could a complete change in the attendants have been made on the 1st of May, a continuance of these disasters might have been averted.

In a Report made by me to the Committee of Inspection of the Hospital in the early part of June, I furnished the following statement of the measures adopted, under my direction, with a view to checking the epidemic: "The wards have been changed; a lavish use of carbolic acid and Labarraque's solution enjoined; oakum, as recommended by Dr. Goodell, has

<sup>1</sup> Regarding the fate of one patient, Lizzie F., æt. 24, single, primip., I have been unable to obtain definite information. She had an attack of peritonitis, was very ill, but I find nowhere any record of her death.

Number.	Date.	Age.	Social Condition.	Number of Pregnancy.	Duration of Labor.		Character of Labor.	Causes of Death.	REMARKS.
					First Stage (hours).	Second Stage (hours).			
5	4th	25	S.	2	10	7 <sup>50</sup> <sub>00</sub>	For- ceps.	Peritonitis.	JULIA S.— Peritonitis developed second day. Death took place on the 8th inst. (fourth day). <i>Autopsy</i> : Decomposed membranes in uterus. Usual post-mortem appearances of peritonitis in the abdomen.
11	7th	16	S.	1	.....	.....	.....	Peritonitis.	MARY L.— Chill on second day. Repeated chills. <i>Autopsy</i> : Uterine phlebitis. Pus in cellular tissue about uterus. Pus in ovaries. Ovaries size of pigeon's eggs. Thrombus in ovarian veins.
14	12th	20	M.	1	28 <sup>30</sup> <sub>00</sub>	21 <sup>00</sup> <sub>00</sub>	.....	Peritonitis.	ANNIE S.— Developed on second day. Died April 21st.
18	13th	19	M.	1	25 <sup>15</sup> <sub>00</sub>	1 <sup>00</sup> <sub>00</sub>	.....	.....	MARY J. R.— Transverse presentation. Delivered by one of the hospital internes. Died from exhaustion sixteen hours after.
21	14th	19	Wid.	1	.....	.....	.....	Peritonitis.	MARY MCE.— Chill on fourth day. Erysipelatous inflammation over right natis and hip on the 27th of April. Lochia very offensive and scanty.
24	15th	35	Wid.	5	38 <sup>55</sup> <sub>00</sub>	5 <sup>00</sup> <sub>00</sub>	.....	Peritonitis.	KATE C.— Old rupture of perineum. Vulva gaping. Symptoms of intense septicæmia. <i>Autopsy</i> showed large flabby uterus, cervix lacerated, parametritis, endometritis, and peritonitis.
28	24th	31	M.	5	7 <sup>30</sup> <sub>00</sub>	1 <sup>00</sup> <sub>00</sub>	.....	Peritonitis.	THERESA F.— Uterus large and flabby. Lochia scant and offensive. Died May 6th.
29	25th	28	M.	1	24	5 <sup>40</sup> <sub>00</sub>	.....	Peritonitis and diptheritis of vulva.	ANNIE C.— Died April 30th. Both pleural cavities contained serum and fibrine. Kidneys white. Uterus and ovaries covered with purulent exudation.
32	27th	22	S.	2	16 <sup>20</sup> <sub>00</sub>	1 <sup>50</sup> <sub>00</sub>	.....	Diphtheritic appearance of vulva, peritonitis.	MARY D.— Died April 29th. <i>Autopsy</i> : Uterus eight inches long. Pus in lymphatics of cervix and pelvic cellular tissue. Sero-purulent œdema. Lymph glands enlarged.

been substituted for napkins ; in syringing, each patient has a glass tube assigned for individual use ; the nurse in attendance during labor is not allowed in any way to come in contact with the recently confined ; the windows are constantly kept widely open ; and the physicians in charge are fully alive to the importance of promptly removing from their supervision any patient who presents symptoms in anywise suspicious." There was, however, one fatal defect, viz., with these precautions, a continuance of nurses who had attended infected cases.

I shall not attempt to enter into a special description of the character of the epidemic. This work has already been performed with great ability by Dr. Parry, of Philadelphia, who witnessed one presenting similar features in the Philadelphia Hospital.

The diphtheritic nature of the grayish deposit upon lacerations about the vulva and vagina is unquestionable. Dr. Steurer, who was my House Physician during the month of May, and is at present a pupil of Prof. von Recklinghausen, of Strasburg, writes that he, with the assistance of Prof. R., has since been working up the microscopical appearances of the deposit in an epidemic which has recently occurred in Strasburg. "We found it," he says, "a true diphtheria. Micrococci were found beneath the deposit, and scattered throughout the tissues of the uterus, whence they were taken up by the sinuses, and conveyed into the circulatory system. They may be found in the muscular structure of the vulva, and always occur in colonies. They are sometimes found in the blood-vessels of the kidney, distending whole glomeruli."

In my own experience prompt canterization with strong carbolic acid, or the application of Churchill's tincture to the diphtheritic membrane, was often followed by a surprising amelioration of symptoms which had assumed an alarming character.

In the Report to which I have already alluded, I felt called upon to represent the necessity of at once breaking up the obstetric service of the hospital, and vacating the wards for the time being. This report received the endorsement of the Medical Board, and, in consequence, the closure took place by order of the Commissioners of Public Charities and Correction, on the 11th of June.

of *Puerperal Fever*.

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Number.	Date.	Age.	Social Condition.	Number of Pregnancy.	Duration of Labor.		Character of Labor.	Diseases.	REMARKS.
					First Stage (hours).	Second Stage (hours).			
3	1st	30	M.	4	20	$\frac{5}{60}$	.....	Pelvic peritonitis.	MAGGIE McR——. Pelvic peritonitis. Offensive discharges. Left hospital May 22d.
9	7th	25	M.	2	$3\frac{45}{60}$	$\frac{5}{60}$	.....	Moderate pelvic peritonitis.	BRIDGET S——. Patient epileptic. Well May 23d.
12	9th	18	M.	1	$18\frac{45}{60}$	$\frac{55}{60}$	.....	Diphtheria of vulva.	ALICE M——. Relaxation of uterus three-quarters of an hour after delivery. Ice employed. Diphtheritic condition of vulva. Local treatment and quinine.
18	13th	35	M.	8	7	$\frac{30}{60}$	.....	Pelvic abscess.	MARY McG——. Uterus had to be held for five hours after delivery. Patient entered hospital three weeks previous to confinement with delirium tremens.
20	16th	26	S.	1	$4\frac{10}{60}$	$\frac{57}{60}$	.....	Diphtheria of vulva.	ELLEN O'D——. Uterus was large. Patient suffered much from cystitis.
25	19th	26	M.	1	$19\frac{30}{60}$	$1\frac{34}{60}$	.....	Diphtheria of vagina and vulva.	ELLEN S——.
28	25th	21	M.	1	$21\frac{30}{60}$	$2\frac{36}{60}$	.....	Diphtheria of vulva.	JENNIE S——. Uterus large and flabby.

CASES IN *May* TERMINATING IN *Death*.—No. 4.

Number.	Date.	Age.	Social Condition.	Number of Pregnancy.	Duration of Labor.		Character of Labor.	Causes of Death.	REMARKS.
					First Stage (hours).	Second Stage (hours).			
1	1st	18	M.	1	12	4 $\frac{5}{6}$ 00	.....	Peritonitis & septicæmia.	BELLA M——. Breech case. Chill on the second day. Delirious on the third day, and on the fourth day death.
4	2d	20	S.	1	26 $\frac{2}{6}$ 00	1	.....	.....	MARIAN WATERS.
10	8th	21	S.	1	11 $\frac{2}{6}$ 00	1 $\frac{0}{6}$ 00	.....	.....	ANNIE SMITH.
14	11th	24	S.	2	.....	.....	Twins.	Peritonitis and diphtheritis of vulva.	ELLEN M——. Precipitate labor (first child born in water-closet; second child inside of thirty-five minutes from first labor pains). Uterus failed to contract. Ice, ergot, battery used for three hours. Twelve hours later hemorrhage recurred, and was stopped by pressure and ergot. On visiting the patient I drew from the bladder sixty-four ounces of urine.
21	17th	20	M.	1	16 $\frac{3}{6}$ 00	12 $\frac{0}{6}$ 00	.....	Diphtheria of vulva.	ANNIE M——. Attended a patient who died of puerperal fever. Felt badly ten days before confinement
22	18th	25	M.	2	5	4 $\frac{0}{6}$ 00	.....	Diphtheria of vulva.	MAGGIE N——. Died May 23d.
29	26th	21	S.	1	24 $\frac{3}{6}$ 00	4 $\frac{5}{6}$ 00	.....	Diphtheria of vulva.	KATE M——. Relaxation of uterus. Uterus held for twenty-four hours after confinement.
30	26th	25		2	27 $\frac{4}{6}$ 00	3 $\frac{5}{6}$ 00	.....	Diphtheria of vulva.	MARY T——. Relaxation of uterus.
34	27th	19	S.	1	62 $\frac{0}{6}$ 00	2 $\frac{5}{6}$ 00	.....	Diphtheria of vulva.	JENNIE G. D——.
35	28th	38	Wid.	1	51 $\frac{0}{6}$ 00	8 $\frac{0}{6}$ 00	.....	Peritonitis, lymphangitis, diphtheria.	CORNELIA S——. Autopsy: Uterus 9 × 5 inches. Cervix diphtheritic. Sinuses filled with soft thrombi. Pus in lymphatics, and enlargement of lymphatic glands.



CASES IN *June* TERMINATING IN *Death*.

Number.	Date.	Age.	Social Condition.	Pregnancy	Duration of Labor.		Character of Labor.	Diseases.	REMARKS.
					First Stage (hours).	Second Stage (hours).			
3	2d	20	S.	7	8 $\frac{3}{4}$ 00	1 $\frac{1}{2}$ 00	.....	Diphtheria of vulva and cervix uteri.	ELLEN T——. Perineum ruptured to sphincter ani, became covered with diphtheritic coat. With speculum was discovered diphtheritic condition of cervix.
5	6th	28	M.	3	28	1 $\frac{3}{4}$ 00	Breach.	.....	MARIA D——. Extraction by Dr. Taylor. Mental worry. Delirium. Swelling of right arm. Hypogastric pain, fever, etc. Discharged July 23d.
7	8th	28	M.	1	44	2 $\frac{1}{2}$ 00	Forceps.	Diphtheritic condition of perineum.	ALICE W——. Rupture of perineum (after removal of forceps), diphtheritic deposit on abraded surface. Abscess of forearm. Discharged July 23d.
9	11th	22	S.	1	17	4 $\frac{5}{8}$ 00			

Number.	Date.	Age.	Social Condition.	Pregnancy	Duration of Labor.		Character of Labor.	Causes of Death.	REMARKS.
					First Stage (hours).	Second Stage (hours).			
1	1st	25	M.	2	6	1 $\frac{1}{2}$ 00	.....	Metrophlebitis.	ALICE DE L'H——. Chill six hours after delivery. Uterus enlarged, five inches above pubis, and tender.
4	6th	25	M.	3	12	2 $\frac{1}{2}$ 00	.....	.....	ELIZA M——. Towards end swelling of right arm.
9	11th	22	S.	1	17	4 $\frac{5}{8}$ 00	.....	Diphtheritic condition of parts.	LENA J——. Lateral incisions made into vulva during delivery of head. The wounded surfaces became covered on the third day with diphtheritic deposit. Patient died June 23d, and post-mortem examination revealed pus in the peritoneal cavity, in the uterine sinuses, and at the junction of the broad ligament with the uterus.
10	11th	23	M.	1	24 $\frac{1}{2}$ 00	2	.....	Metrophlebitis and peritonitis.	NELLIE R——. June 17th, 9 A.M., P. 184, R. 32, T. 109 $\frac{1}{2}$ ° F. Autopsy showed uterus to be seven inches in length. Sinuses contained pus. Sero-purulent œdema of cellular tissue. Sero-pus in peritoneal cavity.

From the 1st to the 11th of June inclusive (Table 5) there were ten confinements with four deaths.

From the 1st of January to the 11th June, out of 166 patients confined there were thirty-one deaths.

A careful survey of all the facts recited seems to justify us in drawing two conclusions:

1st. That puerperal diseases may be engendered by the atmosphere alone. This is shown by the experience in September, 1873. Here febrile disturbances were developed among the patients of the ward, which at once subsided on removing them to another locality, though, in the new quarters, no changes were made either in the doctors, the nurses, nor in the utensils used. Nor was this an isolated experience. It owes its importance to the fact that the same thing had happened over and over again. The nature of the miasm can only be the subject of conjecture. The closure of the ward for the space of three or four weeks usually restored it to a healthy condition. The poisoned atmosphere invariably was generated in the ward set apart for the newly confined, and, as a rule, it rarely manifested any special virulence except during the winter months, when the ventilation became imperfect. At a time, when puerperal fever raged with greatest intensity, the deposit of diphtheritic membranes, characterized by colonies of micrococci, upon all lesions about the genital organs, seems to point to a parasitic origin.

2d. That, in distinction from the above, there is a form of puerperal fever possessing eminently contagious properties, not primarily derived from a miasm, but capable in time of generating a poisoned atmosphere. In support of this proposition we find that a patient, at a time when the general health of the ward was good, was suddenly attacked by a fever, presenting symptoms of intense severity. This patient died in a few days, and the autopsy revealed no local lesions. From the time of her attack onward, there was no period when puerperal diseases failed upon the obstetrical service. Removal of the patients from the ward in which she was attacked, a change in the medical staff, and the utmost precaution regarding the use of utensils, were of no avail. Through the agency of the nurses the disease was kept alive and active.

After the epidemic had prevailed for a season, it became

characterized by the formation of diplitheric membranes, usually upon the external genitalia. At first prompt cauterization of the affected parts possessed a surprising influence in arresting symptoms that seemed to point to an inevitably fatal result. However, the occurrence of symptoms, preceding the completion of labor, shows that the poison found other channels of entry into the economy than the lesions resulting from childbirth. At first this miasmatic condition was not sufficient to produce fatal results. Subsequently, however, it reached a stage of development in which it alone rapidly produced death. Cauterization of the local lesions then ceased to possess any marked efficacy.

Worthy of being remembered is the following fact: That three months later, the obstetrical wards were occupied by the surgical service of Dr. Jas. R. Wood, and that in the year's time that has since elapsed, though there have been many capital operations, not one case of septicaemia or pyaemia has occurred in that section of the "tainted" hospital.

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#### THE INJECTION OF FLUIDS THROUGH THE ABDOMINAL WALLS INTO THE LARGE INTESTINE BY MEANS OF THE TROCAR.

By JAMES R. CHADWICK, M.D., OF BOSTON.

(Read before the N. Y. Obstetrical Society, Nov. 2, 1875.)

WHEN reporting, recently,<sup>1</sup> a case of ovariectomy, in which the intestines had been punctured by the aspirator for the relief of distressing flatulence, I suggested the feasibility of injecting brandy, beef-tea, or other fluids, through the canula into the small or large intestine, after the escape of the gas. By this means it seemed to me that a patient's strength might be supported when nothing could be retained in the stomach, and absorption from the rectum was too slow to meet the demands of the system. In this way I thought that fluids could be introduced in considerable quantities into that part of the alimentary

<sup>1</sup> Boston Medical and Surgical Journal, July 22, 1875.

canal from which they would be most readily absorbed. In the same report I raised the question whether peristaltic action might not also be excited by the injection of suitable liquids, by which means a second and equally important indication would be met.

As the opportunities of testing a new procedure like the above must naturally be rare in the practice of one individual, I venture to bring before this Society the result of my first experience—meagre and unsatisfactory though it be—in the hope that others may thus be stimulated to test the value of this measure, should the proper emergency arise.

The case was one of peritonitis following the delivery by version of a 9–10 lb. boy, after a labor complicated by the presence of four large fibroids in the uterus, and of placenta prævia. When the abdominal distention by flatulence became so great as seriously to embarrass the respiration, I introduced the smallest trocar of my Potin's aspirator through the abdominal walls into what appeared to be the transverse colon, or sigmoid flexure. A rubber tube was affixed to the canula, and its other extremity carried into a basin of water, so that the escape of gas could be noted by the steady rise of bubbles from the water. After quite an amount of flatus had escaped, but before there was any sign of its cessation, I pinched the tube, inserted into its end the nozzle of a syringe, and essayed to inject beef tea into the intestine. For some reason—probably because the minute lumen of the canula became plugged with the shreds of tissue which were seen floating in the fluid—the attempt utterly failed, though persisted in for quite a while. Not wishing to take the time requisite to strain the liquid, because of the mental agitation of the patient, I made a solution of one part of very choice rye whiskey in three parts of water, and proceeded to inject it through the canula into the intestine. In this I was successful, but the very first drops of the fluid gave the woman so intense a colic—referred to a spot just below the point of injection—that I stopped at once for fear lest the intestinal wall had dropped off the end of the canula, and allowed the whiskey to enter the peritoneal cavity. As the pain soon subsided, I injected again with like effect, though the pain was less acute, the sensation being rather that of burning. Not deeming it right to subject the patient to further suffering,

I then desisted, having first satisfied myself that the canula had not escaped from the intestine by allowing more gas to pass off before withdrawing the instrument. Of this fact there was no doubt, either in my mind or in that of Dr. A. D. Sinclair, who rendered me most efficient assistance.

In reflecting recently upon the likelihood of such an occurrence as that just referred to, I have gradually been driven to the conclusion that this mishap is hardly possible, owing to the anatomical relations of the peritonæum. For all practical purposes (the openings of the Fallopian tubes may be disregarded), the peritonæum is a closed sack, which, in a state of health, is entirely empty; consequently we cannot conceive of the opposed layers, as represented by the intestinal, visceral, abdominal, etc., falling apart, because such a phenomenon would necessitate the formation of a vacuum between them. This being contrary to nature, the only remaining inference is, that when the gas escapes from a coil of intestine, either the subjacent convolutions must rise to fill the space previously occupied by the one that has been emptied, or the abdominal walls must contract so as proportionately to diminish the capacity of the abdominal cavity. In either eventuality, the coil evacuated must collapse between the abdominal walls and the underlying intestines. The point of the canula is not then in danger of emerging into the cavity of the peritonæum, but rather of penetrating the walls of the next intestinal coil, provided it is sufficiently sharp. In the case under discussion, the canula was blunt and unquestionably remained in that portion of the colon into which it was originally plunged.

It will be seen that, in the above remarks, I have assumed the absence of gas within the cavity of the peritonæum; this is justified by analogy with the other serous cavities (plenra, etc.), and by the silence of all anatomists on this subject. The question is an important one, with bearings upon many mooted points. For instance, the presence or absence of intestines in Douglass' pouch has been often discussed; if my view, however, is correct, this duplicature of the peritonæum cannot be void, and consequently the space commonly assigned to it in the illustrations of our text-books must in reality be alternately invaded by the uterus, bladder, rectum, and intestines, according as one or another of these organs is distended by solid, liquid,



or gaseous contents. It seems to me not improbable, that the normal development of distensible gases within the intestines is partly designed to prevent the painful suction that would otherwise be exerted upon the walls of the peritoneal cavity—notably of the abdomen—to fill the vacuum formed by the emptying of one or all of the viscera.

An attempt to obtain corroboration of the views here expressed is met at the outset by the serious obstacle, that the condition of the peritoneal cavity is completely altered when it is opened post-mortem for examination. A reference, however, to the superb plates representing sections of frozen cadavera, published by Branne, shows in every instance the correctness of my conclusions. In Branne's plates, as well as in the numerous illustrations taken by him from the works of Le Gendre and Pirogoff, no spaces are represented in the peritoneal cavity between the uterus, bladder, intestines, etc., and the walls that enclose them. In several of the descriptions it is expressly stated that no such spaces existed.

To return from this digression to the case under discussion, one thing is at all events certain, that in peritonitis the intestines would inevitably be glued to the abdominal walls and to each other by the effused lymph, and thus such a mishap as was feared be prevented. Perhaps this pathological condition may account for the harmlessness of puncturing the intestines, although the contraction of the muscular walls of the intestine would probably suffice to close the minute aperture made by a trocar, as is certainly the case with the bladder.

The woman continued to complain of intermittent lancinating pains running down to the groin, and clearly due to peristaltic action. To relieve the suffering, and arrest the action of the bowels, which was undesirable in view of the peritoneal inflammation, I administered a quarter of a grain of morphia subcutaneously and left the room. In ten minutes I was summoned by the patient's complaint "that something was about to pass from the bowels." Examination with the finger demonstrated the presence of two large scybalæ in the rectum, which had previously been empty, although not even flatus had escaped for nearly twelve hours before. The woman died of septicæmia two days later. No antopsy was allowed.

My aim in the above procedure was to introduce nutrient or

stimulant fluids into the intestine, whence I hoped they would be absorbed so as to support the patient's strength, which object could not be satisfactorily attained per vias naturales, owing to the constant vomiting, and to the fact that enemata could not be forced above the lower three inches of the rectum, from which very little was likely to be taken up. The constriction of the rectum was probably due to the pressure of the large group of fibroids contained in the uterus. I entertained the hope that, if my patient could be sufficiently nourished, she might survive the peritonitis, which did not seem very intense or diffuse. In this scheme I was accidentally foiled, but should not be deterred from trying the same plan again with properly strained beef-tea or a much more dilute alcoholic stimulant.

As a means of exciting peristaltic action, my procedure was eminently successful. The extreme sensitiveness of the intestinal mucous membrane to the contact of an alcoholic fluid so dilute as not to produce any appreciable astringent sensation in the mouth or rectum, is particularly worthy of note. In lieu of any other data, my experience points at alcoholic solutions of varying strength as suitable for this purpose. Future experiments will, I trust, demonstrate that this method is capable of being utilized in the treatment of very obstinate cases of intestinal torpor, faecal impaction, and the like. In the latter condition it may perhaps be found, that the hard scybalous masses can be softened and broken up by the injection of different fluids into their midst, when enemata and purges have proved powerless.

If this use of the trocar to facilitate the escape of intestinal gases is as harmless as is claimed by those who have tried it, I see no reason why the intestines should not be repeatedly tapped for the injection of fluids in many desperate conditions and diseases, when the ordinary resources of medical art have failed.

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## ON DERMOID CYSTS OF THE OVARY.

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By the term dermoid cysts we designate, dating from the investigations of Lebert,<sup>1</sup> those cysts which contain one or more of the constituent parts of the derma or integument, and in which, besides, cartilage, bone, even perfectly developed nerves and smooth muscular fibres—the latter, however, only very rarely—are also observed.

Lebert deserves the credit of having chosen this term as the shortest and most expressive, not, however, as is contended by many, of having been the first to use it. Leblanc, in a paper published in 1831,<sup>2</sup> already speaks of a "*kyste dermoïd*" at the basis cranii of a horse.<sup>3</sup> Kohlrusch in 1843<sup>4</sup> furnished us with an exceedingly exact description of the microscopic appearance of structures of this kind, referring especially to their analogy with the skin; and Steinlin, of Zurich, in an excellent paper published in 1850,<sup>5</sup> to which we shall refer further on, says verbatim as follows: "The peculiar change in the primary constitution of these cysto-lipomatous tumors of the ovary consists in the development of a structure in every respect identical with that of the skin, the contents of the cysts also corresponding to the secretions of the skin and its epithelial appendages." From this declaration to the name itself Steinlin had but one step to take.

<sup>1</sup> Mémoires lus à la soc. de biologie pendant 1852. Paris, 1853, T. IV., p. 203. In 1858 Lebert published an extract from this, his first paper, with several additions, in the Prager Vierteljahrschrift für prakt. Heilkunde, Vol. 60, p. 25. The other quotations from Lebert are taken from the first paper.

<sup>2</sup> Leblanc, sur un kyste dermoïd du crâne, Journal de Méd. vétérinaire, T. II., p. 23.

<sup>3</sup> The specimen is reproduced in Lebert's Path.-Anat. Atlas, plate 39, Fig. 1.

<sup>4</sup> Müller's Archiv, 1843, p. 365.

<sup>5</sup> Zeitschr. für rat. Medizin herausg. von Henle und Pfeufer. Heidelberg, 1850, Bd. IX., p. 155.

The dermoid cysts generally possess thin membranous septa. Their internal surface is usually smooth, but occasionally rendered uneven by distinct prominences,<sup>1</sup> and presents either a universal or only a partial cutis-like appearance.

The innermost coat of the lining membrane consists of thick layers of tessellated epithelium,<sup>2</sup> first of squamous elements without nuclei, followed by nucleated and finally spheroid cells, exactly as in the integument. Under this is seen a layer of cellular tissue like that of the cutis, the surface of which, bordering on the epidermis, is occasionally<sup>3</sup> covered with papillæ, which, however, never possess the regular arrangement in rows, and which generally differ exceedingly as regards length and thickness. Below this layer the cellular tissue is loose, and its fibrillæ separate here and there for the reception of adipose matter; a distinct, often quite extensive, layer of fat (*panniculus adiposus*) is thus seen to succeed the cutis.

In these tissues completely identical with the skin we often see all the attributes of the latter developed in considerable quantity. The dermoid area is first usually covered with hair, the roots of which spring from distinct follicles, into which *sebaceous glands* open, generally near the mouth of the follicle. Besides these enormous sebaceous glands associated with the hair-follicles, which glands measure  $\frac{1}{3}$  to  $\frac{1}{2}$ ''', occasionally 1''' and more in length, there are found others opening directly on the internal surface of the cyst. Besides the sebaceous

<sup>1</sup> Some of these prominences may even be found to have attained the shape of actual papillæ.

<sup>2</sup> Ciliary epithelium has hitherto been found only in a few cases. Compare Virchow's Archiv, XI. 469, XIII. 498. Klebs, in his work on Pathological Anatomy, Berlin, 1873, p. 816, traces its origin to the ciliary epithelium of the endymatous canal of the spinal marrow.

<sup>3</sup> We are obliged here to contradict a statement of Spencer Wells (Die Krankheiten der Eierstöcke, Leipzig, 1874, p. 51), who states that papillæ are always found in dermoid cysts, and refer for our authority to Wagner (Allg. Path., 1874, p. 550), Klob (Path. Anat. der weibl. Sexual Org., 1864, p. 366), and others. We have also had occasion to satisfy ourselves personally of the fact by the inspection of a large quantity of carefully prepared microscopical preparations belonging to Dr. Haffter. Lebert, in his "Traité d'anat. path.," I., 258, says: "Plusieurs fois j'ai vu des papilles qui représentaient tous les caractères de celles de la peau." As none of these specimens are reproduced in his Atlas, a control of this "tous les caractères" is scarcely practicable. See Rindfleisch, Pathol. Gewebelehre, 1874, p. 484.

glands, *sudoriferous glands* are also met with. Since their first discovery by Kohlrausch,<sup>1</sup> Wagner, Heschl, Helfft, Lebert, Axel Key, Friedländer, Haffter, and others have seen them.

The walls of some cysts contain also cartilage or bone (the latter oftener than the former), or teeth, a few even vegetations of a warty or pachydermatous character. The cysts are filled with a soft grayish or yellowish mass, similar to butter or suet, in which entire balls of tangled hair are frequently found. Under the microscope the constituent parts are found to be free fat, discarded pavement epithelium, free nuclei, fatty epithelial cells, crystals of cholestearine, the latter occasionally in such numbers as to give a glittering appearance to the whole contents of the cyst.

The *hair* presents the most varied appearances; at times it resembles thin beard-hair, at others the ordinary hair of the head; frequently it is like lanugo; sometimes the hairs project free into the cavity; sometimes they are situated in the wall of the cyst, as mentioned above, seldom perpendicularly, generally growing out at an acute angle from the wall.<sup>2</sup> The color by no means always corresponds to that of the hair of the head or pubes of the individual; Andral found "in the ovary of a negress, who had black woolly hair, soft, smooth, red or blonde hair, and some of a silvery-white color, like that of white children."<sup>3</sup> Different colors have been observed, not only in the same cyst, but even in the same hair.<sup>4</sup>

The length of the hair varies exceedingly. The longest seen by Andral was barely 6" long, Regnaud met with some more than a foot in length, Busch with some 16" long (we also saw it of this length in a cyst removed by Professor Freund of Breslau), Thiebault 20", and Tyson even 2 $\frac{3}{4}$  feet

<sup>1</sup> In his above quoted paper he describes them as "unusually well developed; the coils of some of them  $\frac{1}{2}$ " in diameter, and the separate spirals not flattened out  $\frac{1}{12}$ — $\frac{1}{15}$ " broad." Friedländer (Virchow's Archiv, Bd. 56, p. 365) has also observed the considerable size and very abrupt spiral curve of the tubes. Others, such as Wagner (Archiv für phys. Heilkunde, 1857, p. 247), have found them entirely normal.

<sup>2</sup> See Wagner's classical treatise, l. c.

<sup>3</sup> Cfr. Busch, Der Geschlechtsleben des Weibes, Leipzig, 1843, IV., p. 329.

<sup>4</sup> Cruveilhier, in the text of the 18th edition of his Path. Anat. Atlas, says verbatim: "Le même poil peut affecter diverses nuances de coloration dans les différents points de sa longueur," cfr. Lebert, Traité d'anat. path., I., p. 257.



long. Lebert, in his Pathologico-Anatomical Atlas gives the drawing of a very fine tress of hair about one metre long, but says that he was able to disentangle only a single hair as long as 60 centimetres. Generally the hairs are short, one to two inches long or still shorter. The authorities agree, on the whole, that the hair found in dermoid ovarian cysts is generally of a blonde or reddish color. Frequently the hairs in the interior of the cyst have assumed the shape of matted curls. Thus Axel Key<sup>1</sup> at the autopsy of a woman 68 years of age, found a dermoid cyst of the ovary of the size of an adult head, which contained a curl of gray hair. In the museum of the Path.-Anat. Institute at Leipzig there is a specimen, the cavity of which is traversed by two bridges of skin, around one of which a coil of hair is wound.

*Teeth*, more or less developed, are not unfrequently found in ovarian dermoids. Lebert<sup>2</sup> found teeth recorded in 63 cases of the 129 dermoid ovarian cysts collected by him. Of 245 cases compiled by us, none of which are identical with those reported by Lebert,<sup>3</sup> teeth are expressly mentioned as a portion of the contents of the cyst in 46 cases.

A celebrated case is the one first described by Plouquet and later more accurately by Antenrieth in which "about 300 teeth were taken from the right ovary of a woman 22 years of age, who had never borne children; this number was counted, but comprised scarcely the half of those contained in the ovary, which weighed over 20 pounds," so says the report verbatim.<sup>4</sup> Over 100 teeth—the largest number observed thus far after the above case—were found by Schnabel<sup>5</sup> in a cyst belonging to a girl 13 years of age, who had not yet menstruated.

*Osseous tissue* has been met with both in the walls and in

<sup>1</sup> Hygieia, Bd. 26, p. 306, cfr. Schmidt's Jahrb., Bd. 126, p. 156.

<sup>2</sup> L. c., p. 244.

<sup>3</sup> The list of literature at the end of this paper (*omitted in the translation for want of space*, ED. and TRANSL.) shows that none of the cases included among these 245 numbers is identical with one of those minutely reported by Lebert on pp. 270-273; I am enabled, therefore, to draw my own conclusions from my cases without reference to those arrived at by Lebert.

<sup>4</sup> Reil and Antenrieth, Archiv für Physiologie, VII. Bd., Halle, 1807, p. 255.

<sup>5</sup> Kiwisch, Klin. Vortr. Prag., 1849, Bd. II., p. 176, cfr. Württemb. Corr. Blatt, 1844, XIV., No. 10.

the interior of the cavity. Heschl (Prager Viertelj.-Schr., 1860) describes several pieces of bone, which were connected by a false joint composed of periosteum and dense fibrous tissue. Besides the hair and teeth, the bones have furnished the best evidence for the vague theories of those observers who saw the remains of a fœtus in these inexplicable and heterogeneous formations, and the fancy of each writer has exhausted itself in seeking comparisons for these osseous fragments, which usually served as receptacles for the teeth and therefore showed alveolar openings.<sup>1</sup>

The osseous tissue is true bone, that is, it shows medullary canals and concentric lamellæ, but according to Klob,<sup>2</sup> the bone-corpuscles are somewhat scarcer and larger, and supplied with fewer processes than the normal ones.

Of not very frequent occurrence in dermoid cysts—indeed known only since the investigations of Steinlin<sup>3</sup> and Gray<sup>4</sup>—are *nerve-fibres* and *ganglion cells* with double outlines more or less similar to those in the brain.<sup>5</sup> Virchow (Deutsche Klinik, 1859, p. 197) in one case found nerve-substance “in strata similar to those of the cerebellum.”

Rokitansky observed an entirely separate nerve-plexus springing from a ganglion, together with a system of vessels, in a conoid osseous neoplasm covered with derma growing into the cavity of an ovarian cyst. The reddish ganglionic mass was

<sup>1</sup> Some authors make the most peculiar comparisons. Philipps in the Med.-Chir. Trans., VI., p. 124, 1815, reports the autopsy of a 2½ years' old child and says: “Several bones, among which the tibia and ossa tarsi, were plainly distinguishable, which latter were connected with the tibia by means of delicate cartilaginous ligaments.”

<sup>2</sup> Path. Anat. der weibl. Sexualorgane. Wien, 1864, p. 366.

<sup>3</sup> Steinlin, l. c., p. 150, says that he saw nerves enter the papillæ, but was unable to trace their terminal filaments.

<sup>4</sup> Gray (Lancet, 1853, Aug. 27, and Med.-Chir. Trans., Vol. 36) claims to have been the first to find nerve-tissue in a dermoid ovarian cyst. As we have just shown, Steinlin preceded him in discovering nerves in the cyst-walls. As regards Gray's statement, which refers also to nerve-substance in the interior of the cyst, we shall see below what it amounts to.

<sup>5</sup> The third cyst of the ovarian dermoid, which Gray found in the body of a woman, 28 years of age, who died on the sixth day of typhoid fever,—the whole tumor, of the size of an orange, consisted of five cysts enclosed in its cavity—contained “on microscopical examination nothing but nerve-substance,” the fifth cyst “nerve-substance and ganglion-cells, but less cerebral in their character, than in the above case.”

situated in a capsule in the wall of the cyst near the base of this osteoplasm. The nerve springing from this ganglion ramified in the osseous growth as far as its tip—similar to the digital nerves—and also sent a few branches to the cutaneous envelope. No less interesting is the report of an autopsy by Friedreich<sup>1</sup> of Würzburg, who found in the left ovary of a woman, 38 years of age, who died of Bright's disease, a tumor of the size of an apple, consisting of two cysts, one of which was a dermoid. Thick heavy bundles of broad double-outlined nerve-fibres on one side, on the other whitish masses of parallel, thin, varicose nerve-fibres, occasionally large uni- or bipolar, more or less pigmented ganglion-cells, containing immense nuclei, and of such a delicate construction as to be destroyed by the mere addition of water, intersected by a delicate capillary net and joined together by a fine, but distinct, neuroglia—such is the sum of Friedreich's report.

Among the later observers we find nerve-tissue mentioned as an element of ovarian dermoids by Axel Key<sup>2</sup> and Stilling.<sup>3</sup> We quote the case of Axel Key at length, because it offers many points of interest:

"At the autopsy of a woman 68 years of age, who had suffered from habitual constipation and had complained for six years of abdominal pain and periodical rigors, Key found a dermoid cyst of the ovary of the size of an adult head. The microscopical examination of the internal surface revealed a structure similar to the cutis, with the exception of the absence of papillæ; sebaceous and sudoriferous glands, however, were present. From the lateral internal wall projected a soft mass nearly 4" long, resembling a deformed fœtus, one side of which was convex and corresponded to the supposed dorsal surface. Bones, teeth, nervous tissue, were found in this tumor, which Key pronounced 'a freak of the formative power of nature.' The nerve-tissue was found in an osseous cavity which," says Key, "might be considered a sort of cranial cavity." By some authors, as for instance Spencer Wells, the observation of Chalice is placed in this category. Inasmuch as this case relates to the ovarian cyst of a young girl—about the structure of which cyst

<sup>1</sup> Virchow's Archiv, XIII., p. 498.

<sup>2</sup> Cfr. Schmidt's Jahrb., Bd. 126, p. 156.

<sup>3</sup> Deutsche Klinik, 1869, No. 8.

only the following is known: "the cavities of the cyst contained brain-like, soft, white and grayish-red masses"—serious doubts as to the nature of this tumor, especially as no microscopic investigation was made, may justly be entertained. With equal right we might accept the case of dermoid cyst reported by Alt,<sup>1</sup> who at the autopsy of his 10 year-old daughter found the right ovary "as large as a lemon and in its interior tarry blood, brain-like masses and a tress of shiny white hair, one inch long, surrounded by fat." But we believe that the word "brain-like," like encephaloid, is used by Chalice and others merely to designate the degree of consistency of the soft, fatty contents, and that we are not justified in considering these two cases in this paper.

We have quoted those cases with special minuteness in which the authors found nerves in ovarian dermoids, and recapitulate these statements to the effect, that the nerve-elements were always found in the processes projecting from the internal surface of the wall, or in that wall itself, and that Gray was the only one who found, as he says, "nerve-substance and ganglion cells" in the cavity itself.

Admitting that Gray examined the contents of that cyst microscopically, he can only have been either mistaken in his interpretation of what he saw—and this is our opinion—or he has discovered a mysterious condition, which for the present renders every embryological explanation of dermoid ovarian cysts impossible.

So long as no other reports similar to that of Gray, however, are furnished us, and so long as the doubts in that case are not cleared up by a more exact description than the one given by Gray, so long we are obliged to agree with Klebs, who, on page 813 of his text-book on Pathological Anatomy (1874), says: "Generally the gray nerve-tissue, as also the more rarely met with hollow nerve-fibres, form soft, grayish-red protuberances, which protrude into the cavity of the cyst, and are attached to the dermoid formations."

<sup>1</sup>Alt. in Schmidt's Jahrbuch, 16. Bd., p. 94. We might with equal propriety count in also the case reported in Schmucker's "Verm. Chir. Schriften," 1782, Bd. III., p. 330, where the unknown observer found "something similar to brain-substance" in a bony shell, which Himly in his "History of a Fœtus in Fœtu" already took for fœtal brain.

Most recent authors mention among the contents of dermoid cysts of the ovary, the presence of smooth and *striated* muscular fibres, and name Virchow as their authority for this statement. We have carefully studied the works of this great savant on this subject, whose authority alone would suffice to remove all doubts of the correctness of this unique statement (Verhandl. der Ges. f. Geburtshülfe in Berlin, III., 1848, p. 197, Verhandl. der physic. med. Ges. zu Würzburg, 1850, I., p. 189, and Deutsche Klinik, 1859, p. 197), and have found that he does, indeed, speak of striated muscular fibres, but in cysts not dermoid in character. In the second of the papers above quoted, Virchow even proposes the name of "myosarcoma" as suited to the tumor in question.<sup>1</sup>

*Smooth* muscular fibres, however, have been found by Virchow in dermoid ovarian cysts, and considered as *arrectores pili* (erectors of the hair).

The occurrence of *nails* we find mentioned in a general manner by only a few authors. Benda in his dissertation published 1844 (De Ovariorum Degeneratione, Berolini) writes: "Nails are also occasionally found in dermoid cysts." Lebert (Traité d'Anat. Path., I., p. 257) says: "Dans deux cas on y a observé une substance cornée, qui dans l'un des deux avait la forme d'étais, qui ressemblait à des ongles." To this observation of Lebert, Bauchet probably referred in his prize thesis in the Mém. de l'Acad. Impériale de Méd., XXI., Paris, 1857, although he does not directly mention it. Lebert's authority, again, is certainly Cruveilhier, who in the XVIII. edition, pl. V. fig. 3, gives the diagram of a dermoid cyst of the ovary, which represents "deux étais cornés"—two projections, more resembling small claws, a clear idea of which it is impossible to derive

<sup>1</sup> Although Spencer Wells accurately quotes this very microscopical report from Virchow's second paper, and mentions the word "myosarcoma," he still, strange to say, classes this tumor with the dermoid cysts, although it did not possess a single formation corresponding to the skin. Compare this with the statement made on this point by Rokitansky in his work on Path. Anat., 1855, I., p. 189, "The new formation of striated muscular fibres is reported to have been repeatedly observed; in every instance the correctness of the observation was open to doubt," etc. From this standpoint the dermoid cyst should also be regarded, which was exhibited in the Société Philomatique in 1825, and in which "on reconnut des os, des portions des muscles, et de la peau." This cyst is briefly mentioned by Parmentier in the Gaz. Méd., 1861, No. 1.



from the illustration. Galen, indeed, briefly mentions<sup>1</sup> nails as occurring in dermoid cysts; the passage is as follows: "Si quidem *ungium*, pilorum, ossium, testarum, lapidum, et pori fragmentorum similia inventa in his corpora quædam sunt."

Therefore, if nails are found at all, they must be considered an *exceedingly rare* occurrence.

Finally, let us mention, that in sheep wool, in birds feathers,<sup>2</sup> have been found in the interior of dermoid cysts.

Dermoid cysts are generally *unilocular*, or at most *only one* cyst of a multilocular tumor has undergone the dermoid transformation.

Combinations with serous or colloid cysts occurring together in the same ovary, have been observed not unfrequently, and Flesch<sup>3</sup> is wrong when he considers the two cases of Eichwald and Virchow, and, to a certain degree, that of Martin, as the only known cases. Lebert<sup>4</sup> was already able to prove this combination in seventeen cases. And are not the cases of Goos, Spencer Wells (three cases), Brann, Peyer Imhoff, Virchow (in his Archiv, Bd. III., p. 377) and others, instances of this kind?<sup>5</sup>

The situation by preference of dermoid cysts is undoubtedly

<sup>1</sup> Galenus, Methodus medendi, Lib. IV., cap. 12.

<sup>2</sup> Cfr. Bauchet's paper. Lebert also (Traité I., p. 257) relates three cases of dermoid cysts in birds, which contained feathers. Meckel in his Archiv für Physiologie, I., p. 528, says, that in dermoid tumors in cows, cow-hair has been found. Baillie and Hunter are said frequently to have found dermoid growths in animals. In a bitch, Tyson found dermoids of the ovary (Leske, Abhandl., 1774, p. 326).

<sup>3</sup> Verhandl. der physic.-med. Ges. zu Würzburg, N. F. III., p. 111, cfr. Virchow-Hirsch's Jahrb. für 1872, II., 3, p. 630.

<sup>4</sup> Traité, etc., I., p. 258.

<sup>5</sup> The combination of colloid cysts with dermoids of the ovary has been very frequently observed by the older authors. So frequently, indeed, that Autenrieth, in his Archiv, Vol. VII. (1807), p. 260, states, that bones and teeth are found only "when the cyst contains a clear, brown, gelatinous substance, or whitish, tenacious, fatty, coagulated mucus," whereby, probably, colloid is meant. Autenrieth remarks that this condition is identical with that of a normal osseous and dental development, for "all normal bones develop only in cartilage, *i.e.*, solidified gelatine, the teeth in little sacs filled with fluid viscid gelatine." Virchow (Verhandl. der Berl. Geburtsh. Ges., III., 1848, p. 211) draws a different conclusion from this statement of A.'s: "The frequent occurrence of these accidental formations merely warrants us in the supposition that their presence in the ovary tends to produce the colloid degeneration of that organ," all of which speaks plainly against the statement of Flesch.

the ovary. We must, however, join Lücke<sup>1</sup> in opposing the opinion of Lebert, that the majority of dermoid cysts are situated in the ovary. For even if Lebert, in 188 cases of dermoid tumors, found 129 dermoid cysts of the ovary, this predominance is unquestionably due to the fact that dermoids of the ovary are more frequently described than the much more commonly observed dermoids of the subcutaneous cellular tissue. Neither have we been able, from the cases collected by us, to discover a special preference of the degeneration for the right ovary above the left, as is inferred by Lebert from his statistical compilation. Lebert finds the side, on which the disease was seated, noted in sixty-four cases, viz.:

The Right.	The Left.	Both Ovaries.
39 times.	19 times.	6 times.

In some of the older authors we already find sometimes the right, sometimes the left side, mentioned as the one most frequently affected, and each one endeavors, in accordance with the side to which he believes his experience warrants him in giving the preference, to find the most astute explanations of the causes why the right or the left side, as the case may be, is the one diseased.

E. G. Bose<sup>2</sup> says: "It is really wonderful that such hair is found chiefly in the left ovary." Whereupon Josephi<sup>3</sup> calmly remarks: "According to my observations it has been found with equal frequency in the right ovary." Hofrichter<sup>4</sup> decides in favor of the left ovary, and adds the following reflections: "Generally the left ovary is the one diseased. Can the pressure exerted on the ovary by the fecal accumulations in the rectum during pregnancy, and in consequence of sedentary life and irregular habits, besides being the occasion of much subsequent suffering, be the original true causative agent of the affection?"

This opinion is shared by Röhr<sup>5</sup> (*Vasorum sanguiferorum*

<sup>1</sup> Lücke, *Lehre von dem Geschwülsten*. Erlangen, 1869, p. 125.

<sup>2</sup> *Programma de præternaturali pilorum proventu*. Lipsiæ, 1776.

<sup>3</sup> Josephi, *Ueber d. Schwangersch. ausserh. d. Gebärmutter*. Rostock, 1803.

<sup>4</sup> *Zeitschrift für Geburtskunde*, herausg. von Liebold, etc., VII., p. 245, also Gräfe und Walther's *Journal*, 1818, XII., 1, pp. 60-87.

<sup>5</sup> *De hydrops Ovarii*. Jenæ, 1842.

et lymphaticorum sinistri ovarii compressio a flexura sigmoidea et intestino recto affecta, etc.).

Kehrer (1842)<sup>1</sup> says on this point: "There is as yet no physiological explanation why the left ovary in women is more frequently diseased than the right. Heim, in sixteen cases of tubal and ovarian pregnancy, found the *left* side affected in more than ten cases. Dohlhoff, Groth, Malin, and others, also report cases of disease of the left ovary."

Meisner<sup>2</sup> in the work compiled by him with exemplary diligence, when speaking of the opinion expressed by Heim, mentions several cases where the right ovary was the one affected. Meckel in his work on Pathological Anatomy (II., 2, p. 272), contending for the right ovary, says also, that affections of the ovary are to be observed "most frequently on one side, particularly, in opposition to the general opinion, the right, much less frequently on both sides." The most interesting statement on this point is contained in Peter Frank's excellent work, "De curandis hom., morbis," VI., 1, p. 314:

"Forsitan, quod monente id egregio Galliae anatomico<sup>3</sup> ovarii sinistri positio, illa dextri, sæpius elevatio sit, illius frequentius quam istius tumor percipitur."

We regret not to have succeeded in discovering any comments on Frank's statements in other authors.

In 245 cases of dermoid cysts of the ovary we found the side affected reported 117 times, viz., the disease was situated in

The Right Ovary.	The Left Ovary.	Both Ovaries.
52	45	20 times. <sup>4</sup>

Inasmuch, as already stated above, none of our cases are identical with those of Lebert, a statement which can at once be controlled by comparing our respective bibliographical notices, it is feasible to join the results obtained by us both, as follows:

<sup>1</sup> Miscellen aus dem Gebiete der Weiber- und Kinder-krankheiten, in the Neue Zeitschrift für Geburtskunde, XII. Bd. p. 239.

<sup>2</sup> Forschungen des XIX. Jahrhundert, etc., von F. S. Meissner, Bd. II.

<sup>3</sup> Peter Frank never quotes an authority by name, but always says: "egregius ille," or "clarissimus ille," etc.

<sup>4</sup> The incorrectness of Foot's statement (Dublin Quart. Journ., Vol. XXXVI., 1863) that dermoids of the ovary are always unilateral, thus demonstrates itself.

	The Right Ovary.	The Left Ovary.	Both Ovaries.
Lebert,	39	19	6
Pauly,	52	45	20
	—	—	—
Total,	91	64	26

The predominance of the right ovary, as seen from these figures, already appears much less marked than previously. But, as little as we believe ourselves justified in concluding from our collection of 117 cases, that the affection occurs on one side or the other twice as often as on both together (45 and 52:20), just as much do we consider the deductions drawn from Lebert's compilation<sup>1</sup> to be incorrect. We believe that neither side is specially favored, that it is a mere chance, if in a number of indiscriminately collected cases—and such a small one at that—one side is found to be particularly affected,<sup>2</sup> and that it is, therefore, a useless trouble to endeavor to discover the reason for the preference of one side. At the best, it will be difficult to find a physiologically unassailable reason—a vague guess would probably be, as heretofore, the limit of our ingenuity.

Some authors have further laid special stress on the statement that ovarian dermo-cysts occur most frequently in unmarried females, or rather, particularly in sterile married women. The somewhat frivolous remark of Astruc,<sup>3</sup> the physician of Louis XV., would scarcely have induced us to attempt a serious refutation of this, as will be shown, erroneous opinion. The words of Astruc are rather more curious, than serious; he says: —“*Sæpius inveni in viduis iisque feminis cœlibus præsertim in*

<sup>1</sup> How Lebert came to say among other things, that the subcutaneous dermoid cysts appear to be more common in men than in women (*Traité*, etc., I., p. 257), because of 12 cases, 7 occurred in men, 4 in women, and 1 in a child whose gender is not given, is as inexplicable to me as the justification for his above-stated deduction. It is evident that the possibility of drawing inferences from statistical material does not begin until at a certain point, and that, of course, a proportion of 7:12 (such as the one mentioned here,) is as valueless, as if I were to say, because I happen to have seen 2 cases out of 3 occurring in male subjects, that these are predisposed to the disease.

<sup>2</sup> Morgagni says in his *De Sedibus et Causis Morborum*, Cap. 39, Art. 38: *Fuerunt quidem ut tum dicam, qui in sinistro nec pauciores tamen, qui videntur in dextro ovario pilos, etc.*

<sup>3</sup> J. Astruc, *Traité des Maladies des Femmes*, T. III., p. 34.

iis, quæ rationis et castitatis præ se ferunt speciem, quam in ipsis uxoribus." But Baker Brown<sup>1</sup> asserts, that the disease occurs particularly in unmarried women, and that in the majority of cases in which it is found in married females, the marriage was sterile, a statement which Churchill very properly contradicts.

We have found it stated in 118 cases, whether the patients were married or single: there were 49 single and 69 married women. Of these last sixty-nine, we found the remark "childless" only in five cases. But on the other hand, in many cases there is no note made whether the marriage was fruitful or not. But in thirty-four cases the presence of children is distinctly stated, even as many as nine living children, and once,<sup>2</sup> eleven children.<sup>3</sup>

We now turn to the question of the development of dermoid cysts of the ovary, a question which the cleverest heads of past and present ages have endeavored to solve. In discussing this subject, we shall devote considerable space to the opinions of older authors, less for the purpose of elucidating this—frequently unintentionally—entangled question, than to present the peculiar views which have reigned at different times on these wondrous formations, which permit interesting reflections on the observers and the spirit of each epoch.

And really, no theory of the middle ages can be considered too extravagant and preposterous, when we reflect, that not quite a generation ago, Velpeau<sup>4</sup> himself, while exhibiting to his clinic a young man, twenty-seven years of age, from whom he was about to remove a dermoid cyst of the testicle, the existence of which had been recognized since the fourth month of his life, gave the following explanation of the nature of the tumor:

"The external fistulous opening of the cyst is nothing else than the anus of the child,<sup>5</sup> the yellowish green mass in the cyst—now

<sup>1</sup> On Ovarian Dropsy. London, 1862.

<sup>2</sup> Zeitsch. für Geburtsh. herausg. von Liebold, etc., Bd. XXXII., p. 288. Case of Heinrich.

<sup>3</sup> An interesting compilation by Tilt, partly belonging here, is found in the London Gazette, September–March, 1850–51, who collected the observations of Lee, Chéreau, Bluff and himself, and found chronic ovarian affections 290 times in married, 167 times in single, women.

<sup>4</sup> Cf. Cruveilhier, *Traité d'Anat.-Path. Gen.* Paris, 1841. T. I., p. 375.

<sup>5</sup> This fantastic explanation is scarcely equalled by any other, except that



removed by him—is meconium. The bones found in the interior correspond: the first to the scapula, the humerus and the clavicle; the second admits of a doubt between the petrous portion of the temporal bone and the sacrum; the third corresponds to undeveloped fragments of vertebræ.” (!)

The explanation for the fact, that a man like Velpeau could launch into such absurdities, is to be found solely in the prevalence of the traditional opinion that foetal remains must be discovered at all hazards in every dermoid tumor, and Comparative Anatomy served to stimulate the flight of even such a bold imagination as that of Velpeau. The manner of interpretation of the osseous fragments in dermoid tumors, particularly, proves to us most clearly, that they especially led to the greatest errors in description, and permits the most correct estimate of the veracity of the respective authorities.

For, the imagination of the narrator does not always keep pace with the incorrectness of the observation, as in the case reported by the Dominican Father Bertrandus Lot, and the Jesuit Father Gaspardus Schott,<sup>1</sup> in which “a male child developed so completely in the thigh of its father, like Bacchus in the hip of Jupiter, as to permit of its being brought into the world alive by an operation at the end of nine months.” The two venerable fathers, says Himly<sup>2</sup> very wittily, while relating this case, did not attempt to doubt the story itself, they only indulged in speculations as to whether such a child required baptism or not.

The oldest medical opinion on a case of dermoid cyst of the ovary, is to be found in the excellent work of Johannes Scultetus, entitled “*Trichiasis admiranda seu morbus piloris mirabilis. Norimbergae, 1659.*”<sup>3</sup> After relating the very minute

of Castella, who, under the title of “Pregnancy outside of the Uterus,” describes in the *Schweizer Zeitschr.*, Vol. II., No. 2 (Cfr. Schmidt’s *Jahrb.*, Bd. 17, p. 62), a dermoid cyst in the ovary of a 44 years’ old woman, which opened through the abdominal integument, and in which C. “found the soft parts transformed into a fatty spermaceti-like mass; on examining the trunk, however, the female gender was discernible.” To make assurance doubly sure, he adds, that after an injection on the following day several knots of hair and two bones were expelled.

<sup>1</sup> *Physica curiosa seu Mirab. naturæ et artis.* Herbipoli, 1697, p. 645.

<sup>2</sup> *History of the Fœtus in Fœtu.* Hanover, 1831.

<sup>3</sup> A work, of which Rayer, *Sur le trichiasis des voies urinaires*, in the *Mém.*

record of the autopsy, which—as well as the accompanying illustrations—leave no doubt that the case was really one of a dermoid ovarian cyst, he continues as follows: “*Imperitia quorundam folliculum hunc crinitum*” (which was formed in the cyst) “*magna cum admiratione pro utero habebat, qui nullo modo eidem respondit; siquidem neque locus pro utero inveni-endo hic locorum idoneus neque vasa uterina neque uteri substantia ipsa apparuit, ut pote qui reperiebatur paulo infra costas mendasas.*”

“*Hujus aspectu multi in diversam abibant sententiam, pars eorum potissima ex primâ, eaque leviori consideratione and veneficii suspicionem confugiebat, omnem et rationem et experientiam repudiantes, ceu quibus constabat, quod daemonum alias maleficia pilis summopere delectentur, cum nullo fere alio loco facilius aut libentius, quam in his se recondant: argumento sane clarissimo veneficae esse possunt; quas daemonibus et veneficiis omnium maxime oppletas, non aliam tamen ob causam, ab omni veneficii labe immunes existere, vitamque sanam atque hilarem ducere cernimus, quam stabulante daemone et ejusdem maleficio in pilis, parte totius corporis ignobilissima. Unde Judices in Quaestionum ac Torturarum negotio, ubi de istiusmodi maleficio iisdem suspicio oboritur, crines demere jubent, edocti scilicet a Domitiano Imperatore, qui Apollonio Thienaeo, ob veneficia suspecto, pilos abradi jussit.*”

“*Haec, inquam, levior veneficii suspicio ultra modum et rationem adeo invaluit, ut mox doctiores in hanc sententiam, quam facillime traheret, cui credula plebs more suo flexibili, ita esse, spontaneo applausu magnâ cum admiratione subscripsit. Fraudulenta manus speciem veri addere moliebatur, subducendo ligulam longiorem intra folliculi complicationes, parte videlicet non ita compactâ; circumspectiores oculi tamen crispin deprehenderunt: unde moniti quidam sanio-rem amplexati sunt sententiam, ex causis nimirum praeter naturam, aequae ac ex naturalibus pilos aut horum analogae oriri posse statuentes; ceu quos trichiasis illa quatuor annorum in Consule Hornano et aliis plurimis vellicabat. Nihil enim novi capillos ex ejusmodi*

de la Soc. de Biologie, 1850, p. 167, says, he regrets not having been able to find it in the Paris library. The copy used by me was from the Leipzig University library.

crasso et glutinoso sanguine cen alimento conveniente oriri posse; modo adsit praeter materiam aptam, loci raritas, et laxitas, ut et calor agnes, apte dispositus, robustus satis, et vegetus, qui gerandis pilis, annis adhuc vigentibus, idoneus esse potuit. Rationem hanc superius autoritate Nic. Florentini confirmatum ibamus, cui nunc duorum quoque Principum Medicorum mentem substituemus: Hippocratis nempe et Galeni, quorum ille lib. de Carnibus, pilos ex glutinoso fieri voluit: hic vero 14. Meth. Med. 18 f. 185 C. Codicis Basil. Ostensum, inquit, est, quod ex succis, qui per cutim transpirant, quod limosum est, id pilos tum a principio gignit: tum de caetero veluti succrescendo auget."

Probably but few observers, at that time, were as free from prejudice and as rational in their observations as Scultetus. Not every one, either, may have dared to accuse his opponents of possessing a "lingua volubilis, totum hoc inter veneficia numerantes" (p. 26 *ibidem*).

It was not the caustic sarcasm apparent in this criticism of these superstitious opinions, but astonishment at what they saw and the desire to explain an enigma by something still more inexplicable, which suggested to the physicians of the Middle Ages the apparent explanation of these growths. Some considered them to be the consequence of sexual relations with the devil, others an expression of the much talked of "incubus" (night-mare), still others, a kind of not clearly specified witchcraft. And thus free license was given to that offensive fanaticism, which thirsted only for an opportunity to destroy its pitiable victims, and which finally came to consider the miserable possessor of a dermoid cyst either as bewitched or as a witch herself.

A much less uncharitable explanation was certainly that of regarding these tumors as a punishment imposed on their possessors for their wickedness. The case of the two venerable monks mentioned above,<sup>1</sup> is an illustration of this belief; they relate the story of a man named Ludwig Roessel, of Flanders, "qui ridens dolores uxoris suae parturientis, quasi eos simularet, et optans eos experiri, ex imprecatione uxoris et ultione divina diu dolorem maximum in femore dextro pertulit, spatio

<sup>1</sup> Cfr. page 417 of this paper.

scilicet novum mensium ; quo tempore completo, inde excisus fuit infans masculus, qui nomen patris sortitus est."

We are thus informed, at least, that among the punishments imposed by Heaven, is included, under certain circumstances, also pregnancy for men, and farther, that dermoid cysts have even been baptized. The latter occurrence has also taken place, by the by, in our own century.

In a catalogue<sup>1</sup> of the Breslau Museum, published in 1728, we find, under Art. 17, the following notice: "Remarkable remains of a blighted ovum, which was carried 21 years in a meliceris<sup>2</sup> and was known under the name of the great and disgusting Burlisch growth." At that time, therefore, dermoids of the ovary were looked upon as "blighted ova." The name of the tumor in this case was probably derived from its possessor.

Astruc, the body-physician of Louis XV., already mentioned, held the opinion that coition must always have preceded the formation of the tumor. He reasoned, that an ovum with its envelopes remains in the ovary until it becomes steatomatous, whilst the hair, less subject to decomposition, still undergoes a species of growth, like the hair of dead animals which continues to grow after death.

Later on—a sign of advanced habits of observation—pathologico-anatomical causes for these still wonderful growths were more sought for. Thus in the work of Josephi,<sup>3</sup> who was Professor of Anatomy, Surgery and Obstetrics in Rostock, we find an enumeration of seven different possibilities, which may lead to the development of ovarian pregnancy, that is, to dermoid cysts of the ovary, which it is evident, on careful perusal of the book, are meant by the author :

1. The external coat of the ovary is too thick or diseased.
2. The Graafian follicles do not all project equally from the stroma of the ovary ; if a deep-seated ovum becomes impregnated, its entrance into the tube may be attended with difficulty and eventually ovarian pregnancy ensue.
3. Fear, fright, anxiety, anger, which interrupt the orgasm

<sup>1</sup> Kemla's zweytes Supplement zu den Bresl. Sammlungen. 1728.

<sup>2</sup> An encysted tumor with a substance resembling honey.

<sup>3</sup> Josephi, Ueber die Schwangerschaft ausserhalb der Gebärmutter. Rostock, 1803, p. 55.

during or at the moment of conclusion of the sexual act, whereby the condition of the over-excited nervous system becomes so changed as to interfere with the entrance of the ovum into and its reception by the Fallopian tube. Josephi refers to Krüger (Diss. path. ovarii mul., Gött., 1782) as his authority for this statement, who says: "An illicit sexual act may be the source and origin of this trouble, in which a woman permits a man to approach her, but, for fear of impregnation and the loss of her honor, withdraws from his caresses before the completion of the act, which she thus strives to frustrate."<sup>1</sup> As a reliable authority is mentioned Astruc, who, as we remarked above, claims to have found dermoid tumors particularly in young girls and widows.

4. The Fallopian tube is too long, or too short, and does not reach close to the ovary.

5. The tube is pathologically adherent to the adjacent parts, and is thus prevented from free motion.

6. Abnormal condition of the tube, such as spasmodic contraction, paralysis, ulceration, etc.

7. Absence of the fimbriated border, or imperfect development of or defect in the fimbriæ, such as adhesions of vesicles, whereby they become incapable of enclosing and receiving the ovum.

At all times, continues the author in another place (l. c., p. 63), are such phenomena to be considered as evidences of imperfect<sup>2</sup> conception (the chemico-animal process occurring during procreation does not take place properly, or the formative impulse follows a wrong direction, or is not sufficiently active to induce the proper development of a well-formed ovum). It is only when hair<sup>3</sup> alone is found in an ovary that he con-

<sup>1</sup> This—really more transrhenan—explanation may have been taken by Krüger, and the numerous others who have repeated it after him, from the same source as the Dict. des Sciences Méd., T. XIX., p. 399, which contains the following passage: "La femme racontait avant sa mort, que la crainte d'être surprise dans les bras de son amant, en entendant remuer la clef de la porte de sa chambre, qu'elle avait imprudemment laissée au dehors, lui fit éprouver la plus vive émotion à l'instant ou elle a dû concevoir."

<sup>2</sup> A theory which runs like a red thread through nearly all the books up to the middle of this century, and is also to be found in Peter Frank's *De cur. hom. morbis*.

<sup>3</sup> In Bauehet's above-mentioned prize thesis we find the universal, system-



siders it improbable that it should result solely from coition, or be the only remnant of a foetus; for such hair might be developed without previous sexual intercourse, even in a virgin ovary—bearing in mind the celebrated case of Baillie<sup>1</sup>—perhaps in consequence of an error of nature, as may be the case also in other viscera and tumors.

We have found this array of causes, in which is to be recognized the desire to trace the origin of these mysterious anomalies of the ovary to a pathologico-anatomical basis, repeated with great fidelity in a large number of essays up to the fifth decade of the present century, generally without mention of the source from which they drew this valuable information. Others agreed with Boehmer<sup>2</sup> in summing up all the causes under the one head of *intemperantia in re venerea* (such as the soon to be quoted Alquié and others). Others again were satisfied with the comprehensive phrase,<sup>3</sup> *par une sorte d'extase amoureuse*.

In giving the above, as much as possible, chronological account of the various views on this subject, we omitted to state that one of the oldest theories was, that the hair found in the dermoid tumors had been swallowed by the patients, and been deposited in various parts of the body, such as the ovary.

Several passages in Aristotle (*Hist. anim*, 3, 6. *De part. anim*, 4, 2), having been more or less misinterpreted, have created a complete myth of a *morbus pilaris*, especially as a third passage in Aristotle's *Hist. anim*, 7, *Cap. II.* (in Latin it reads: *Interdum mulieres potu haurire pilum; qui ubi ad mammas pertingit, dolores et cruciatus maximos parit deinde educto pilo hoc malum finitur*), appeared to give the apparently most suitable explanation for the passages above quoted. But Scultetus, whom we have already cited, answers briefly and pointedly: "*Sane mihi probatur admodum difficile videtur,*" and further on: "*Cur autem Aristoteles in hunc modum decep-*

atic, and utterly unscientific classification of the cysts according to their contents, as *kystes sébacés*, *kystes foetaux*, *piloux*, and dermoid cysts are thus discussed separately under three heads. Cfr. Wagner's critique of Bauchet's paper in Schmidt's *Jahrbuch*, Bd. 97, p. 294.

<sup>1</sup> London Med. Jour., XIII., No. 8.

<sup>2</sup> P. A. Boehmer, *Observ. anat. rar.* fasc. 1, p. 43.

<sup>3</sup> Dict. des Sciences Méd., T. 19, p. 399, art. Grossesse.

tus sit pueriliter, fortasse causa fuit, ut dicit Vesalius, alienjus mulierculæ, quæ id perperam Aristoteli persuasit, narratio."

What Vesalius and Scultetus declared erroneous, appeared quite credible to Cleghorn<sup>1</sup> and Cheston Browne.<sup>2</sup> The latter only permitted himself the slight modification of considering the teeth found in the ovary to have been swallowed, whereas the hair, in all probability, developed at the spot where it was found.

Meckel<sup>3</sup> expresses the opinion that "in young girls, as well as occasionally also in older women, and particularly in old virgins, these growths might readily develop without being preceded by sexual intercourse. Especially when the ovaries or the uterus of old maids were the seat of the tumors, partly because their moral virginity is supposed not always to be on a par with their physical virginity;<sup>4</sup> partly because their genital organs show a great liability to abnormal formations; and partly again because one does not know when the development of this substance began. Within certain limits a *lucina sine concubitu* might be assumed in such cases"—a theory of which Cruveilhier says verbatim: "*Cette théorie ingénieuse est purement hypothétique et ne repose sur aucun fait positif.*"

But Meckel adds expressly, that his interpretation will not suffice for all cases; then the possibility of an extra-uterine pregnancy, a view against which he had just previously advanced the strongest reasons, must be allowed.

This opinion, advanced by such an authority as Meckel, was concurred in by the majority of his contemporaries. Thus in various writings of the beginning of this century we meet with an *ex insolito modo adaucta certe ante tempus provocata activitas partium genitalium, sed et plana propria et spontanea, quin coitus viri admissus sit*, or with a *vis formativa* peculiar to the ovary, or a *nisus generandi pilos et dentes*, or on the other hand with a view the very opposite of the parthenogenetic doctrine, a *debilitas vis generandi, sei es apud viros tenui valetudine utentes*, or *apud valde juvenes mulieres*, in old men

<sup>1</sup> Trans. of the Irish Acad., Vol. I., pp. 73-88.

<sup>2</sup> Path. inquir., p. 47 seq.

<sup>3</sup> Deutsches Archiv für Physiologie. Halle, 1815. I. Bd., p. 579.

<sup>4</sup> Pleraque horum non sine culpa affectuum animi contingere, likewise says Morgagni De sed., etc. Lib. III., Cap. 39, Art. 38.

married to young women, and the reverse. Others again looked upon dermoid tumors as the "product of fertile coition with subsequent degeneration of the ovum"<sup>1</sup> and called these growths in consequence "hair-moles."

Cruveilhier<sup>2</sup> and Bicheteau<sup>3</sup> and with them many authors, believed in two modes of origin of dermoid tumors, accordingly as they occurred in adults or in children. Cruveilhier's exact words on this point are: "*Une conception extra-utérine à lieu, une adhérence s'établit dans les premiers temps de la conception entre les parois du kyste et l'embryon. Suit une destruction plus au moins complète.*" Only the skin remains, and with it the basis for the hair, thus hair-cysts arise. If portions of the maxillæ and rest of the skeleton are found they have escaped destruction (*ils ont échappé à la destruction*). But Cruveilhier himself feels that his theory does not apply to all cases, but is, as he says, *en défaut chez les jeunes filles pubères<sup>4</sup> et les hommes*. In the latter cases the hair-cyst is *les débris d'un fœtus contemporain de l'individu qui le porte*. This view, according to Meckel (l. c., p. 569), had already previously been held by Tumiatì,<sup>5</sup> "because the normal structure of these parts proves that they cannot have developed spasmodically and by chance, but must have sprung from original germs."

Ferguson appears to coincide in this opinion when he says: "In earlier fœtal life nature neglected to fully develop the second fœtus."

Very recently Klebs<sup>6</sup> has supported this theory of fœtal in-

<sup>1</sup> Meissner, *Forsch. des XIX. Jahrh.*, Bd. I. Leipzig, 1826.

<sup>2</sup> *Anat. path. Lib.* 25.

<sup>3</sup> Cfr. Vogel's *Path. Anat. Abth.* 1, p. 224.

<sup>4</sup> Baillie's correctly observed cases, particularly, led to the desire for the adoption of a theory, applicable also to virgins. If perchance a dermoid cyst happened to open into a neighboring organ and discharge its contents outside, the reputation of the poor patient—no matter how innocent she might really be—according to the old theory (extra-uterine pregnancy) was at once destroyed. Spencer Wells relates an interesting case, to which he was called, where he found the family in deep grief over their degenerate daughter, who had passed fœtal bones per anum. The extra-uterine pregnancy theory has thus caused trouble even as recently as in our day.

<sup>5</sup> Su un amasso di capegli. *Gr. Scelti.* XX., p. 217.

<sup>6</sup> In his *Treatise on Path. Anat.*, Berlin, 1869, p. 332, whereas in the first edition of the same work, p. 99, he had expressed the opinion, that an invagination of the skin occurring during fœtal life gave rise to this anomaly.

clusion, originating in a fissure of the blastema. The two germs grow together before the formation of the allantois, consequently the foetal parts are immediately contiguous to the wall of the cyst. In the fourth edition of his work, which appeared last year, Klebs has, however, in consequence of the investigations by His and Waldeyer on the development of the chalazæ and their branches, returned to the view now entertained by most pathologists, to which we will presently refer again.

Before doing so, permit me briefly to state the opinion of Alquié, who in the *Gaz. des Hôpitaux*, 1857, No. 53, under the title of "Large Ovarian Tumor consisting of Ten Embryonic Sacs," describes a case of dermoid cysts of both ovaries in a woman forty-nine years of age (four cysts on the right, six on the left side), and closes his consideration of this "evidently repeated intra-ovarian pregnancy," by laying down the following maxims: "The impregnation of the ova in the ovary through their enveloping membrane is possible. An intra-ovarian pregnancy may repeat itself, in this case ten times. The discharge of an ovum is not necessarily connected with menstruation."

The first to emancipate himself from the wild theories advanced by his contemporaries—and not alone by them, as we have just seen—was Ruysch.<sup>1</sup> He believed that such organic bodies, as bones and teeth, may develop also in simple encysted tumors, and refers to a case where he found in the stomach of a man "a soft cheesy tumor with teeth and bones." This case, however, was not regarded as sufficient proof by most authors; it looked too much like an instance of mystification, and Josephi himself says: "Probably these bones had been swallowed by the patient." Cleghorn and Soemmering<sup>2</sup> express the same opinion of this case.

Analogous to Ruysch's theory was that of Gambini, which we find briefly stated in the *Gemeins. Deutsche Zeitschr. für Geburtsk.*, 1831, Vol. VI., p. 539.

Meissner, whom we have already quoted above, says (l. c., I. Bd., p. 91) on this subject: "As much as such cases incline us to assume an original pregnancy of the ovary, and to seek the reason for the new formation in the organic change of the latter

<sup>1</sup> Ruysch, *Hist. Anat. Med.*, Dec., III., No. 1, p. 2.

<sup>2</sup> Cfr. Meckel's above-cited paper, p. 568.

or in the degeneration of the foetus, still this does not appear to be quite the true conclusion. It seems to us, after mature thought, that we are ignorant of the conditions necessary for the development of organic bodies in diseased tissues, and we can see no reason why hair should not be developed in a tumor, without previous conception, as well as membranes, adipose tissue, cartilaginous and bony concretions, which we have met with in similar growths. This view is supported by the formation of hair in males.”

Rokitansky looks upon them as ordinary fat-cysts, developed from the Graafian follicles, the contents of which may be either fat only, or fat, hair, and teeth or bones. He, as well as Paget,<sup>1</sup> classes the dermoid with the pearly tumor or cholesteatoma of Johannes Müller.

Rokitansky's opinion is opposed particularly by Pelikan,<sup>2</sup> who seeks an explanation in the supposed excessive development of the numerous embryonic formative elements in the cellular tissue of the ovary. The reason for the different development of originally identical elements is, however, not given by Pelikan.

Steinlin of Zurich,<sup>3</sup> who had occasion to examine ovarian dermoids in a fresh condition and at various stages of development, makes the following statement as regards their origin: “As the first rudiment of a fat-cyst in the ovaries we find at the site of a Graafian follicle a little fleshy lump of the size of a millet-seed, which is enucleated with some difficulty from a sac, which I take to be the former sac of the Graafian follicle. Later this little lump separates from the sac with the exception of one spot, where it remains attached to the sac with a broad pedicle, through which vessels enter and spread in loops throughout the tumor. The sac still hangs closely to the tumor, and on section the interstice between them appears only as a small fissure. Still later, this interstice becomes filled with a thin layer and we find sebaceous follicles developed. . . . . The explanation of the formation of bone in dermoids is more difficult. If we should insist on finding a connection between

<sup>1</sup> Cfr. Pöhn. Eine Dermoidcyste des Mediast. ant. Berlin, 1871.

<sup>2</sup> Petersb. Med. Zeitschr., 1, 3, p. 85, 1861. Cfr. Schmidt's Jahrb., Bd. 114, p. 179.

<sup>3</sup> L. c., p. 151.



these osseous fragments and the formation of skin in the cyst walls, we might compare them with the so-called cutaneous or deposit-ossifications."

At this weak spot, Ritchie<sup>1</sup> attacks Steinlin's paper, and maintains that these bones found in the ovary show true bony structure under the microscope, whereas the external skeleton envelopes of cretaceous animals are formed from the skin and are nothing more than callosified epidermis and deposits of lime. And Ritchie is perfectly right in making this objection. But he uses it only to advance a far more incorrect hypothesis, which he announces as his own, although Baillie, Meckel and Voigtel proposed it long before, namely, "that every dermoid cyst of the ovary is an ovum, which has passed through certain stages of development, and is a perverse attempt at parthenogenesis."

At a preceding passage he says verbatim: "Nature disappointed in her efforts to make a perfect foetus, did the best she could, and thus, teeth, bones and hair were formed, which might be termed animal vegetables."<sup>2</sup>

An absurdity, which one would scarcely have expected of the man, who enjoyed the good fortune of being permitted to examine the cysts removed by Spencer Wells!

Von der Porten, in his work, "Investigations on the Teratomata of the Goose," published in 1873, has furnished us with an intelligent refutation of this parthenogenetic theory.

"Most arguments," says he, "merely contradict the view that teratomata are the product of sexual impregnation. However much reasoning scepticism may object, undoubted observations have decided, that the unimpregnated ovum also, under a certain course of breeding, undergoes visible transformations, which take place entirely in accordance with the laws framed by us from the process of procreation by non-sexual proliferation in lower organic bodies. The investigations of Hensen<sup>3</sup> on

<sup>1</sup> Contributions to Assist the Study of Ovarian Physiology and Pathology, London, 1865.

<sup>2</sup> Honorius Faber, De Plantis, etc. Norimbergæ, 1672, p. 103, already says the same in the same words, excepting that he includes under "his animal vegetables" hair, wool, feathers, nails, horns and teeth. Thus Ritchie deserves the credit only of having added the bones to this list. Compare Leske, Auserl, Abh. aus den philos. Transact. von 1699-1720, I. Lübeck, 1774.

<sup>3</sup> Centralblatt für medic. Wissensch., 1869, p. 403.

the breeding of unimpregnated ova, have determined that the protoplasm of these ova is capable, without impregnation, of undergoing segmentation and sending out processes, which grow to tendrils of  $\frac{1}{2}$ " and more in length. The size of these tendrils is so considerable, says Hensen, that the original substance of the ovum evidently could not suffice to supply them. Therefore, *an actual increase of material must have taken place.*

"Admitting such an activity of the unimpregnated ovum," continues Von der Porten, "why should it not extend also to the formation of the different tissues and organs? Why should the consequence of such parthenogenetic activity not be a monstrosity, a teratoma? Why not? Indeed, why should the fable of the immaculate conception not be true, why the parthenogenetic development of a human being not be possible?"

Finally Von der Porten says: "From simple non-sexual segmentation, which is unquestionable, to the non-sexual formation of a human being, which is scarcely even a hypothesis, is a large and daring leap. Besides, the occurrence of exquisitely developed teratomata in organs far distant from the ovaries, such as the anterior mediastinum, etc., has dealt the death-blow to the hypothesis of the parthenogenetic origin of these tumors."

Lebert (l. c., p. 266) explains their development by the *loi d'hétérotopie plastique* framed by him, which reads: "*que des tissus simples ou composés et des organes même plus complexes peuvent se former de toutes pièces dans les parties du corps où à l'état normal on ne les rencontre point.*"

I have seen from the literature at my disposal, that I am not the first whom this explanation causes to feel still more plainly the want of an explanation. This *hétérotopie plastique* has not brought the problem of the origin of dermoid tumors a single step nearer its solution. Heschl<sup>1</sup> has paid particular attention to this question and discussed the manner of development of dermoid tumors in an excellent paper. While admitting the invagination of the integument assumed by Remak<sup>2</sup> for all

<sup>1</sup> Prager Vierteljahrschr., 68 Bd. Jahrg., 1860, p. 36.

<sup>2</sup> Deutsche Klinik, 1854, p. 170. A remark of Bauhin is interesting, who looked upon the presence of hair in a case of dermoid of the ovary as an instance of transposition, because the hairy covering of the pubis happened to be absent in that very woman. We do not intend to maintain, however, that Bauhin in making this statement thought of an invagination according to

dermoid growths—except those of the ovary and the testicle—and strongly supporting his views by several cases of his own, he makes the following statement regarding these same dermoid cysts of the ovary and the testicle :

“For the present it would seem advisable to withhold all opinion on the development of these dermoid tumors. It appears to me highly probable that they originally belonged to intra-uterine life; the elements out of which they grew are, however, entirely unknown. The real explanation of this anomaly has to satisfy too many claims to allow of its being discovered as yet.”

Before coming to the latest opinion expressed on this subject, that of Waldeyer, we will briefly mention a remark made by Mayweg,<sup>1</sup> because it appears most suited to precede Waldeyer's opinion, in which we see but a more precise definition of the one now to be quoted. Mayweg says (p. 50): “Other authors believe that dermoid cysts originate in a transformation of the epithelium and theca of the Graafian follicle, an opinion which I share, for the reasons, that the ovum is not necessary to the explanation of dermoid cysts, then because calling them ‘congenital’<sup>2</sup> does not decide the question, and finally because in a mucous membrane with an epithelium of so indifferent a character as that of the Graafian follicle, every condition necessary to the formation of hair and teeth is given.”

Waldeyer<sup>3</sup> defines this theory as follows: “The epithelial cells of the ovary are capable, in the way of ordinary proliferation, of furnishing differently formed products than are usually found during the division of cells, where the descendant cells always bear the same character as the parent cells. This power, peculiar to the epithelium of the ovary, is made comprehensible by the fact that all ovarian epithelium must be considered as undeveloped germinal cells, as undeveloped ova. If a proliferation of the deep-seated ovarian epithelium takes place in

Remak. Cruveilhier mentions this theory of Bauhin (p. 4 of the text to the 18th edition of his Atlas), and says: “Je n'ai jamais vu les poils crépus et frisés, comme ceux du pubis, par exemple; ce qui réfute l'opinion de Bauhin.”

<sup>1</sup> Entwicklungsgeschichte der Cystengeschwülste des Eierstocks. Bonn, 1868.

<sup>2</sup> No one probably before Mayweg has ever pretended that the question was solved by this assertion.

<sup>3</sup> Archiv für Gynäkologie, I. 2, 1870, p. 306.

the ordinary manner, in most cases a myxo-cystoma (colloid cyst) will be the result, whereas if the proliferation referred to is attended with a deviation of the products of development, a dermoid cyst will be formed."

Friedländer's designation (in his article in the fifty-sixth volume of Virchow's Archiv, p. 365) of Waldeyer's theory as "the assumption of the so-called parthenogenetic development from a germinal cell," and his comparison of it to Meckel's *lucina sine concubitu*, seems to us a very proper criticism of Waldeyer's hypothesis, in which he (Friedländer) also concurs.<sup>1</sup> Waldeyer, by means of his own investigations on "Ovary and Ovary," places on a pathologico-anatomical basis the views entertained by Meckel at the beginning of this century, especially in opposition to the extra-uterine pregnancy theory then generally accepted—views, which Lanzweerde, Schacher, Haller (the latter at least as regards the hair; teeth and bones he considered remnants of a foetus), Blumenbach, Baillie, Voigtel, Treviranus, shared with Meckel.

We add yet another theory, that of Pidoux,<sup>2</sup> not, however, because it is of special value or characterized by unusual clearness, and then pass to the opinion now generally accepted.

Pidoux said in the Transactions of the Paris Academy of Medicine in 1857, that he assumes an individual temperament (crasis) peculiar to ovarian cysts, which he calls *diathèse ovaro-chorioleuse*. This diathesis is local in so far, that it always affects only the ovaries. One and the same cyst may in course of time assume the various shapes and characters, which are ordinarily observed in different cysts.

We have deferred to the end of the chapter the description of that theory of the genesis of dermoids of the ovary which may well be termed the most generally accepted, and which attributes their development to a process of separation by strangulation occurring during embryological growth, such as

<sup>1</sup> While doing so, he says that this hypothesis applies only to the ovary. Although perhaps not intentional, Friedländer, with this remark, has hit exactly the, in our estimation, weak point of Waldeyer's theory. Dermoid cysts have been observed in so many different organs of the human body; why should those of the ovary alone have a separate genesis?

<sup>2</sup> Cfr. Schmidt's Jahrb., Bd. 95, p. 330.

Remak<sup>1</sup> assumes for the cholesteatoma, and Thiersch<sup>2</sup> for the dermoid cysts.

Just as subcutaneous dermoid cysts may develop through simple invagination of the skin and strangulation of the sacs of epidermis from which are formed the future hair-follicles, so also may the like development be accepted for the far more deeply seated ovarian dermoids, with the difference that in the latter the closure of the abdominal cavity during foetal life is to be considered, especially since the investigations of His<sup>3</sup> have indubitably demonstrated that the Wolffian bodies and Müller's ducts are not formed by separation, as hitherto supposed, from the middle, but from the superior blastodermic membrane.

"Thus dermoid tumors," to quote a clever phrase of Heschl,<sup>4</sup> to which we add, not only the subcutaneous, but also the deeper seated, "are to be compared to the truant stones of geology—*si magnis licet componere parva*—which have been separated from the rocks, the characters of which they bear and to which they belong, and transplanted to regions where they are as much strangers as our dermoids."

Of the objections made to this separation theory, the majority on mature deliberation must be rejected as invalid. Von der Porten's reply, for instance, that every tissue-change might be explained in this manner, and that if one feels justified in asserting this of a theory, it amounts to an admission, in other words, that the theory explains nothing, is an entirely arbitrary deduction, which needs a refutation as little as Remak's great and important discovery now needs confirmation. We agree with Von der Porten, however, that this separation theory completely excludes the possibility of metastasis, a hypothesis which may boast of the weighty authority of Virchow<sup>5</sup> as one of its defenders.

<sup>1</sup> Deutsche Klinik, 1854, p. 170.

<sup>2</sup> Epithelialkrebs, S. 69.

<sup>3</sup> Archiv. für Microsc. Anat., 1865., I., p. 150.

<sup>4</sup> L. c., p. 52.

<sup>5</sup> Cfr. the case of mediastinal teratoma described by Virchow, in which "metastasis took place" in the liver and kidneys; also the case observed in the Pathological Institute in Berlin during the winter of 1872-73, in which about three months after the removal of a simple cystic tumor of the scrotum



We do not deny that some things still remain inexplicable even with this separation theory, such as the occurrence of products of the middle blastodermic membrane. On the other hand, it still remains undecided whether these portions of the middle blastodermic layer are really to be regarded *as belonging to the neoplasm*, that is to the cyst, or *as merely situated in the wall of the growth*—thus representing displaced portions of the original stroma.

The absence of any derivatives from the intestino-glandular plate is remarkable only, if dermoid cysts are looked upon as the lowest grade of a foetus in foetu, but is self-evident, if they are considered as products of strangulation from the upper blastodermic layer, and their envelope derived from the middle blastodermic membrane as the product of irritation—as is the case in many similar instances. In proclaiming our adherence to this theory, we have said at the same time that we consider dermoid cysts of the ovary to be congenital. There is nothing strange in our day in the assumption, that a person may go about for years and tens of years<sup>1</sup> with the germ of a tumor, untroubled by the Danaic gift which an embryonic ill-luck has planted in his cradle, not to say, in his mother's womb, and that suddenly by any accidental external favorable circumstances, as we must assume, the development of that germ is started.

The age of puberty, in many cases marriage, are, according to most authors, the chief predisposing circumstances. In a large number of cases we have found that the complaints of the patient began exactly at the time of puberty or marriage.

Pregnancy should also be mentioned as an important factor; we shall have occasion to refer more particularly to this cause farther on.

Lebert has published in his repeatedly cited paper a table of the age of his patients, as far as it was noted in his 129 cases. The problematical value of such statistical tables is evident.

a secondary (?) teratoma of a retro-peritoneal lymphatic gland (with cartilage, bone and muscle) had developed itself. *Secondary???*

<sup>1</sup> Virchow in his Archiv, Vol. VIII., p. 418, says of a tumor of the basilar piamater of a man fifty-four years of age: "During life, the man had no cerebral symptoms whatever, as has also been observed in the majority of similar cases; therefore, it is probable that the growth of this tumor was very slow." This is merely one instance of thousands which might be mentioned here.

Not even a guess at the duration of the affection should be ventured therefrom. The majority of the cases described were not treated up to the death of the patient by the gentlemen reporting them; the ages at which they happened to present themselves to their physicians are classed here with so and so many death-years of others. And not every one who died and at whose autopsy the dermoid cyst was discovered, died of her ovarian dermoid, frequently not even of a disease connected with the tumor. Such tables of ages possess, therefore, in the present instance no value whatever. They become of interest through the number of cases in their first and last columns, that is, the age of the youngest and the oldest person in whom a dermoid of the ovary was observed. And here we must consider the finding of dermoid tumors in very young children, perhaps even in fœtuses, as a very important proof of the congenital origin of dermoids. It is of some little importance to us to know the greatest age of these patients, in so far as it enables us to see how long such a momentous germ may rest undisturbed and undeveloped, or how in these very cases predisposing circumstances for the development of the germ happened to be wanting or at least were insufficient to undermine the lives of the patients at an earlier period.

With this mental reservation we reproduce Lebert's table and add to it our own—in 103 cases we have found the age given—and as third in the list Pigné's interesting table, which contains chiefly cases of children up to twelve years of age. To our regret we could not obtain the original of Pigné's paper,<sup>1</sup> to which we attach considerable importance, and were thus unable to compare his cases with our own, if for no other purpose than to ascertain whether any of them are identical with ours. A glance at the two tables (Lebert's and Pigné's) shows us that Lebert was not acquainted with Pigné's article.

	1-5	5-10	10-15	15-20	20-25	25-30	30-35	35-40	40-45	45-50	50-55	55-60	60-65	65-70	Over 70 years.
LEBERT (59).....	0	1	8	3	8	7	4	8	7	6	2	3	1	0	1
PAULY (103).....	4	3	10	8	12	14	10	11	8	10	5	3	1	2	2

<sup>1</sup> Bull. de la Soc. Anat., 1846., T. XXI., p. 205.

	Fœtus in 8th month.	New-born children at term.	From 6 months to 2 years.	From 2 to 12 years.
PIGNÉ .....	3	4	6	5

Before concluding, we will not neglect referring to an observation, which, to our knowledge, has not yet been directly appreciated, particularly in its connection with the congenital origin of dermoid cysts, and to which our attention was called by Prof. W. Alex. Freund of Breslau. We mean the infantile state of the genital organs accompanying dermoid cysts of the ovary.

When observers will have begun to bear this point in mind more generally during life also, as far as can be ascertained by examination, a larger number of observations will enable us to test the correctness of this opinion. In the material collected by us there is a not inconsiderable number of records of post-mortem examinations, but usually not sufficiently accurate to be of service in this investigation. The same must be said of the majority of post-mortem records of our own century, especially of its first half. If the number of instances which we propose to enumerate in support of this statement in question is, therefore, but a small one in comparison with the actually large number of cases, the only reason therefor is to be sought in the defective character of the reports from a period, when investigators preferred to wonder at marvels, rather than to make patient and rational observations, and when case after case was brought forward in the hope of perhaps discovering from a subsequent example what had been left unexplained in former cases. In the following cases a precise account of the infantile condition of the genital organs is given:

Baillie <sup>1</sup> mentions two girls, one 12½, the other 18 years of age, of whom, besides the intact state of the hymen (on which, by reason of the extra-uterine pregnancy theory, much stress was laid at that time), he expressly reports that the uterus was smaller than usual. Meckel says in Vol. II. of his *Path. Anat.* (Part 2, p. 251): "The uterus in external tumors of this kind

<sup>1</sup> *Philos. Trans.*, Vol. V., 1789. Cfr. *Neue Zeitschr. f. Geburtsk.*, Bd. 33, p. 231. Meckel, *Archiv für Physiologie*, I. Bd., 1815, p. 563.

attached to a pedicle"—in the preceding pages Meckel speaks of dermoid tumors of the ovary and nothing else—"is much smaller than usual." A case of a woman 40 years of age, which he describes shortly before, may perhaps also be counted here.

Spencer Wells<sup>1</sup> relates the case of an unmarried patient 18 years of age, operated upon August 1, 1870, who had a dermoid cyst of the right ovary, and remarks: "The uterus and left ovary were almost undeveloped."

In Peter Frank's "*De curandis hominum morbis*, T. VI., p. 2, p. 278," we find as *Observatio magni inter Germanos anatomici* the following interesting notice: "Ut qui cum in ovario virginis, hymene illaeso ac præter modum angusto instructae steatoma pilos ac dentem detexisset: utrum eidem proprium, mole normale adeo minorem ac tunica decidua ne quaquam vestitum reperiit."

In all probability the cases of Hodgkin<sup>2</sup> and Schultze<sup>3</sup> belong here also, as well as one presented by Brodie to the Musée des Chirurgiens (No. 2631 of the collection), which was that of a woman 29 years of age. Also the cases of Bruch and Pelikan. But the reports are so scanty, that it is impossible to ascertain from them with certainty, whether the genitals were really in an infantile state or not.

Unquestionable, however, are several cases observed by Professor Freund, detailed notes of which are not at our disposal at present. In May, 1874, we ourselves had occasion to see a patient 40 years of age with a dermoid cyst of the ovary in Freund's Dispensary, whose genital organs were in a most pronounced infantile condition.

Perhaps these lines may induce investigators to pay some attention also to this not unimportant point.

<sup>1</sup> *Krankheiten der Eierstöcke.* Leipzig, 1874, p. 56.

<sup>2</sup> *Cat. of the Prepar. in the Anat. Mus. of Guy's Hosp., Sect. 8, 1823.*

<sup>3</sup> Stahlberg, *De pilorum et dentium formatione in ovariis.* Gryphæ, 1842, p. 41.

THE DANGERS OF THE TRANSMISSION OF SYPHILIS BETWEEN  
NURSING CHILDREN AND NURSES IN INFANT ASYLUMS  
AND IN PRIVATE PRACTICE.

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THE mode of syphilitic contagion to be studied in this paper is for various reasons interesting and peculiar. Its origin is wholly unassociated with the sexual act, and on the contrary is intimately dependent upon that process so necessary to the growth and maintenance of the being, namely, nursing at the female breast. The agent which communicates the contagion is neither a syphilitic man nor woman, but on the contrary is an innocent infant, which itself has been the victim of syphilis inherited from either or both parents. The secretion in which the disease originates is not derived from a primary syphilitic lesion, but is the product of secondary lesions, namely, mucous patches, which are so frequently found in the mouths of syphilitic infants. The method of this form of syphilitic contagion is as follows: An infant, the victim of hereditary syphilis, having mucous patches in its mouth, is nursed by a healthy woman; the morbid secretions of its mouth are deposited on her nipple, and thus she becomes infected with syphilis, having its initial lesion on this site, which is, in due time, followed by general manifestations. Such instances are peculiarly distressing in themselves, and unfortunately very often the trouble does not end here, as a woman thus syphilitic is very liable for obvious reasons to infect others, so that instances have been recorded in which syphilis thus communicated to a nursing mother has, in time, infected every member of the family. This further source of infection is also important and interesting, and will require our study. In this case the reverse of the previous fact is observed, as the contagion is here derived from the woman with a syphilitic chancre, or chancres, on her breast, the secretion of which infects, or is liable to, as many nursing infants



as may be assigned to her. These modes of contagion, especially the first, have attracted attention, and have been particularly dwelt upon by authors, especially French and Italian, but in America, until now, they have escaped special attention. In other countries they have been shown to be prolific sources of syphilis, and means have been taken to prevent them. My own observation shows me that the time has arrived when they should be thoroughly discussed among us, since undoubted instances of syphilis thus communicated have occurred both in hospital and private practice. But by far the greater danger exists in large hospitals or asylums, where great numbers of children are congregated, and where wet nurses are employed to care for them. In private practice isolated cases of contagion are liable to occur, but in the institutions referred to, owing to certain circumstances to be considered further on, a very great and serious danger exists that syphilis will be communicated. We shall see that there are special reasons why, owing to existing regulations and customs in these institutions, this danger is peculiarly menacing, and certainly no one will doubt the propriety of inquiring thoroughly into it, with a view of its future prevention. During the summer I have had brought prominently to my notice by Dr. C. A. Loring a woman thus rendered syphilitic in a large infant institution, and her case brings out forcibly the dangers I allude to. I propose therefore to make it the basis of this study, as by so doing I can present the whole subject in all of its aspects in such a manner I hope as to be clear and forcible to my readers. The case is as follows:

Emma McNally, English, aged 19, a married woman, entered a lying-in institution in this city on the 5th of May, 1875. She was pregnant for the first time, and that night was delivered of a healthy male child. The mother was and had been very healthy, and the next day her milk came in abundance. In obedience to the rules of the institution, that she should nurse one or more children besides her own in payment for the care received, she gave the breast to two or three babies. The next day a very sickly looking, wrinkled child was assigned to her for care. She nursed this together with her own for a number of days, and then, owing to its debilitated condition, it was sent to the country. She did not notice any sores about the child,

though she did not see it undressed, but she says that it had the sprue, for its mouth was very sore. The next day a foundling was given to her to nurse regularly, and though she was very reluctant to take it, she did so. She described the child as being very thin and wrinkled, and appearing like a little old man, and having sprue very badly. Besides having this child as a regular nursling, she occasionally gave the breast to other children, at the request of their nurses, as her milk was very abundant. While in the asylum she reserved the right breast for her own child, and gave all the others the left. She remained in this condition, taking care of the sickly child, for exactly two weeks, during which time she sometimes noticed that her nipple was slightly sore. She was then in the asylum from the 5th to the 20th day of May, two full weeks. From the 20th of May to the 25th of June she remained at home, and only nursed her own child, then giving it either breast indifferently. At the latter date, June 25th, and on the day following she nursed at the breast the infant child of a lady in private life. The latter child is said to be in appearance healthy, as was its mother also, except that she did not have much milk. In a day or two after this again, that is about June 27th or 28th, she noticed that her left nipple was slightly sore; she describes it as presenting a raw appearance in one spot and being tender. The pain and inconvenience prompted her to spare that breast, so she nursed her child with the other, except for a few days, when she put it to that breast in order to relieve the tension produced by the accumulated milk. She thinks that, in all, the child did not nurse from that breast more than two days after she discovered that it was sore. It may be well to add here that she attributed the trouble of her nipple to the *lady's* child which she had last cared for. The ulcer increased in size, and caused her severe pain, so she consulted Dr. C. A. Loring, who diagnosticated it as a syphilitic chancre. In order to thoroughly verify the diagnosis, I was asked by the doctor to see the case, and the following were the appearances observed July 20th, 1875. On the outer two-thirds of the apex of the left nipple was a thin brownish-green crust, which, being slightly adherent, was painlessly removed with ease, revealing a shining grayish-pink ulcerated surface, of great smoothness, the edges of which were not at all elevated. Upon

pinching the nipple, a very well-marked induration was felt, and this act showed well a blanching of the tissues affected, so often observed in hard chancres, and due to the density of the deposit. The lesion was evidently an infiltration into the greater part of the stroma of the nipple, with superficial ulceration. The pectoral glands could not be felt, as the woman was quite fat, but in the axilla a typical syphilitic adenopathy was felt, the gland being nearly as large as a walnut. One of the left post cervical glands was in a similar condition, as was also the left epitrochlear, which was more than usually enlarged. The inguinal ganglia were not noticeably enlarged. The peculiar character of the ulcer, its indolence, the state of the glands, and the history of the case, led me to confirm fully the diagnosis previously made by Dr. Loring. If any doubt had been entertained, it would have been completely dispelled in a short time, for in the last of July, constitutional syphilitic manifestations showed themselves. On the first of August Dr. Loring addressed a note to me, in which it was stated that she had within a few days witnessed the evolution of a general syphilide, and had observed that the temperature was  $104\frac{1}{2}^{\circ}$  Fah. A few days later I saw the patient, and observed a general roseola, which was very copious, and showed a slight tendency to become papular, also much hyperæmia of the pharynx. The woman complained of great weakness, and of fugitive pains with nocturnal exacerbation. For the purposes of this paper, no further particulars of this case are necessary, as we have the undoubted evidences of syphilis in the woman. It is essential, however, to our object to consider the condition of the two infants, and it was through the assiduity of Dr. Loring that the details were obtained. The first child given her as an extra was, as we have stated, sickly and wrinkled in appearance, and had a very sore mouth, which was considered to be of an aphthous nature. The next child which was assigned to her in a day or two was a female foundling, which she kept for the remainder of her term in the institution. The woman states that so sickly and withered did the child appear, looking like a little old man, that it attracted the attention of other nurses as being diseased. She very reluctantly assumed its care at the urgent solicitation, even command, of the visiting physician, who was, she said, aware of the fact that the child had a sore

mouth. Owing to the extreme condition of debility of these two children, they were sent to a suburban branch of the institution, where it was thought that the fresh air might be beneficial. Here the diagnosis of syphilis in each case was entered; still they were each for a time nursed by a woman, and after that they were fed by the bottle until their death, which occurred on the nineteenth of June.

Let us now study the history of the woman who nursed these children. She entered the hospital on the 5th of May, and between that date and the 20th of that month, she gave her breast to two syphilitic children. From the fact that her chancre or initial lesion of syphilis appeared on the nipple, it is evident that the contagion took place through that organ. As she nursed these children who had sore mouths, it is certain that from either one of them she derived her contagion. Knowing as we do that the first incubation period of syphilis ranges between 7 to 10 days at the shortest, and 60 at the longest, we apply the fact to this case. Thus the nursing of the suspected children occurred between the 5th and 20th of May, and the appearance of the chancre was on the 28th of June, so that, supposing that the contagion was due to the first child which she nursed about three or four days, the incubation period would be 50 days. This, though somewhat long, considering the great vascularity of the nipple, and the favorable condition which it offers to the transudation of infecting fluids, is fully in accord with the well-demonstrated law of syphilis. Again, supposing that the contagion was derived from the second or foundling child, the incubation period would be shorter, it being nursed later: it would then be from 42 to 47 days. This is reckoning that the child was nursed from the 10th to the 20th of May. Such being the case, as the woman was healthy, and had not, of course, given the breast to any child before her confinement, nor had that organ been subjected to any unnatural contact, contagion certainly did not occur prior to her entering into the institution. Following up the woman's history, we find that she notices nothing unnatural as to her breast, and that she nursed only her own child until June 26th, when for two days she gave the breast to the child of a lady, who temporarily hired her as wet-nurse. Two days after this she noticed that her nipple was sore, and she naturally, but of course erroneously, at-

tributed her trouble to the lady's child. As her lesion, was syphilitic, we are able to positively state that she did not derive the contagion from this child, for only two days elapsed from the date of its nursing by her to the appearance of the chancre, which is too short a time for the development of syphilis. The circumstance of the woman wrongly attributing her contagion to the last nursling cared for, we shall find later on to present important points. The plain facts, then, of this case are these : A perfectly healthy woman, free from syphilis, enters a lying-in institution in order to be cared for during the period of childbirth. Being unable to pay, she consents, according to the rules, to nurse one or more children of the institution in addition to her own during her stay. To her are assigned two children, which at the time have sore mouths, and which soon after die of syphilis. She nurses them for two weeks, at the same time that she nursed her own child. In less than six weeks after this stay in the hospital a chancre develops upon her left breast, and it is very necessary to add here that she always gave the left breast to the strange children, and *never* the right one, which she reserved for her own child exclusively. From these facts we are warranted in the conclusion, and such is supported by the well-known laws of syphilis, *that the woman was contaminated in that hospital.* She certainly did not receive the contagion prior to her entering, for, apart from the improbability of the fact, her statements prove, as has been said before, that neither of her breasts at that time were subjected to any process by which syphilis could be communicated. It is certainly positive that she did not become syphilitic from the child nursed two days before the appearance of her chancre, as such an incubation would be too short for syphilis, and besides this the child was without a blemish. All the circumstances and the scientific facts deduced therefrom point to the two sickly children in the hospital as the sources of the syphilis. It is impossible to determine which one was the infecting agent ; still such knowledge is unnecessary, since we have the undoubted fact that the syphilis was communicated by one of the children in the institution. Now upon this fact hinges a number of serious considerations. Let us endeavor to learn how this syphilitic contagion was caused. We have the fact as given by the woman, who did not know its full weight, that the two chil-



dren had sore mouths. As they were subsequently pronounced to be syphilitic, it is fair to suppose that the soreness was really due to mucous patches, the secretions of which we know to be extremely contagious. It is to be inferred that at that time the attending physicians in the hospital did not recognize the syphilitic condition of the infants, as the woman was led to believe the children had only a simple nursing sore mouth. Certainly that is all they were treated for at that time. As far as I can learn, upon careful inquiry, syphilis was not suspected nor settled upon as the disease affecting the children until they reached the suburban hospital. The fact now is perfectly plain that a syphilitic condition of the mouth was regarded as of simple nature, and the result was that a woman became a victim of syphilis. This is a serious circumstance, and its occurrence is very significant, as it suggests to us that under the present regulations of some of our infant charities there is a great danger of syphilis being transmitted to nurses, and of course they may further communicate the disease. It becomes, then, an interesting subject for study to determine exactly the sources of the danger, and to settle upon such suggestions which, if properly carried out, will prevent in future such sad accidents. Now we know how very prevalent syphilis is, and that in infant asylums many cases of infantile syphilis are to be found. We also know that mucous patches in the mouth (I shall speak only of them in this study) occur very frequently in hereditarily syphilitic children. From my own observation I am able to say that nearly 70 per cent. of all cases of hereditarily syphilitic infants have their buccal lesions, which we know to be equally as powerful in conveying the contagion of syphilis as is the secretion of its initial lesion. These being the facts, we see that we have here, I hope I may say not an unsuspected, but certainly a prolific source of syphilitic contagion. As these lesions occur in the mouth, it is plainly seen how readily their secretion is conveyed to the nipple, the tissue of which presents every facility to the implanting of the syphilitic poison. These facts cannot be overlooked; their significance demands careful attention on the part of the authorities, both lay and medical, of these institutions, for if they are unheeded the benefits of such, when tried in the balance, become very doubtful. I am loath to arraign any set of gentlemen, and accuse them of neglect;

but I am certain of the fact that, in several of the infant institutions, this mode of the transmission of syphilis is not fully, I will not say understood, but rather appreciated and provided against. With this serious danger of the occurrence of syphilitic contagion in these institutions, too much care cannot be exercised in its prevention. Under the rule that a woman shall nurse one or more infants besides her own, how great is the danger that she will become syphilitic, and then that she will transmit the disease to her own child. Here we find that the evil is double. In the present case the woman, very luckily for her infant, reserved one breast for it exclusively, hence it escaped, for I was unable to find any trace of syphilis in it. Still it might have received the contagion from its mother. Of such cases there are unfortunately many precedents; for there are in French journals the records of cases in which not only the nursing infant was affected, but also several members of the family received the contagion from the chancre in its mouth by kissing it, the infant having gotten its chancre from its mother's breast, upon which a similar lesion existed caused by mucous patches in the mouth of a tainted nursling. Then, again, the liabilities to contagion do not end here. A woman having plenty of milk (particularly among the lower classes) very frequently nurses other children, whose mothers do not have enough, or they may engage themselves, after they leave the institution, as wet nurses. Here we see what dangers are run. In the early stage, and in some instances throughout their whole course, chancres of the breast are very slight in appearance, and often regarded as of simple character; sometimes as a pimple or again as a slight chafe or fissure, hence their danger is not feared, and syphilitic contagion may result. These are not imaginary circumstances, for I have before me while writing details of cases in which these facts were observed in France. Turning again to the case which is the text of this paper, we find that this woman engaged herself as a wet-nurse; fortunately for the child attended, the woman was yet in her incubation period, and her chancre had not yet appeared, so the child escaped. The woman says that for a short time she regarded it with no apprehension, consequently it would not have prevented her from giving that nipple to the child, for we know that these women nurse children even with breasts quite sore. Further

on I shall make some practical suggestions apropos of this period of incubation. These, then, are the dangers, and it certainly may be said that they are of formidable character, of syphilitic contagion between nursing children and nurses. Let us see how they may be in a great measure prevented. But in this direction we meet an obstacle of some importance in practice, that is, the very great difficulty of determining in some cases whether or not a certain sore state of the mouth is or is not due to syphilis, or whether it is simple in nature. Indeed I think it will be conceded by those who see many cases of simple nursing sore mouth, which are so very prevalent in large institutions, that their diagnosis from syphilis, particularly when severe in form, is sometimes difficult; indeed in cases in which false membranes form over the ulcerations, it is almost impossible, unless concomitant symptoms and lesions are taken into consideration. The scope of this paper does not permit me to fully consider the various features of the simple and specific sore mouths of infants, and I shall but briefly allude to certain general facts. In many simple cases of stomatitis no suspicion of syphilis is liable to occur, as the benign character of the buccal lesions is apparent, therefore there is little liability to error as regards them; but in the severer forms, in which there is sometimes great systemic debility, and extensive ulceration, with patches either membranous or parasitic, there is much danger of error. This danger is rendered all the more marked as certain changes are liable to take place in the syphilitic lesions in the mouths of children, which mark their character. If we had only mucous patches in their typical form to recognize in the case of syphilis of infants, an error of diagnosis would not often occur to an educated physician; but unfortunately these lesions sometimes undergo rapid and extensive metamorphosis, losing their peculiarities of appearance, and coming in the end to look very much like the severer forms of stomatitis and aphthous sore mouth. I think that observers who have studied this matter will agree with me as to the pathological resemblance. Still I think there are certain facts in the lesions themselves which will assist the observer. In syphilis, the coryza which most constantly accompanies the mouth lesion is much more severe, and the snuffling much greater, and there will be observed a tendency for the

syphilitic lesions to develop at the angles of the mouth; and there to induce ulceration, which may extend to the integument. This as a diagnostic point is of much importance. In syphilis the focus of the inflammation is developed upon the tongue, the fauces, and the region named, whereas, in the simple form of sore mouth, the tissues generally, except the throat, are involved, and the gums are affected, while they most constantly escape in syphilis. Again, in the simple form of trouble, the sulci between the lips and the teeth are often implicated, and in syphilis it is usual for them to escape.

But in all instances we have to fall back upon the history of the case, and upon the existence of other lesions, and here we generally have features and facts which are satisfactory. In hereditary syphilis it is very rare to observe buccal lesions only; we shall generally find dermal manifestations, and especially do we very most constantly see anal mucous patches. If such unequivocal lesions are found, the diagnosis is conclusively settled. It must be remembered that in some cases of hereditary syphilis, the cutaneous lesions are very slight and ephemeral, and in a hospital they might escape the observation of the attendants, especially where there are so many children. Too much care cannot be exercised in this matter of diagnosis, for we see that upon it in many cases depends the future health of previously healthy women. Now, then, as we have, I think, fully, and I hope satisfactorily, demonstrated the facts of the prevalence of syphilis in institutions where a large number of infants are congregated; of the frequency of occurrence and dangers of mucous patches in the mouths of these children; of the very frequent occurrence of nursing some mouths parasitic and non-parasitic, and of the great liability to mistakes in diagnosis which exists in many cases between simple and specific buccal lesions,—having, then, demonstrated these facts, we see that they have a very practical import. Thus they show us that more than ordinary care is required in their diagnosis and treatment, in order that errors may not occur, and that syphilis may not be communicated to those women, who, according to requirement, nurse more than one child.

Now, then, when these children are thus given to healthy women, it should first be ascertained that the infant is free from syphilis; should it have a sore mouth, that such is un-

doubtedly of simple nature. The greatest care should be exercised in the examination of the children, and this duty should be performed only by a physician. An orderly of the institution should not have the power of assigning an infant to any nurse, and, as I have said, the attending physician should satisfy himself fully of the freedom of that infant from syphilis. In a case of doubt the child should be bottle-fed until it is pronounced free from syphilis. This care in the assignment of children to nurses cannot be too rigidly enforced nor too sedulously carried out, for as dangers are numerous and powerful, a proportionate amount of circumspection is necessary. Indeed I think if error is made, it should be on the safe side, and that it would be well to segregate temporarily all cases of severe and doubtful mouth affection. During this period of uncertainty they should be bottle-fed, and each child should have its own utensil, which should be used for it alone, for the reason that syphilitic secretions might be retained on the india-rubber nipple used, and thus the disease would be communicated to an infant having only simple sore mouth. This suggestion is a most important one, and is thus rendered prominent because it is highly probable that among a promiscuous number of children with sore mouths, there possibly might be some afflicted with syphilis. In this event, if the utmost care is not taken, that disease might be communicated. Let us put the matter practically. Suppose, for instance, that among a large number of children with affections of the month, and in consequence fed by the bottle, that a nipple and bottle was not reserved exclusively for each, but that they were used promiscuously,—if, as is very probable, there were among them infants with syphilitic mouth lesions, how easy could it be for the contagious secretions of such to be retained upon the nipple, and from this implanted in the mouth of the next infant to which it was given. Infants having sores in the mouth are, for that reason, much more liable to contract syphilitic contagion than healthy ones are. This mode of *mediate contagion*, as it is called, is not at all improbable, and though we have not examples of it under the precise conditions now spoken of, there are numerous cases of syphilis on record in which the disease has been conveyed by the presence of the virus upon some utensil, as, for instance, those for drinking purposes. This prob-



able mode of the transmission of syphilis should be fully borne in mind and acted upon in infant institutions. I am aware that the routine of segregation of children and of their temporary nursing by the bottle would cause an extra amount of labor, and that strong objections might be urged against the use of the bottle, but I think that there is a great need for it, and that it would produce good results. If in a year two women were spared syphilis, I think it might be considered to have worked well. I am led to believe that in some institutions there is a laxity which needs immediate reform; that duties are assigned to orderlies and ward nurses and other women who are not fitted by their education for such cares.<sup>1</sup> Then, again, another important point is brought out by the case which I have reported. According to the woman's story, which I have believed to be true, as it has been verified in all of its particulars by Dr. Loring, a foundling infant was received in the institution, and immediately assigned to her as her regular nursling. This fact conclusively shows that sufficient care was not used; indeed it confirms in our mind the suspicion of laxity. It will be remembered that the infant referred to was a child having the look of a little old man, and that the woman was very loath to care for it, and only yielded when almost peremptorily ordered to do so. The circumstance calls to our mind forcibly the fact, that in hereditary syphilis its manifestations are not usually present at birth, but that generally they appear about the third week, and even later. This delay, as we may call it, in the evolution of syphilitic manifestation in infants suggests to us the practical point, that in the case of a child whose history is not known, as, for instance, particularly that of a foundling, or again one in which it is doubtful, that we may be unable to determine whether it is syphilitic or not.<sup>2</sup>

<sup>1</sup> In some of these infant institutions, women, zealous in the work and indefatigable in action, come to occupy positions which require, on occasions, an acquaintance with medicine which is not possessed by many of the sex, and in matters of assignment of patients they have more power than either resident or attending physicians. The present instance shows that in all such matters the medical man should be supreme in power.

<sup>2</sup> In many cases, however, even when nothing is known of the antecedents of the infant, a careful examination of its whole body will reveal its true condition. Though syphilitic manifestations are somewhat late in showing themselves, in the hereditary disease there are in many cases such facts to be gleaned

hence that, if assigned to a healthy woman to be nursed, it should be kept under observation, particularly as to its mouth, during a period of about a month. At the end of this time its condition can generally be determined with certainty. I think that the fact is very important indeed, and that its practical application should be enforced in infant asylums. There is, again, one more secretion by which syphilis may be transmitted from a tainted child to a healthy nurse; I refer to the blood. We know of a certainty that this fluid is contagious in the early stages of syphilis acquired or hereditary, consequently a danger, perhaps very remote, exists of an infant thus contaminating a nurse. This of course would be due to an abrasion or fissure in the infant's mouth giving exit to blood which might be planted on the nipple of the nurse. I merely allude to this as a possible source of syphilitic contagion between these parties, but I do not know of an instance of the kind, nor do I think such to be very probable. Still if a child is the victim of hereditary syphilis, it is a dangerous subject to any healthy nurse during the whole of its suckling life; if it has not at one period mucous patches in the mouth, such may develop at any subsequent time, and of course they would be highly contagious. Such being the facts, the natural deduction is evident, *that no syphilitic child should be given to a healthy nurse at any time whatever, certainly not when it has buccal lesions, as then contagion is almost inevitable; nor at any other time, even if the mouth is free from syphilitic lesions, as such are liable to appear at any time.* Apart from their extreme frequency, early in hereditary syphilis they often are developed later on, and have a marked tendency to relapse. These, then, are the sources of syphilitic contagion which are liable to occur to nursing women in large infant asylums.

In venturing to suggest that, in the cases of infants with suspicious sore mouths, bottle feeding should temporarily take the place of the human breast, I do not think I am advocating

even quite early as will either cause a grave suspicion of that trouble or lead to the opinion of the child's purity. Much stress may be laid on the condition of the body, whether emaciated or not, also on the typical senile appearance. The facts which I have elicited show forcibly that the whole body of each and every child should be examined in the presence of a trained physician; to this there should be no exception whatever.

an impossible or impracticable procedure ; nor do I think that there is any reason why, in a large institution, the care of which I have spoken, and which, I know, is somewhat minute, and requiring very considerable time and attention to detail, as well as great familiarity with the appearances of simple and specific lesions, cannot be exercised. I do not think that it is so intricate but that it can readily be used in all cases. I am aware that persons who advocate reforms very often suggest impracticable measures for their establishment, or, again, consider necessary thereto such sweeping changes that, being impossible, they fail in their object. I do not advocate any such measures. The suggestions which I make are entirely indicated by science, and called for urgently by the condition of affairs in these institutions, and, when once put into operation, I am convinced that the extra amount of care and work involved would not be irksome. At any rate, it is conclusively seen that under the existing rules of management, syphilis is very liable to be transmitted to the women who nurse more than one child ; consequently the authorities and medical boards of these institutions cannot shut their eyes to the danger, nor with the evidence before them of the fact which I have given, can they say that the present appeal for more care in management is a visionary dream of an alarmist, for I have stated only what I know to be true, and I think that I have not magnified the danger. In other countries this same accident has happened, and I judge that it is now provided against and the reform has been in each instance due to the plain published statement of facts by a medical man. I am not positive that the case of syphilis which I have reported is the only one produced by these two children, for I have learned that they were assigned each to a nurse in the suburban hospital. Not being able to trace these women, I am unable to state the result, but that the children were assigned to them is one which adds weight and force to what I have already stated.

I am disposed to think that strong objections will be made to the suggestion that infants, about whom there is a serious doubt of their being syphilitic, should be fed by the bottle, and that this part of the plan of reform will be looked upon with less favor than will be the suggestions for their regular and sys-

tematic examination.<sup>1</sup> I am an advocate of the plan of nursing children by the breast. I fully recognize the great advantage resulting from it, and I think that the plan adopted in these institutions by which motherless children are breast-fed, is a wise and beneficial one; but I think that I have shown a weak part of this general plan which requires modification of the whole. It will be urged that children having severe affections of the mouth are usually weak and debilitated, and that they particularly need human milk, and will not thrive, and that some may even die if they are bottle-fed. Still against this fact we have that of the dangers of syphilis to the nurse, and of its transmission in some cases to others. In France this difficulty is met by nursing such children by goats and goats' milk. This plan is perhaps adopted on account of the immunity which animals are said to possess to the syphilitic virus. I am unaware of the measure of success thus obtained, and I make the suggestion as it occurred to me, that in suburban branches of these hospitals many of these animals may be kept, and thus used at very little expense.

These, then, are the circumstances and dangers of syphilitic contagion between nursing infants and nurses, and it may be well here to remark that, in private practice, instances will be met with in which women become syphilitic by nursing an infected child with syphilitic buccal lesions. I have seen two such cases in which I traced the contagion clearly. One of these I report in the October, 1875, issue of the *Archives of Dermatology*, in an article on Syphilitic Chancres of the Breast. It is important that every practitioner should bear this fact in mind, as by so doing he may prevent the transmission of syphilis in

<sup>1</sup> It should be taken into consideration that if the examinations of infants are made only by physicians in a very thorough manner, there will be even in large institutions not very many cases of mouth lesions in which the doubt will be so great as to require the segregation of the child and its temporary artificial feeding. Of course this should only be done with care and caution, and the education and skill of the physician will be especially called into requisition. Should he not be thoroughly conversant with the affections of the mouth, which are so frequent in infants, he might carry his case and precautions so far as to put every infant with buccal trouble on the bottle. This need not be at all, for, as I have said, if care and skill are combined, the number of doubtful cases will be few. This greatly does away with the objection of bottle feeding.

many instances. It has often been a matter of surprise to me that this point has not been more forcibly and practically brought out in the text-books, as the danger induced is one which may befall any physician, or desolate any family.

*The Dangers of the Transmission of Syphilis from Nurses to Nursing Children.*

Let us now consider the dangers to which the community is liable from nurses with syphilitic chancres on the breast. It has been shown in the foregoing study, that nurses not infrequently have on their breasts syphilitic chancres. Not only may they become thus infected in infant hospitals (certainly in them by far the larger number of cases occur), but also they may thus become affected accidentally in the manner just pointed out. It is readily seen that these women may prove to be more or less extensively dangerous in proportion to the number of children to which they may happen to give the breast. Such nurses are very dangerous at two periods of their syphilis—first, in the period of incubation of their mammary chancre; second, in that state of development of the lesion in which it is as yet either an abrasion or a slight fissure or a small papule. When fully developed they usually, by their size and ulcerative tendency, prevent lactation, and attract such attention as leads to the discovery of their nature. Let us look at these points practically. After the deposit of the syphilitic virus on the woman's breast, a varying length of time elapses before the syphilitic chancre shows itself—called, as we have said before, the period of incubation. Now as there is in this person no perceptible morbid condition of the breast, the woman imagines herself healthy, and to a person unacquainted with the dangers of syphilis among this class, would undoubtedly be regarded as a proper nurse. This is well shown by the case of the woman McNally, who engaged herself in that capacity during the period of the incubation of her mammary chancre. Fortunately for the child nursed by her, she ceased to care for it a day or two prior to the development of her lesion, and thus it escaped syphilis. If, however, the woman had kept on as its wet nurse, or, again, if she had been employed a week later, when she regarded her lesion as of a trivial nature, this infant would undoubtedly have become syphilitic. This, then, opens to us a danger of great magnitude



and of insidious nature, and suggests to us that too much care cannot be exercised in the selection of a wet nurse. In this period of the incubation of the chancre, no one, unless he is familiar with the children which have been nursed by the woman, can have any suspicion of her being on the verge of syphilis; consequently the only reliable means of ascertaining whether she will probably develop a mammary chancre later on, *is to inquire into the history of all children nursed by her within a period of two months.* If she has nursed infants which have died, it is necessary to ascertain beyond a doubt the cause of death, and to direct the examination very searchingly in the direction of lesions of the mouth, skin, and nose. Should the facts developed appear at all suspicious, such a nurse should not be employed to care for a healthy child until a period as long as that of the incubation of syphilis shall have elapsed. At the end of that time she can, if her breasts are normal, be regarded as healthy. Again, if the woman has at certain times nursed children temporarily, the same inquiries should be made as to their condition. This amount of necessary care and attention is not at all overdrawn, for the reason that we know that nurses are so liable to become infected, and that for a time their syphilitic chancres are of such a seemingly simple character as not to attract attention. I can well illustrate and confirm what I have here advanced by the case of the woman McNally. Suppose she, as many women do, had remained in the hospital for a much longer time than she did, and then, wishing to earn money, or for any other reason, had engaged herself as a permanent wet nurse, she would almost inevitably have transmitted syphilis to her new infant. If, in her incubation period, she only had been examined, and had no questions been asked as to her antecedent nursings, she would, being then strong, well-built, healthy, and without any blemish on the skin, have been regarded as a very fine wet nurse, indeed a more than ordinary one. The aspect of the case, however, was far different two months later. The practical conclusion, then, is this: that in taking wet nurses from these various institutions, and from agencies for such, there is a very grave danger that women in whose systems syphilis is then being developed will be employed; therefore the danger should be met in the manner I have suggested. The next point to be consid-

ered is the fact of the seemingly simple nature of the syphilitic lesion of the breast in its early stages.<sup>1</sup> Nursing women are so liable to have little ulcers, fissures, and abrasions, that usually they do not attract marked attention. Unfortunately the syphilitic lesion in its first stages resembles strikingly these simple affections; hence their danger may be unsuspected. This being the case, the doubt being so serious, all such cases should be watched for a long period with great care, and in the meantime no infant should be exposed to the risk. Of course if such a nurse was submitted to a physician for his judgment, he would, for the reasons given, pronounce it a doubtful case. These, then, are the dangers of syphilitic contagion which both nurses and children are liable to, and it will be seen that they are numerous and insidious. Considering the nature of the disease, and, under these circumstances especially, its danger of being propagated to many others—it being developed really, in many cases, in the midst of the family circle—our most watchful care is imperatively demanded, as private physicians, to prevent, as far as we can, all cases in our own practice, and, as members of infant hospitals, to be alive to the dangers which have been pointed out, and to use diligent and continual effort for their suppression.

125 E. 12TH ST., NEW YORK, Oct. 1st, 1875.

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## ON THE ETIOLOGY AND PATHOLOGY OF AREOLAR HYPERPLASIA OF THE UTERUS.

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IN accordance with the "*New Basis for Uterine Pathology*," which formed the subject of my paper in the last number (August, 1875) of this Journal, I propose here to attempt an explanation of the pathological phenomena of "areolar hyperplasia," as we now, since the publications of Dr. Thomas, understand that term.

<sup>1</sup> See an article by me in the October number, 1875, of the Archives of Dermatology, entitled Clinical Features of Syphilitic Chancres of the Breast.

It is almost needless to say that the principles of biology upon which my "new basis" rests are those given to the world twenty years ago by Herbert Spencer (in his "Principles of Biology," 1866, and "Principles of Psychology," 1855)—principles which have been so generally admitted and so frequently elaborated by other writers, that they have now become common property in the treasury of scientific knowledge.<sup>1</sup>

The main principles or facts upon which the theory of the present paper rests, may be thus briefly stated:—

1. Normal functional activity secures perfect structural integrity (other things being equal); but total absence of function *necessitates* organic atrophy.

2. In the structural integrity of the active organ, as also in the atrophy of the idle one, we recognize a *conservative adaptation of structure to function*.

3. Such conservative modifications of structure naturally follow a typical course, and are then usually latent and extremely slow in their development.

4. The *primary* causes of *atrophy* are such modifications of environment as fail to bring the function of the affected organ into play.

5. Conservative modifications of structure of the kind specified can only reach their naturally designed typical termination, when the *new environment remains constant for a long time*.

6. Sudden vacillations of environment, especially if they are very decided, and often repeated, lead to inflammation of the modifying organ, particularly if the organism is at the same time chilled by exposure to cold; and in the absence of inflammation, they interfere with the typical source of the structural modification, causing it to assume a half-and-half, mongrel construction, in part adapted to a plurality of environments, but wholly and perfectly to no single one.

I must next call attention to the more common and decided anatomical phenomena of organic atrophy. They are these:

- I. In *mucous membranes*, loss of epithelium (by desquamation and fatty degeneration); reduced vascularity from oblit-

<sup>1</sup> In my first publication on this subject (see this Journal for November, 1872, p. 438) I could not forbear referring to Spencer by name, but deemed it unnecessary to do so in my last production.

eration of blood-vessels and their conversion into fibrous tissue (hyperplased connective tissue).

II. In *muscular structures*, gradual disappearance of muscular tissue and its supplantation by connective tissue; later transformation of the new connective tissue into tissues resembling successively fibro-cartilage, cartilage, and bone. Coincident induration of organ and obliteration of many of its blood-vessels. Impaired contractile power.

III. In *serous membranes*, loss of epithelium on surface; agglutination of opposing surfaces together, from adhesions formed of connective tissue; impaired mobility of affected organ and obliteration of its vessels.

IV. Organs designed for active function have full supply of blood; when they do not perform their function blood accumulates in them, leading to congestion; prolonged congestion leads *invariably* to hyperplasia of connective tissue, and this last in time supplants the normal tissue elements of the congested organ.

In accordance with these principles I now propose to venture the somewhat startling announcement that the reproductive apparatus (I shall confine myself hereafter to the *uterus* alone) of the celibate female begins, after puberty, to *undergo a process of more or less latent, very gradual, organic atrophy*, which is naturally designed, when it shall have reached its completion, to adapt the organs concerned to the total absence of function entailed on them by the abnormal environment of protracted celibacy.

I have said this atrophy is *more or less latent*:—not entirely so, however, be the case ever so typical a one. And why? Because the change of environment has necessitated so *very wide* a functional departure from the normal state. With the reproductive function there are no intermediate grades of functional exercise, such as belong to digestion, respiration, etc. Naturally each reproductive act is complete in itself. A woman is either pregnant or not pregnant: we cannot say she is more or less so. She is either a mother or she is barren.<sup>1</sup>

<sup>1</sup> Abortions are *abnormal*; and while a female may bear one child or many, and have single or multiple births, these facts do not impair the meaning of the text.

Environing changes necessitating so *extreme* a deviation from normal functional action can never be completely latent.

It is from this latent and partial, but progressive atrophy, from which arises the more marked evolution of connective tissue, to which Dr. Thomas has given the name of "areolar hyperplasia" (as will be presently explained), and from it also, as a beginning, the great majority of uterine diseases and displacements may be said to have their origin.

At present, unfortunately, direct anatomical evidence of this view is wanting. Histologists have not yet shown, nor indeed, that I know, have they yet sought to find the difference in elementary structure between a uterus that has reached mature development, and is prepared for procreation, but which has not yet been the seat of menstrual congestion, and one that for a series of years *has* been the seat of the menstrual process. I make no doubt, however, that evidence showing that in the latter case there exists a partial substitution of the normal tissues by connective tissue is forthcoming.

In old cases of "areolar hyperplasia," which are nothing more than cases of interrupted or disturbed instances of what was, before pregnancy, latent atrophy, we find the anatomical conditions very striking. Klob thus describes the second stage of the disease: <sup>1</sup> "The parenchyma on section appears white, or of a whitish-red color, deficient in blood-vessels, from compression of the tissues by the contraction of the newly formed connective tissue, or from partial destruction or obliteration of vessels during the growth of tissue; the firmness of the uterine substance is also increased, simulating the hardness of cartilage, and creaking under the knife."

Under the old term, "chronic metritis," Schroeder <sup>2</sup> thus describes the late anatomical changes of areolar hyperplasia: "When the proliferating process has ceased, the newly formed connective tissue undergoes cicatricial retraction, the vessels become imperforate, and the young mucoid connective tissue becomes firm and fibrillated. The uterus then again diminishes in size, and on section exhibits an exceedingly firm, almost cartilaginous tissue, which creaks under the knife, and has a white, anæmic, cicatricial appearance."

<sup>1</sup> Quotation by Thomas, *Dis. of Wom.*, 4th ed., p. 288.

<sup>2</sup> Ziemssen, vol. v., p. 106.



He further states (*Ibid.*, p. 110) that in certain cases "the affection proceeds to induration (described by Scanzoni as the second stage of chronic metritis). The newly formed connective tissue undergoes cicatricial retraction, the uterus becomes harder and smaller, and premature amenorrhœa occasionally supervenes. The worst symptoms, the acute exacerbations, however, cease, and this process must therefore be considered at all events as a relative cure."

Simpson mentions the transformation of the uterus, in long-standing cases of "chronic inflammation," into a substance resembling bone.

That these are the usual final terminations of the disease, and that they usually entail sterility, or a tendency to frequent abortions, is well known.

To show that such cases begin from having been preceded by the proliferation of connective tissue incident to the gradual atrophy that occurs from prolonged celibacy, we remark:

*First.* That they only or chiefly occur *after the female has borne a child*. Now if the uterus were perfectly normal before pregnancy why should the occurrence of gestation and delivery—the natural functions of the uterus—generate disease? Is it common for organs to become diseased by the simple performance of their natural office? Certainly not. And we can only account for the occurrence of such postpartal disorders by admitting the existence of previous structural change—the change of partial latent atrophy, which had in part adapted the organ to total idleness, and in so far rendered it unadapted to the reproductive function.

*Second.* The disease ("areolar hyperplasia") is less frequent in females who become pregnant early in life than in those who only do so after having passed many years of celibacy, or who, after having borne one or more children early, become widows, and remain sterile for a series of years before again bearing children. The woman who, very often correctly, dates all her uterine ills from labor, when she was comparatively well before marriage, is one in whom the atrophy of celibacy was gradually progressing, when the sudden change of environment (marriage) entirely changed the order of things, and necessitated a reconstruction of the uterus in order to fit it for

reproduction. Such sudden vacillations of environment are always more or less disastrous.

*Third.* Partial atrophy, prior to pregnancy, *leads to subinvolution after delivery.* Under the stimulus of gestation the new growth of proliferating connective tissue (which, before impregnation, had already supplanted some of the muscles and vessels, and welded together the fibres of the former so as to impair their function and development) itself undergoes a much more rapid growth, just as do the tissues composing fibroid tumors under the same condition; so that while the remaining normal tissues of the womb are undergoing their exalted evolution, the proliferating connective tissue does the same. Thus the uterus loses some of its elasticity, and its muscular walls, stiffened by the parenchymatous layers of connective tissue, cannot perform their function thoroughly well, hence follows, after delivery, *imperfect contraction of the uterus*, with consequent hyperæmia, subinvolution, and still additional productions of connective tissue. Such a case is then recognized as "induration," "chronic metritis," or "areolar hyperplasia," due to pregnancy, when in reality it is but an exaggerated phase of a previously existing structural change.

Should pregnancy again occur, such a uterus would be less than ever fitted to perform the reproductive office, hence there would arise the greater liability to abortions; or, if the gestation went to term, the imperfect contraction of the uterus after labor would be further manifested by the occurrence of "*after-pains*," and to which, for the reasons stated, *primipara* are less subject than multipara. In other words, the "areolar hyperplasia" of Dr. Thomas arises as follows: During prolonged celibacy, and consequent prolonged menstrual congestion, a moderate amount of connective tissue begins in the uterus as a formative tissue—a new growth; during a succeeding pregnancy this new growth proceeds rapidly *pari passu* with uterine development; after delivery (owing to imperfect uterine contraction, subinvolution, and increased congestion) the new growth of connective tissue proceeds even more rapidly than ever, and the womb is now recognized as an enlarged, indurated organ, while the usual symptoms of uterine disease become well marked.

In the case of a virgin uterus, the environment of celibacy remains constant, the organ is slowly, and without much incon-

venience to the female, wending the even tenor of its way towards a structural atrophy that is naturally designed, in the end, to adjust the organ to its total want of function ; but when this has been progressing for years, the sudden change of environment that entails reproduction is most unfortunate as regards the structure of the uterus ; under such circumstances the female not infrequently becomes a decided invalid, and since she was comparatively well while a virgin, now attributes all of her troubles to marriage, and in this *she is right*.

It may be asked if every woman who lives single for a considerable time becomes so unadapted to procreation as that the performance of that function induces disease, why is it that the occurrence of areolar hyperplasia and its attendant ills is not more frequent ? I answer that many circumstances (besides time) modify the *extent* to which the latent atrophy of celibacy has progressed, and so great are the recuperative powers of Nature, that, amid all the confusion of organic changes that take place, the organ can still often so far readapt itself to its changes of environment as that the female gets along well enough to escape the clutches of the gynecologist. If, however, I am correct in my explanation of subinvolution and "hyperplasia" after delivery, its lack of frequency cannot be alleged against me. "Arrest of involution," says Dr. Thomas,<sup>1</sup> "of the puerperal uterus is an occurrence of *very great* frequency. It constitutes the *chief cause* of *all* chronic uterine disorders, and for this reason its importance cannot be over-estimated. Until this subject receives the attention which it deserves, the present confusion as to the causes, pathology, and general features of chronic metritis, which helps to weaken uterine pathology, must continue."

*Fourth.* During the prolonged functional inactivity that attends protracted celibacy, it can hardly be otherwise than that the folds of serous membrane (naturally designed to allow a free mobility of the uterus during its enlargement while pregnant, and its perfect contraction after delivery) lose something of their normal structure, becoming to a certain extent fibrous and adherent to each other, just as do the pleural surfaces of a sedentary consumptive who does not fully expand his lung, and the parts about an articulation (destined to secure its free mo-

<sup>1</sup> Dis. of Women, 4th ed., p. 264. The italics are mine.

bility) when the joint is kept for a long time at rest. So-called "false adhesions" from development of fibro-connective tissue is one of the common phenomena in serous membranes belonging to organs undergoing atrophy from want of function. That such perimetric adhesions (in the case of the uterus) occur in celibate females as a latent adaptive growth, is perhaps not yet proven by anatomical evidence; but that such adhesions are frequently found after death, when they were least suspected to exist during life, is unquestionable. It seems highly improbable that serous surfaces naturally designed for extensive mobility, when left in contact and at rest for a period of ten years (from fourteen (puberty) to twenty-four for instance), should *not* become more or less adherent to each other. It is not impossible that the abdominal pains experienced during the later months of pregnancy by (especially) primiparous women, and which have hitherto been attributed exclusively to the resistance which the abdominal walls offer to distention, may be found hereafter to be due, in part at least, to the stretching or separation of peritoneal adhesions that have occurred latently as the result of prolonged functional inactivity. Should such adhesions be found hereafter to exist, it would sustain the views I have presented as to the origin of uterine hyperplasia, for, as Dr. Barnes tells us, "it may be laid down as an aphorism that whenever the mobility of the uterus is arrested, whether the cause be external or internal, a degree of hyperplasia is the result. Thus, as in the case just mentioned <sup>1</sup> of perimetric adhesions, *imperfect involution* and a process of slow infarction follows." (Barnes on Women, p. 408.)

*Fifth.* The theory I have proposed implies that the reproductive organs of the celibate female have in some degree departed from the typical line of health, and that in truth every such celibate woman is consequently more or less a "patient."

However distasteful this admission may be, the question, "Is it a fact?" must be considered. Now, laying aside all preconceived prejudices, let us ask: Is such a woman perfectly well, or does she have *symptoms*? We answer: She is the victim of

<sup>1</sup> He is here referring to perimetric inflammation during childbed, still adhering, probably, to the idea that such adhesions are *always* of inflammatory origin, when in truth they are growths of an abnormal kind that become themselves the seat of inflammation.

uterine hemorrhage once a month, and to show that this bleeding is accompanied by unpleasant symptoms (all of us know it, but I quote from the latest authority (Schroeder in Ziemssen's Cyclop., v. x., p. 326) as follows) the *italics* being mine :

"Menstruation, *even when running a perfectly normal course*, has a disturbing influence upon the general condition. The women are slightly irritable. Disturbances of the circulatory apparatus and of the digestive organs, and particularly of the nervous system, are quite commonly developed; also slight dragging pains in the back and loins may be present without pathological changes in the uterus.<sup>1</sup> It may be regarded as highly characteristic that the women themselves say 'they are unwell.'<sup>2</sup>"

Dr. Schroeder follows this paragraph with the usual orthodox but inconsistent statement that "all such symptoms *can* (only) *be considered as physiological* as long as they are present to only a slight degree and do not lead to more serious disturbances of the system at large. In *many* persons, however, they lead to the most serious disturbances in bodily and mental condition," etc. So then : to be ill *a little*, is to be well; but to be *a little more so*, is disease. This is indeed strange logic. Who will draw the line which determines whether the patient is "well" or "unwell"?

In presenting in his work a chapter on "*Normal Menstruation*," preliminary to treating of menstrual disorders, Prof. Schroeder acknowledges that he is abandoning the plan "of considering the diseases of the separate organs *on a basis of pathological anatomy*," which he had till then adhered to, and goes on to state that "we must not fail to recognize the fact, that in the gynecological manual of the future, menstruation and its derangements may no longer find a place, for menstruation itself is to be treated of in lectures on physiology." I, however, affirm exactly the opposite, namely, that in the physiological manual of the future menstruation and its derangements will not find a place, but will be treated of in works on gynecology, when the missing link of "*pathological anatomy*" in the menstruating uterus will have been made out, and when, consequently, the apology of Prof. Schroeder will be seen to have

<sup>1</sup> I do not, but should like to know, what are Prof. Schroeder's grounds for this statement.



been supererogatory. It is difficult to see how any process whose description requires the enumeration of "*symptoms*," can be consistently embraced in works on physiology; and equally so to understand why, if menstruation is a perfectly normal phenomenon, it should be so elaborately considered in almost every work on uterine pathology.

*Sixth.* The mucous membrane of the uterus during the menstrual process *undergoes fatty degeneration*. In corroboration of this fact, I need only refer to the elaborate paper of Dr. Engelmann (see this journal for May, 1875, p. 40), in which he states that the fatty degeneration "involves not only the cells of the interglandular tissue, but also the blood-vessels and the glandular and surface epithelia. That these structural changes take place in the tissue coincident with the menstrual discharge, is unquestionably proved." Already, therefore, the light of a better understanding of the *pathology* of menstruation begins to dawn upon us—an understanding, too, be it noted, that is based upon "*structural changes*." But it remains to be decided whether these structural changes are purely physiological or whether they are pathological, whether natural or *unnatural*.

In his chapter on the "*Conditions of Involution*" (foremost among which is mentioned fatty degeneration) Rindfleisch<sup>1</sup> determines for them the general character, "that a gradual transformation and final destruction of the normal form goes hand in hand in them with a parallel *decrease and final extinction of the normal function of the parts*." This, of course, is generally admitted. We witness it very constantly in the decline of function and nutrition attending senescence—in the atrophy of old age. The same "decrease" and "final extinction" of function occurs at each menstrual period in the uterine mucous membrane. But can it be that this is the designed typical career—the natural functional and structural destiny of a membrane so highly elaborated in construction and so amply supplied with blood as is that of the uterus? The atrophy of senescence occurs from *diminution of blood*; hence, it is seen first and most prominently in the cornea and cartilaginous organs; in the uterine mucous membrane degeneration occurs at the very time when the *supply of blood to the part is most plentiful*. Again, if

<sup>1</sup> Text-book of Path. Histology, Kloman's Trans., 1872, pp. 32 and 33.

successive monthly renewals and degenerations of the uterine mucous membrane are in conformity with the *natural* design of its formation, let us ask, what good purpose in the organism does it accomplish? Of what *use* is this process? And if it is perfectly normal for this formation and involution of mucous tissue to go on for five or ten years after puberty, why not also for twenty or thirty years, or even to the menopause? Under such circumstances will it be said that the organ has performed the function for which it was naturally designed? Evidently not. The normal office of the mucous lining of the womb is its well-known participation (after impregnation) in the process of procreation. This is the only function for which it was naturally designed, and in the absence of which there is absolutely no other normal functional exercise for it to perform. Between the ovulatory periods the mucous membrane undergoes a normal hypertrophy preparatory to receiving an impregnated germ, and we know well enough that under the physiological stimulus of impregnation it is destined to undergo a still higher development into decidua vera, reflexa, etc., in accord with the designs of nature; but, impregnation not taking place, involution occurs in the membrane by fatty degeneration, just as it does in the muscular walls of the womb after delivery; with, however, the very manifest difference, that in the latter case it begins at the *end* of a natural process, the degenerating muscles having *finished the reproductive function for which they had been so highly developed*; while in the former the fatty degeneration takes place nearer the *beginning* of the normal reproductive function and *before the mucous membrane has at all accomplished the office for which it had been so highly developed*. Nature will not support the nutrition of a useless organ; in the physiological offices of the system she will allow no sinecures; hence we always find that organs performing no useful purpose become prone to disease, degeneration, and decay. It was in obedience to this principle that the learned president of the Clinical Society of London (Sir William W. Gull) remarked in one of his addresses before that body: "*Were I to make a man, I do not think I would put tonsils in him*;" and hinted that the extirpation of superfluous organs was "a grand prospect for the surgeons of the future."<sup>1</sup>

<sup>1</sup> "*Popular Science Monthly*," vol. i., No. 1, p. 122, quoting from "*Brit. Med. Jour.*"

Exactly on the same principle might I venture a somewhat analogous statement, to wit: *Were I to make an "old maid," I do not think I would put a uterus and ovaries in her.*

Under this head I may add, that the natural separation of the mucous membrane of the uterus that takes place at the end of pregnancy differs in a very important particular from that which occurs at the menstrual period, namely, in the circumstance that at "term" the mucous membrane has so slight or insecure a connection with the uterine walls that it can be separated without such violence as to cause hemorrhage; while at the menstrual crisis its adhesion is so exceedingly intimate and withal vascular, that it cannot be separated without the opening of blood-vessels by fatty degeneration and consequent bleeding. The provision therefore which exists for the natural separation at term does not attend the *un-natural* separation that occurs at the catamenial periods.

*Seventh.* Among the recognized exciting causes of "areolar hyperplasia" must be mentioned prolapsus and other displacements of the uterus. And we note here that such displacements, as a rule, are less frequent in virgins, more so in mothers. To say that they occur in the latter from the reproductive system of the female having become, by protracted celibacy, unadapted to childbearing, would be only repeating what has already been said. To explain further, therefore, we must remember that the vagina undergoes the same phases of increased development and involution as does the uterus. During each catamenial period it is congested; then occurs *some* hyperplasia of connective tissue, supplanting the normal muscular structure; pregnancy occurring, the muscles together with the new growth of connective tissue both undergo increased development. After delivery there occur imperfect contraction, subinvolution, and further areolar hyperplasia of the vaginal walls exactly as before explained in regard to the womb. In this way the physical tone and muscular contractility of the vagina is permanently impaired, so that it cannot contribute its usual support to the womb sufficiently to keep it in place, even if it (the uterus) were of normal size and weight, much less after it has increased in both particulars from coincident uterine hyperplasia.

Finally, it is not improbable that the other supports of the

womb,—the ligaments, perineum, and muscles composing the pelvic floor, which are naturally adapted to undergo certain structural modifications coincident with pregnancy when the latter takes place at the proper age, may be less disposed to go through those cyclical oscillations of development and involution, when they have become habituated to structural stability from long years of barrenness: for them, then, “the die is cast,” and the supple age of easy modificability is forever gone.

In conclusion, it may be added that the orthodox treatment of areolar hyperplasia by the highest authorities is exactly that which the views I have proposed as regards the pathology of the disease would have suggested. Many of our best remedies are but imitations of Nature, and the best philosophy of the physician is to find out what Nature is attempting to accomplish and assist her.

Areolar hyperplasia begins, as I have said, by the uterus attempting to modify its structure, so that it may be adapted to sterility and celibacy. To secure the completion of the process it is necessary for the *environment of celibacy to remain constant*, for the sudden call for function upon an organ partially structurally adapted to total idleness, leads to congestion and unnaturally rapid connective-tissue hyperplasia.

This, the physician imitates in his treatment by recommending abstinence from *coïtu* and the avoidance of pregnancy. Nature allows the disintegration of the useless mucous membrane by fatty degeneration, leaving a film of cicatricial fibro-plastic tissue in its place: the physician burns out the mucous membrane by nitric acid, and attempts to do it so deeply as to leave a thicker layer of fibro-plastic tissues, so as to prevent the re-formation of any new mucous membrane, and thus reduce the organ to such a state that absolute sterility is abruptly as certain as Nature in the course of years would also have made it.

Nature relieves the periodical congestions that occur during the excitement of ovulation, by the salutary bleeding of the catamenia; the physician relieves any discoverable congestions that may have arisen from the excitement of *coïtu* or other forbidden imprudences, by the salutary bleedings that follow scarification, leech-bites, etc.

The coincident discharges of vaginal and uterine mucus which also lessen congestion and keep the development of areo-

lar hyperplasia within the bounds of typical rapidity, are again initiated by the discharges produced by the glycerine plug, and cantharidal collodion applied to the cervix by the gynecologist. The same sort of drain from the uterus also is promoted, and the excessively rapid growth of connective tissue restrained, by clipping off a piece of the cervix with the scissors or galvanocautery. In this way the remainder of the organ will undergo a diminution in size, much in the same manner (says Dr. Thomas)<sup>1</sup> as enlarged tonsils will do when their faces have been shaved off. Thus the useless tonsil referred to by Dr. Gull, and the useless uterus of the celibate female, again meet, and rather unexpectedly, as regards their surgical treatment.

But whatever the treatment of areolar hyperplasia, it is well known to be very difficult of cure, and the best termination that can happen in cases to any great degree advanced is premature atrophy of the uterus and consequent absolute future sterility—a termination that is often sought to be attained by the gynecologist in vain, until Nature achieves it at last, by the redoubled tendency to atrophy that occurs after the menopause.

Perhaps the time may come in the exaggerated civilization of the future when females, to escape the ills of maternity and the menstrual aches of celibacy will submit to “normal ovariectomy” (which indeed has already been done), or to ligation of the uterine and ovarian arteries, but the aspirations of the gynecologist do not properly lie in this direction. His mission is to study the *natural functional career* of the reproductive organs, and recommend such a course of life to the female as will maintain them in the exercise of their natural functions only. At present both physiologists and gynecologists are profoundly ignorant as regards many important questions bearing upon this subject. None perhaps has been more carelessly treated than that which refers to the marriageable age of the young female and the frequency with which the act of procreation (and indeed of *coitu*) should be repeated. At first sight the theory I have suggested would seem to indicate that procreation beginning with puberty ought to be repeated every year or two until the menopause, so as to forestall any intermediate recurrence of the abnormality menstruation, which would necessitate, *apparently*, the bearing of twenty or thirty children.

<sup>1</sup> Diseases of Women, 307-8, 4th edit.



But this conclusion is too hastily drawn. It is questionable whether the so-called menopause would not occur much earlier than it does, if the reproductive powers of the female were exercised to their full, but still normal extent. Thus the atrophy of the uterus which usually occurs after the climacteric, might under those circumstances occur earlier, and coincidentally the processes of menstruation and ovulation might cease. In fact Dr. Barnes tells us that such cases have already been observed in "younger persons, in consequence of the rapid succession of labors."<sup>1</sup> But the whole subject needs a thorough reconsideration, of which it is hoped the papers on my "New Basis" may perhaps be a salutary beginning.

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#### ON THE SEQUELÆ AND PROPHYLAXIS OF SIMPLE TONSILLAR HYPERTROPHY IN CHILDREN.

By DR. BEVERLEY ROBINSON,

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CHRONIC enlargement of the tonsils is an affection so commonly met with amongst children, that the general practitioner is apt to overlook the importance of disturbed function of these glands, and to pay but little regard to their active treatment. So many are subjects of this disease, in a relatively slight degree, and so widely spread is the idea, that even in a somewhat aggravated form, the affection is one which will last only a few months, or years, and then in many instances disappear spontaneously, that it is considered almost, if not entirely, useless to interfere, or at all events to employ topically any very active medicinal agents. The cost in trouble, pain and anxiety, both to parents and children, in ridding little sufferers of their malady, seems to more than balance any evil consequences which will probably arise from its presence. Thus it is, that healthy and sickly children go on from year to year, bearers of this tumefaction of the fauces, without serious attention being directed towards it. Suddenly, however, some acute trouble of the throat shows itself, either idiopathic in its nature, or allied with one of the exanthemata, and then

<sup>1</sup> Diseases of Women, p. 402.

anxiety is at once excited, and this frequent condition of additional risk is considered with a greater or less degree of alarm.

Again, a child, who in its infancy has enjoyed good, or even robust, vigorous health, becomes pale, emaciated, loses appetite and strength, and is afflicted with spasmodic disease (laryngismus stridulus, asthma, etc.) of frequently recurrent character, and soon maternal solicitude is awakened, and the origin, or cause of the change earnestly sought for at the hands of the medical adviser.

Too often, we regret to add, the source of physical weakness may be found in the hyperplastic development of the tonsils;—and that this is true, cannot be doubted, when we fairly estimate the pernicious effects upon the general health, which surely follow in the wake of their diseased state.

What are the tonsils?

Histologists tell us they resemble in structure other lymphatic organs of the body. At the same time they inform us that they are characterized by differential features, which approach them to glandular structures. On the one hand, we find in the walls surrounding the central cavity of each one of the masses, which conglomerated form the entire organ, a number of closed follicles, similar to those that form a component part of Peyer's patches in the small intestine; on the other, we have a number of tortuous tubes not unlike in form and structure the ducts of acinous glands, and these tubes open either directly upon the mucous surface of the tonsil, or else into the lacuna of the lobules. Now, from this structure proceed two distinct functions: 1, the formative function, which relates to the constitution of the blood; 2, the glandular, which determines the secretion of a clear, viscid fluid. This fluid, when poured into the buccal cavity, by the contraction of the surrounding muscles in the effort of deglutition, serves to imbibe and lubricate the alimentary bolus.

The tonsils are situated, as you are aware, between the pillars of the fauces on either side, and when of normal size, although easily seen if the mouth be opened and the throat inspected, are not prominent enough to materially diminish the diameters of the isthmus, or interfere with essential vital functions.

How changed this statement becomes whenever the tonsils are diseased! First, and considered as lymphatic organs, their influence upon the transformation of the white blood corpuscles is imperfect, or null. Secondly, the secretion from the glandular structure is altered. It rapidly becomes cloudy and fetid, is apt to flow less easily from the orifices of the follicles, and in many instances these latter become blocked up completely with a white, sebaceous substance of cheesy consistence, which is also soon rendered putrescent. As a consequence the alimentary bolus, though lubricated in its passage through the bucco-pharyngeal opening, carries with it a quantity of morbid secretion, which rapidly sets up dyspeptic trouble, and absorbed into the circulation leads finally to unhealthy nutrition. Owing to the position of the tonsils, we can at once perceive how in cases of chronic enlargement, no matter what may be their intimate or structural characters, several important functions are morbidly affected in a greater or less degree. If the tonsils enlarge towards the median line, the faucial opening is often greatly diminished. At times, and when the hypertrophy is exaggerated in this direction, deglutition is fatiguing and occasionally painful. If the enlargement also takes place in the upward direction, partial obstruction of the posterior nares and compression of the Eustachian orifices are natural sequences, and hence we have difficult respiration and imperfect hearing, and on account of the relative immoridity of the soft palate, the voice takes that nasal and muffled intonation so familiar to every medical practitioner.

All the above phenomena may be present as well with adults as children. There are others, however, which relate almost solely to children, viz.: those which attach themselves naturally to arrest of development.

A well-known physiological law is expressed in the following terms: whenever an organ of the body during its period of growth is thrown into disuse, or cut off completely from its normal function by some mechanical obstacle, deformation and arrest of development, followed by atrophy, are natural sequelæ. Let us make the application of this law. Owing to the passage of a less quantity of air through the nasal passages, in the condition under consideration, a diminished volume also enters the larynx and lungs.

What is the result? The nasal passages do not increase in capacity proportionately with the growth of the child, and this lack of growth affects in an equal degree the palatine arch; consequently we find the olfactive and gustatory senses restricted. Again, the lungs do not receive a sufficient supply of oxygen, a certain number of their terminal vesicles are not expanded at all during ordinary inspirations, and those vesicles that are filled are insufficiently so, and the chemical blood changes which take place in the pulmonary structure are but very imperfectly accomplished. Besides, atmospheric pressure in the interior of the lungs, owing to the rarefaction of the air, does not fully counterbalance that from the exterior, and sinking in, with a peculiar deformation of the chest walls, is produced. This visible alteration in the chest is sufficiently notable to be in the estimation of some authors as pathognomonic of tonsillar enlargement, as a somewhat analogous appearance is of rickets. The physical development and proper growth of children thus affected is manifestly seriously impeded. Their nutrition languishes and their muscular energy and activity are markedly diminished.

Further, we may add, by compression of the large vessels of the neck cerebral circulation is at times incompletely performed, the brain becomes dull and apathetic, less inclined to exert itself, and suffers concomitantly with the body, from a condition which is local in origin and may at any time, with rare exceptions, be remedied by a ready and safe operatory procedure.

In the prophylactic treatment of chronic enlargement of the tonsils in children we attach great importance to first, the habit of cold bathing.

We all know how readily children "catch cold," and how frequently this *catching cold*, as it is termed, manifests itself upon the mucous membrane lining the faucial opening. It is to frequently repeated inflammations of the throat more than to any other single cause that is due the disease under consideration.

To guard our little patients efficiently then against the recurrence of colds, let us endeavor by bathing in water of low temperature to activate the functions of the skin, and thus to lessen the likelihood of localized mucous congestion.

Mothers, nurses, and even some of our professional brethren,

are too much disposed to underrate the value, or be fearful of cold ablutions. They recognize how useful a preventive agent against colds they have ever close at hand whenever adults are in question, but they do not believe this to be true of children, or if they acknowledge that cold baths might prove useful, they dread putting their knowledge into practice. They seem to consider that a small child is not sufficiently strong, that its vital forces are not energetic enough to be able to resist such an heroic method of treatment. They tremble for the safety of their nurslings, or children of a more advanced age, and cannot bear to carry out this rational therapeutic system.

And yet is it not manifest in our ever varying climate, where extremes of temperature so closely approximate, that in spite of every precaution being exercised which is dictated by maternal solicitude, children must of necessity be more or less frequently exposed to all the evils that result from draughts, or from cold, or excessive humidity in the atmosphere? Our object then should be to protect them efficiently from the adverse effects of our trying climate. Now, *hot*, or even *tepid bathing*, is, we believe, one of the main causes of recurring congestion or inflammation of the throat. And, we may ask, Can it be otherwise? Take a child of relatively feeble and lymphatic constitution and subject it to bad hygienic influences, viz.: surround it with an insufficient or vitiated supply of air, give it improper food, or cover it with badly adapted clothing, and will you not find that it gradually becomes more markedly strumous and sickly? Warm bathing is to be ranked in the same category. It is enervating, and takes away from bodily vigor.

The skin, it is true, is actively congested during the period of the bath and its capillary circulation greatly augmented, but just so soon as the ambient cold air infringes again upon the cutaneous surface, either directly or through the habitual wearing apparel, the blood supply is driven with increased force (owing to the rapid contraction of the small vessels of the integument) towards the internal viscera and mucous linings, which, in their turn, become congested, and remain so more or less *constantly*, unless by a superabundance of clothing the body is kept in an unnatural state of heat.

If the temperature of the water used in bathing is as low or



lower than that of the surrounding atmospheric medium, what a different physiological action takes place!

A temporary shock follows immersion or the use of the sponge filled with water, after which there is a short period when the surface temperature of the body is lowered. and then a natural warmth or glow takes place, the skin is reddened, its capillary circulation is heightened, and not merely in a temporary manner, but shortly becomes so permanently, and the interior organs are relieved of an overload of blood and greatly activated in their several functions. In the event of the natural warmth, which almost invariably succeeds the use of cold water for bathing purposes (unless it be continued too great a length of time on each occasion), not manifesting itself, we ought to have recourse to *friction*. And rubbing, gently at first and soon with a firmer pressure, the entire trunk and limbs, will greatly stimulate the action of the skin. The rubbing should always proceed from the extremities towards the heart, or in the direction of the venous blood-flow. The use of some fatty, or oleaginous substance of bland, unirritating nature is an excellent adjunct in carrying out the above treatment, and is especially of service where the skin is dry, offers a slightly scaly or furfuraceous aspect, and so gives evidence of imperfect nutrition, or lack of healthy power. Of course, in the above indications of prophylaxis, we would naturally include careful attention to the periodical, but not too frequent use of *pure* soap. The selection of this article has its importance, and we shall therefore particularly recommend Pears' transparent glycerine soap, which is manufactured in London, and is one of the best soaps of which we have cognizance.

Another subject to which we claim attention is care of the feet. They should be kept warm and dry. Cold, moist feet are an habitual and most efficient cause in producing hypertrophy of tonsils. Nowadays our people have finally been awakened to the necessity of warm and appropriate clothing for small children, if they hold to their continuance in good health. Happily, therefore, naked arms, legs, shoulders, and chests are not so frequently encountered as formerly.

Mothers are willing for the sake of permanent good to their offspring to abandon in this regard foolish notions of dress or fashion.

While, however, wise solicitude and care have been directed to clothing, properly speaking, how has it been with respect to the covering of the feet? How many little ones do we see walking the sidewalks of a cold, wintry day with light, paper-like soles to their shoes? And in wet weather, do we always find them better protected? True it is, that over-shoes, arctics, etc., are usually put on over the ordinary walking shoe, but is this the best way of securing warmth and dryness? Decidedly not. Excepting those times when there is much slush or snow upon the ground, we consider the cork-sole shoe far superior to the gutta-percha, or other style of impermeable over-shoe, because, 1st, it keeps the feet quite as dry, 2d, warmth is secured without that constant moisture of the feet almost inevitable with over-shoes on account of arrest of insensible evaporation.

Another way of preventing the soles of shoes or boots from absorbing water is to cover them with a coating of oil (castor-oil is the best), which will render them impermeable and may be used without risk of injurious effects to the wearer.

Thick-sole shoes will of course lessen the facility with which the feet become cold or wet. Allowing, however, that the feet are prevented from getting really wet, it must still be admitted that the soles of shoes are frequently made cold and humid. Now this circumstance alone will promote rapid and direct absorption of heat from the feet, and the effect will be considerably increased by the evaporation due to atmospheric action, which is constantly continued.

Colds may also be rendered somewhat less frequent in young children, and attacks of acute tonsillitis may, we are told, be occasionally aborted by putting a plug of cotton wool in either ear whenever they go into the open air during a period of excessive cold or high winds. This habit is recommended in a late number of the *London Practitioner* (and the physiological explanation given), and lauded far above the custom of surrounding the neck with mufflers of any description.

One other point is worthy of consideration, viz.: Cold is taken just as easily by the rapid passage from a very cold, or damp atmosphere, to one where the temperature is excessively dry or elevated, as by the sudden change from the latter conditions to those first mentioned. We should be careful therefore to counsel nurses—

1st. Not to keep the nursery at a too high temperature, and always to have a basin, pitcher, or bucket full of fresh, pure water, with the surface exposed to the air contained in the room.

2d. Not to allow children to approach a fire-place or register immediately upon entering or leaving the house.

3d. Not to keep extra wraps on their little charges, when at home, a moment longer than is absolutely essential.

Practical attention to the preceding rules, and also to what we have written with respect to cold bathing, care of the feet, etc., as prophylactic measures of hypertonsillar growth, will be found, we are firmly convinced, of unquestionable utility.

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#### CLINICAL OBSERVATIONS ON THE MILD FORMS OF PUERPERAL INFLAMMATION OF THE PARAMETRIC TISSUE.

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THE uterus, as is well known, is connected with the other pelvic organs by means of delicate cellular tissue, which is particularly developed at the lower, cervical, segment of the organ. Like all other tissues, this cellular tissue can undergo many changes of nutrition, and can be inflamed by different causes. For a long while this localized parametric cellular inflammation entirely escaped the notice of the profession; to Virchow we owe the first recognition and a careful pathologico-anatomical study of this affection, chiefly as it occurs in women dying of puerperal fever.

Since then other authors have studied and described the disease also from a clinical point of view. It should be mentioned that in a great many cases the affection of the cellular tissue is very sharply marked by a well-defined, more or less voluminous, tumor, which is observed as well in puerperal fever as in the non-puerperal state. A few authors, however, have occasionally diagnosed the trouble, even when no tumor could be detected, by a distinct tenderness and slight diffuse swelling around the lower portion of the womb. Thus Schroeder in 116 cases of *parametritis* observed a defined tumor in only

thirty-three instances, whereas in the other cases he found merely a tenderness on pressure (thirty-nine times) and a diffuse swelling of the lateral parametric tissue (thirty-four times).

Up to the present time but little value has been attached by most authors to the situation of the inflammatory tumor, apparently because the term "parametritis" was used in a collective sense, there being understood by it various inflammatory tumors of different size about the uterus, as well as in the tissue surrounding the inferior segment of the uterus and in the substance of the broad ligament. Spiegelberg, in a short but precise paper, has recently confirmed the propriety of distinguishing these tumors as regards their situation at the side of the inferior border of the uterus and in the vaginal vault, or in the connective tissue of the broad ligaments. In consequence of the peculiar anatomical conditions, particularly the abundance of lymphatic and venous vessels near the lower half of the uterus, the tumors which are developed there are quite different from those which are observed in the other tissues adjacent to the uterus. These inflammatory affections of the cellular tissue surrounding the lower portion of the uterus and the vaginal vault are classed by Spiegelberg under the collective term *parametritis*, in contradistinction to the inflammation of the cellular tissue between the two folds of the broad ligaments, which he calls (as also do the French authors) *phlegmone ligamenti latî*, phlegmonous inflammation of the broad ligament. The former variety properly forms the subject of my paper, and I have employed for it the more convenient name of *paracervicitis*, representing, as it does, the inflammation of the cellular tissue around the cervical portion of the uterus, a condition which ought to be distinguished from *parametritis (propria sic dicta)*, in which the tumor is of greater size and situated near the median or superior portion of the uterus. This latter form should also be distinguished from *phlegmone ligamenti latî*, when the tumor is seated at the side of the uterus in the cellular tissue of the broad ligaments. All these forms of inflammation of the parametric tissue are called by different authors by different names: parametritis (Virchow), phlegmone pelvis, *phlegmon periuterin* (Nonat), pelvic cellulitis (Simpson), periuterine cellulitis (Thomas), and so forth, which terms are all acknowledged to be very indefinite.

I apply the term *paracervicitis* to the inflammation of the cellular tissue around the cervix uteri, when the disease expresses itself either by an insignificant, or distinct tumor, or by a more or less marked diffuse induration, or only by a more or less acute sensitiveness to the touch at either side of the uterus or in the vaginal cul-de-sac.

If the signs ordinarily indicating the presence of the *paracervicitis* are moderate in character and the topical symptoms but slight, the disease may be easily overlooked at the bedside, or mistaken for *metritis*, *endo-* and *perimetritis*, *colpitis*, and so forth. The question thus naturally arises : By what signs can it be distinguished from the last-named forms of inflammation ? In order to properly discuss this point it is evidently necessary to show that the course of *paracervicitis* very much resembles that of the more severe forms of *parametritis*, and to do this it is essential first to give a short sketch of the more prominent peculiarities of the latter disease.

The commencement of *parametritis* in the puerperal state is more or less acute in character. During the first days of that period the woman is seized by a more or less violent rigor, frequently accompanied by pain in the loins and hypogastrium ; she is visibly ill : the conjunctivæ are injected, headache and anorexia with intense thirst are present, and the temperature is more or less increased, reaching up to  $104^{\circ}$  or more ; the pulse is full and frequent, beating eighty-six in the morning and 118 in the evening. The lochial secretion is sometimes mixed with blood. In the lower part of the abdomen, usually to the right or left of the large puerperal uterus, the patient experiences a more or less acute, circumscribed pain. Pressure upon this spot with the hand increases the pain often to such a degree as to cause the patient to cry out. On bimanual palpation during the first two days the finger in the vagina excites considerable pain, and meets with either diffuse hardness or a circumscribed tumor ; the latter, however, is perfectly defined only during the subsequent days.<sup>1</sup> The usual seat of the affection is in the cellular tissue over the lateral vaginal vault ; only occasionally does it appear in the anterior or in the posterior Doug-

<sup>1</sup> According to Veit the tumor cannot be detected until the fifth day of the disease, when an acute tenderness appears in one limited spot.



las' cul-de-sac. The lower border of the tumor is on a level with the infravaginal portion of the cervix, its upper margin may reach up the fundus and sometimes even to the umbilicus; as regards its breadth, the tumor may extend across the whole roof of the vagina from side to side. A sufficiently large tumor is easily detected both by internal and external examination, and its exact location, size and form are then usually ascertained without difficulty.<sup>1</sup> In consequence of its lateral situation in the parametric tissue, the tumor to a greater or lesser degree stretches the adjacent peritoneum, which expansion is accompanied by more or less pain in the tumor itself. Although, as a rule, the tumor increases gradually, it may in some instances enlarge very rapidly.

When this tumor has reached a certain size it may exert a very decided mechanical influence on the neighboring parts, most frequently pressing on the adjacent nerves, and thus producing the various *peripheric neuralgiæ*, such as neuralgia of the renal and sacral regions (caused by pressure on the ramifications of the crural nerve), of the trochanteric and gluteal regions (the external cutaneous and sciatic nerves), and so on.

As regards the uterus, we observe an unusual continuance or recurrence of the after-pains, besides actual tenderness in the uterine region, and retardation of retrograde diminution (involution) of the uterus.

If the tumor be large the bladder and rectum will be more or less affected; occasionally the exceptional position of the tumor in the anterior cul-de-sac will cause more or less violent ischuria (once in twenty-four of my cases).

The febrile condition is closely united with the size and enlargement of the tumor. The fever is generally constant in type, characterized by distinct remissions, the temperature usually falling in the morning. The remission is sometimes complete, but is followed by an exacerbation often slight in degree. For instance, the swelling and pain may disappear on the right side, and suddenly reappear in the cellular tissue to the left of the uterus. These recurrences frequently happen between the third and seventh days of the disease.

The duration of the febrile stage is very vaguely stated by

<sup>1</sup> I refer in this instance only to *extraperitoneal parametritis*, which is closely connected with *puerperal paracervicitis*.

different authors. Veit believes it to continue from nine to seventeen days, Winckel on an average twenty-seven days, and Schroeder<sup>1</sup> from two to twenty-three days. This author observed the febrile stage to last,

In 1 case.....	1 day,
" 5 " .....	2 days,
" 1 " .....	4 "
" 4 " .....	5 "
" 4 " .....	6 "
" 5 " .....	7 "
" 3 " .....	8 "
" 1 " .....	9 "
" 1 " .....	10 "
" 1 " .....	11 "
" 4 " .....	12 "
" 2 " .....	15 "
" 1 " .....	19 "
" 1 " .....	20 "
" 1 " .....	23 "

The maximum temperature may be as high as  $106.8^{\circ}$ , very frequently reaches  $104^{\circ}$ , especially at night; in the morning it falls  $0.3^{\circ}$ – $1.8^{\circ}$ . As a rule, the temperature is highest on the 1st, 2d, 3d, and most frequently on the 4th day of the disease.

Compared with the temperature the pulse is not very frequent, but always full and hard, and corresponds with the intermittent type of the febrile state; in the morning it is from 72–104, in the evening from 80–116–120.

It has seemed to me that authors have paid but little attention to the comparative frequency of the rigors, and particularly of the attacks of diaphoresis. In ten cases of *parametritis* observed by me, rigors occurred on an average in  $3\frac{2}{3}\%$ , and acute attacks of diaphoresis in  $3\frac{3}{8}\%$ . These figures are larger than those of *metritis* ( $3\frac{2}{3}$ – $2\frac{1}{8}\%$ , and  $3\frac{3}{8}$ – $2\frac{3}{8}\%$ ) under similar circumstances.

The course of the disease depends chiefly on the changes taking place within the tumor. In accordance with the rapid growth of the tumor during the first few days we observe high temperature and violent local pain. Later, when the tumor

<sup>1</sup> As the reader will see hereafter, all the cases which terminated on the 1st, 2d, 4th, or 5th day, are to be considered as cases of paracervicitis.

diminishes, the fever subsides. Finally, when the temperature becomes normal, we may take it for granted either that the tumor has disappeared or that it has passed into the condition of induration. A sudden increase of fever indicates the advent of suppuration or merely a fresh attack of inflammation.

The frequently repeated intermissions in the febrile condition appear to depend on partial resolution of the tumor, and are followed by a new increase of temperature, which coincides with a fresh exacerbation of the disease. Such intermissions, or relatively frequent relapses in the disease, probably indicate a great disposition to inflammation of the parametric tissue, as well as the excellence of its power of absorption.

Parametritis generally terminates in the absorption of the tumor (complete recovery), or its transition into the state of induration (relative recovery). Occasionally the disease terminates in suppuration, the formation of an abscess, which opens into the rectum, bladder or vagina, or forces its way through the abdominal walls. Besides, in exceptionally severe cases death may ensue from septicæmia, puerperal peritonitis, or marasmus.

As regards the causes of parametritis, one cannot but agree with those authors who admit both a general puerperal toxæmia of the whole system and a local injury, such as a mechanical irritation of the lower segment of the uterus and the adjacent cellular tissue during labor. A greater or lesser lesion of the os uteri, occurring during the puerperal state, may not always be the actual cause of the inflammation of the cellular tissue, but its presence indicates at least that a certain amount of tension and laceration of the inferior uterine and parametric tissues occurred during that labor.

The list of causes is not, of course, exhausted by the above-named conditions, for in the statistical compilations given by authors, there is generally but little attention paid to various other circumstances affecting the whole system, such as the number and character of the preceding deliveries, the age of the woman, etc. Having thus briefly traced the type and cause of parametritis, I will proceed to sketch the clinical features of the most moderate form of inflammation of the parametric cellular tissue, which has but little attracted the attention of obstetricians, and to which I have given the name of "*paracervicitis*."

These pathologically moderate forms are particularly interesting, because they appear in a great many cases without being associated with other diseases of the uterine sphere, whilst the graver varieties (parametritis, phlegmon of the broad ligament) are frequently complicated, especially in cases of severe puerperal parametritis. Another point of interest in paracervicitis is its relative frequency (12.2% of all women taken ill during the puerperal state). Of the women delivered during my term of service in the Obstetrical Clinic of Moscow, whose histories were carefully noted by myself, there were during the puerperal state,

Completely sound women.....	55
Women with more or less disease of the genital sphere.....	88
The forms of disease were the following :	
Endometritis simplex.....	37
Metritis and perimetritis simplex.....	24
Metritis and perimetritis....	4
Paracervicitis.....	9
Parametritis.....	10
Endometritis diphtheritica, metropéritonitis puer- peralis.....	3
Thrombus vaginae.....	1
	<hr/>
	88

Death occurred (including one case of chronic pneumonia) in four cases.

*Paracervicitis* is ushered in by a more or less violent rigor. In 8 of the 9 cases observed by me, there was a tumor of moderate size, and once I found the paracervical tissue exceedingly sensitive; in these cases the rigor appeared once on the first day after delivery, and 8 times during the second day. Almost simultaneously with the rigor appears the tenderness at the side of the uterus, corresponding to the seat of the disease. The pulse increases to 90 or more beats, the temperature rises, but the general condition of the patient does not undergo any material changes other than those usually noticed in febrile disorders. The after-pains are more severe than usual; the appetite is diminished, thirst is increased, the tongue is moist

and slightly coated. Almost all these patients are able to nurse their children during this illness.

If the second and third fingers are introduced into the vagina as far as the vaginal vault, they will during the first day frequently detect only a moderate degree of tenderness of one or the other lateral portion of the vault,<sup>1</sup> but on the following days they will readily feel either a diffuse induration or an ovoid more or less resisting tumor of about the size of a walnut (in all my cases but one). Such a tumor cannot, of course, be detected by an external examination. It is situated laterally to the lower segment of the uterus (the vaginal portion) in the adjacent cellular tissue. When there is only a diffuse induration in one of the lateral vaginal cul-de-sac, the uterus is less easily felt through the vault of the affected side by the finger in the vagina than on the other sound side. The tumor is sometimes united with the uterus by a band of more or less firm (pathological) cellular tissue, varying from one to two centimetres in length. It is scarcely possible to push the tumor upwards with the intravaginal finger (an important point in the differential diagnosis from puerperal oophoritis), and its connection with the paracervical tissue is therefore beyond question. In 7 cases the tumor was observed to the right of the lower portion of the uterus, once a tumor appeared on each side, and in one case merely a distinct tenderness could be detected to the right of the cervix. In the case where a tumor was observed through each lateral cul-de-sac, no connection between the tumors could be detected. These lateral paracervical exudations frequently extend slightly posteriorly or anteriorly, being rarely confined entirely to the side of the pelvis. I have never found the exudation limited to the inferior wall of Douglas' cul-de-sac.

All my cases of paracervicitis were accompanied by febrile excitement, which lasted in six cases four days; in four cases five days, and in two cases three days (including the case without a tumor). The fever is constant in type, with sharp remissions towards morning, so that on an average the difference

<sup>1</sup> This tenderness of one of the lateral vaults of the vagina may be considered to represent the first stage of the disease, because sometimes successively a diffuse tumefaction and afterwards a tumor appear at the same spot. (See the accompanying cases of paracervicitis, Nos. 51 and 76.)



between the evening and morning temperature is as high as  $1.44^{\circ}$ , when the mean temperature in the morning is  $101.3^{\circ}$ , and that in the evening  $103.2^{\circ}$ . The maximum temperature (which in one case reached  $105.6^{\circ}$ ) ordinarily falls on the second or third day of the disease. The pulse follows the remittent type of the temperature; in the morning it counts 80–90 beats, in the evening 85–108–118. In paracervicitis I could not observe the inverse type of the fever; in parametritis, however, some authors (Olshausen, Samml. klin. Vortr., 1871) have occasionally noticed it.

During the course of this disease repeated rigors and attacks of diaphoresis are of frequent occurrence. On an average, every patient suffering from it experienced  $3\frac{2}{3}$  rigors and  $2\frac{5}{8}$  attacks of subsequent diaphoresis. The frequency of these rigors and attacks of perspiration is very marked compared with their occurrence in simple puerperal endometritis, where the number of rigors is  $1\frac{9}{8}$ , of diaphoresis  $1\frac{8}{11}$ . The subsequent rigors are in most instances more feeble than the first one; considering the small size of the tumor or induration, it is difficult to determine the relation between it and the rigors. As far as I have been able to ascertain, there is no direct or constant connection between the rigors and diaphoresis and the febrile condition; still, it not unfrequently happens that a rigor is followed by an increase, and an attack of diaphoresis by a decrease of temperature.

The question of the *prognosis* of the puerperal state is greatly influenced by rigors and diaphoresis. Were we to base our *diagnosis* on these symptoms, we should sometimes foreshadow an unfavorable termination of the affection, bearing in mind that the most serious forms of puerperal disease are not seldom accompanied by repeated rigors and diaphoresis. The paracervical inflammation (the acute tenderness or the tumor in the vaginal vault) once discovered, however, we can almost positively promise a complete recovery.

On external examination, the uterus in my cases was found to be more or less sensitive to pressure, particularly at the affected spot. The after-pains were always severe, and continued for three days after labor. The lochial discharge was but very slightly fetid, in two cases it showed a considerable

admixture of blood. The involution of the uterus suffered no material derangement.<sup>1</sup>

As regards the influence of the tumor on the adjacent organs, I have to mention only, that in one case the patient experienced moderate sacral pain, in another every pressure on the tumor provoked slight lumbar pain (owing to the compression of one of the branches of the lumbar plexus), and in a third I observed a moderate œdema of the labia majora.

All the cases made a more or less perfect recovery. In five cases the tenderness and the tumor both disappeared completely on the third or fourth day; in four cases the tumor diminished greatly in size, but a moderate tenderness of the affected spot remained. Together with the diminution and disappearance of the tumor all febrile symptoms ceased, and recovery took place. As regards the liability of paracervicitis to return (an accident of not unfrequent occurrence in parametritis) I was prevented from making the necessary observations by the early departure of the women from the clinic after their recovery.

The *etiology* of the disease is two-fold, either traumatic or septic.

As already mentioned, there is in some cases of parametritis *propria sic dicta* an evident connection between the disease and the traumatism of parturition; on the side corresponding to the tumor a fissure of the cervix has been observed, and from this fissure a band of more or less dense fibrous tissue has been felt to extend to the tumor. Some authors assert that the tumor coincides with the position of the foetal occiput; thus, after the left occipital position, the tumor has frequently been found in the left paracervical tissue, and *vice versâ*. For this reason stress is laid by many authors on the causative influence exerted by the pressure of the head during labor on the adjacent uterine tissue and the surrounding parametrium. Contrary

<sup>1</sup> I have compared all the cases of paracervicitis with, as nearly as possible, similar normal puerperal cases (excepting, of course, the inflammation), and have found that, while under normal circumstances, the womb on an average diminishes in one day by 0.068 of its middle size, in cases of paracervicitis it decreases 0.065. (For the method of determining the involution of the womb after delivery, see Serdukoff, On the diminution of the uterus after delivery, *Edinburgh Med. Jour.*, May, 1875.) Winckel says that the defective involution of the uterus in parametritis depends on the lateral or forward deviation of the womb.

to this opinion, however, while these tumors occur with almost equal frequency in the latero-posterior and latero-anterior cellular tissue, the anterior occipital position is much more frequent than the posterior.

It thus becomes evident that we cannot unreservedly acknowledge the influence of traumatism as an etiological factor in many cases of extra-peritoneal parametritis, and the same deduction will also apply to paracervicitis. This is, at least, confirmed by the cases observed by myself, in which the relation of the tumor to the presentation of the head was as follows:—

No. of Case.	Time of Termination of Pregnancy. <sup>1</sup>	No. of Deliveries.	Duration of Labor.	Position of the Presenting Part.	Situation of the Tumor or Tenderness.
50	10th month	2	14 hours	1st occipito-anterior.	Right tumor.
51	8½ "	2	5 "	1st occipito-anterior, then 1st anterior.	Right swelling
61	9¼ "	1	12½ "	1st occipito-anterior.	Right and left tumors.
63	9¼ "	5	6 "	" "	Right tumor.
76	9¾ "	4	11 "	" "	" "
93	10 "	4	9 "	2d occipito-anterior.	Right tenderness.
97	8 "	4	9 "	2d anterior-back.	Right tumor.
				Spontaneous evolu.	
119	10 "	4	3½ "	2d occipito-posterior.	" "
127	10 "	3	9 "	1st occipito-anterior.	" "

Thus, without denying the traumatic origin of the disease in some cases in which the parametric tissue is particularly liable to this affection (such as those in which a band of hard cellular tissue extends between the cervix and the tumor), other factors, especially septic infection, appear to possess greater etiological importance. When we bear in mind, indeed, that these patients spent more or less time in the Clinic at a period when various puerperal affections of greater or lesser severity prevailed almost side by side with them, we may readily imagine that these women were in some degree submitted to the same pernicious influences, on the quantity and quality of which may be supposed to depend the different forms of puerperal disease.

In conclusion, I will briefly relate two cases of paracervicitis:

CASE 51.—W. D., aged 18, prematurely delivered of her

<sup>1</sup> The German method of reckoning the duration of pregnancy—ten lunar months, of twenty-eight days each—is here employed.

second child (labor being brought on by over-exertion at the sewing-machine) at 8 $\frac{3}{4}$  o'clock on the evening of Dec. 15th. The child was alive and healthy, and weighed five pounds; it was not nursed. The after-pains were moderate.

Dec. 16.—Child died. Castor-oil  $\frac{3}{4}$  ss., free evacuations.

Dec. 17.—Moderate swelling of the mammary glands. After-pains. An inspection of the inferior segment of the uterus by means of the speculum reveals the following condition: The anterior border of the anterior lip of the uterus is rose-colored; from the posterior margin of that lip extend bright-red, radiating stripes with bluish centres,  $\frac{1}{2}$ – $\frac{3}{4}$  centimetres in length. The posterior lip is covered with coagula, after removing which a rose-colored, superficial sore (erosion of the mucous membrane) appeared.

Dec. 18.—The uterus moderately sensitive, the lochial discharge sanguineous, but not offensive.

At 4 P.M.—Delirium, rigor, and diaphoresis; temperature 103.1°, pulse 100. Increased pain in the uterine region, and marked tenderness of the right vaginal roof. Increase of the after-pains. Evening temperature 103.4°, pulse 96. Ordered hot fomentations to the abdomen, and  $\frac{1}{6}$  grain morphine.

Dec. 19.—One diaphoresis; morning temperature 98.6°, pulse 82. The uterus is sensitive to pressure. Two loose evacuations; lochia bloody. In the right vaginal roof a firm, painful spot, but no distinct tumor can be detected. Through the speculum the inner border of the lips of the cervix are seen to be of a red color. One rigor; evening temperature 102.8°, pulse 100.

Dec. 20.—Morning temperature 100.9°, pulse 96. Uterus moderately tender on the right side only. The resistance in the right vaginal vault is diminished, and hardly painful to the touch. Slight rigor. Evening temperature 103.6°, pulse 88; diaphoresis.

Dec. 21.—Morning temperature 100.2°, pulse 88. The right vaginal vault has lost its resistant feel, and is as yielding as the left. Evening temperature 101.0°.

Dec. 22.—Morning temperature 99.6°, pulse 72. The patient is out of bed.<sup>1</sup>

<sup>1</sup> Perhaps in this case there was associated with the undoubted paracervicitis a light endometritis or metritis, but the chief symptoms unquestionably point to paracervicitis.

CASE 76.—A. W., aged 24, has had four children, all living, but all the labors were severe. Was delivered, reckoning from her last menstruation, at  $9\frac{3}{4}$  months, of a living male child on Jan. 20th, at  $7\frac{3}{4}$  o'clock A.M. The pelvis is of the flat type (diagonal conjugate 10.5 cm., true conjugate less than 9 cm.), not rachitic.

The child is nursed. The patient has two slight rigors, and one diaphoresis.

Jan. 21.—Free evacuation of the bowels after  $\frac{1}{2}$  ss. castor-oil. Morning temperature  $101.0^{\circ}$ , pulse 98. The body of the uterus is moderately painful, mostly at the right inferior portion. After-pains; lochia normal. Two severe rigors. In the evening a hard sensitive tumor of the size of a walnut is detected to the right of the cervix. Every pressure on the tumor causes distinct lumbar pain. Evening temperature  $104^{\circ}$ , pulse 120. Diaphoresis during the night.

Jan. 22.—Morning temperature  $104.0^{\circ}$ , pulse 96; uterus less sensitive; the tumor diminished in size, less painful, and pressure on it is not followed by pain radiating through the lumbar plexus. Violent after-pains.

Jan. 23.—One attack of diaphoresis. Morning temperature  $98.6^{\circ}$ , pulse 60. The tumor smaller, softer, scarcely sensitive. Lochial secretion somewhat fetid. Bowels moved after castor-oil.

Jan. 24.—Slight tenderness to the right of the uterus.

Jan. 25.—The womb more sensitive on pressure. The tumor again appears on the right side, and attains its former size; it is hard and painful. The departure of the patient prevented me, to my regret, from making further observations as to the cause and duration of this relapse.

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## IS MENSTRUATION A DISEASE?

A REVIEW OF PROFESSOR KING'S ARTICLE ENTITLED "A NEW BASIS FOR UTERINE PATHOLOGY."

BY W. H. STUDLEY, M.D.

(Read before the Yorkville Medical Association, Sept. 28th, 1875, and published by request of the same.)

ON June 25th, 1875, Dr. Paul F. Mundé read before the Medical Library and Journal Association an able Report on the Progress of Gynecological Science during the past year. In this Report he took occasion to say that he was induced "to refer prematurely to a startling and remarkable theory advanced by Dr. A. F. A. King of Washington, Professor in the University of Vermont, to the effect that the generally accepted physiological function of menstruation is really a pathological condition which in biblical, almost pre-historic times, did not exist, but has gradually become a fixed habit of the female sex in consequence of the vitiating influence of civilization. Dr. King bases on this view a new theory of uterine pathology, and defends his opinions by arguments, logical and historical, in an article to appear in the August number of the American Journal of Obstetrics." In accordance with this latter announcement, Dr. King's article appeared, and is now before the profession.

In the present status of the question under consideration, I am inclined to think that Dr. Mundé's epithets of "startling" and "remarkable," as applied to the views advanced, will seem just and appropriate. The theory is not altogether original, however, with Dr. King, whatever may be its merits or demerits. Tilt, in his work on Uterine and Ovarian Inflammation, when discussing the menstrual function, says: "I need not, however, here discuss the opinion of our countryman Emmet, who supposes that menstruation does not occur naturally, but is the result of social habits, which do not permit women to enjoy sexual intercourse when they feel the need of it." But

doubtless no one heretofore has presented the subject with such a seeming array of philosophical and historical arguments, as Dr. King, and to him, more than any one else, must be attributed the parentage of the new fledgling. Will it fly? The future will determine. In the meantime it is permitted to members of the profession to canvass its claims. In the enjoyment of this privilege I embrace the present occasion to place before you my individual views.

After drawing some consolation from Dr. Thomas's remarks to the effect that uterine pathology is at present in an unsettled, a melancholy state of uncertainty, and inferring from them a palliation for presenting his strange views, he begins his work, like a true *deductive* philosopher, by laying down eight *propositions*, which he thinks to be almost axiomatic. The propositions, in the main, and considered by themselves as abstractions, we have no particular disposition to find fault with; there are one or two points however, on which, if we understand him rightly, we must join issue. In his first, second, and third propositions, which are virtually and essentially *one*, he makes use of such terms as these—(referring to health)—“absolutely typical perfection,”—“typical standard,”—“absolutely natural environing conditions,” and teaches that the pro-rata departure from these criteria, “would first entail modification of function—to be followed in time by modification of structure;”—and he goes on to say, in his fourth proposition, that “modifications of structure thus set up, while they are real abnormities, ought to be recognized as processes of *pathological evolution*”—in short, *diseases*. Now what we deny here is, first, that there are any “typical processes” of health applicable to all individuals—to all peoples in all localities and climes. “The broadest and most complete definition of life,” says Herbert Spencer, “is the continuous adjustment of internal relations to external relations.” Assuming this to be the true definition, where is chance for *fixity*—where can be found an absolute type? Outer relations, or, in other words, environing conditions, have as much a part to play in fixing the standard of health as inner relations. “All vital actions have for their final purpose the balancing of certain outer processes by certain inner processes.” Now these are not the same, and can never be the same, for all

in a world presenting such unceasing changes as this, and thereby entailing such different environing conditions. Individuals and peoples will be conditioned and modified in thousands of ways, and the functions and structure of their systems will be commensurately changed, and not until material discomfort be experienced and vitality be interfered with ought those modifications to put on the name of pathological processes. The factors of organisms are inner relations on the one hand, and outer relations on the other. Their healthy continuance consists of a proper adjustment of these relations; and in this adjustment the factor of outer relations has as much to do in moulding the standard of health, the peculiarity of structure and function, as the factor of inner relations. Shall a fine-wooled sheep from the hills of New Hampshire or Vermont, because transported to the plains of Texas, and there, in the course of years, because of its peculiar environment, growing a coarser wool, be said to be in a pathological condition? Are its hair follicles diseased? Shall a tropical sun, which in time will produce the olive complexion, be said to have changed the pigmentary function into a pathological process, and thereby to have diseased the structure thus affected? Shall the Arctic explorer and the inhabitants of those latitudes, because, in consequence of their environment, they are able to live on seal and drink train-oil by the quart, be considered as having functional derangement of the stomach, which in time must get up structural change of the same and hence disease?—and all this because we cannot do the same in our latitudes? Which is the “typical standard” of perfect health in any or all of these instances? We have none. In dwelling upon these points of (1st) a rigid standard of health, and (2d) a departure in any degree from that standard, as constituting disease, let it not for one moment be supposed that I do it with a view of antagonizing Dr. King’s position in his applications of his propositions to the great point in question—viz., *menstruation*—on the ground that it is in any sense or degree a modification of or a departure from a natural function of the uterus. I unhesitatingly take the ground that it is not a departure; that it is purely and simply a natural function of that organ.

My object has been, 1st, to show that his supposed axiomatic propositions are untenable upon the points above discussed,

and, 2d, even for argument's sake admitting that menstruation may be a slight modification of function (so slight that, according to Dr. King's admission, it ranks next best to reproduction), to show that such a modification is unworthy of the imputation of a "pathological process." Passing over the remaining four propositions as containing general truths, but having no special bearing on the question at issue, excepting as applied by Dr. King in accordance with his peculiar views and mode of reasoning, let us pass on to the *proofs* of his assertion, that menstruation is a disease, as gathered from his applications, his assumptions, his so-called historical facts, and his general running arguments.

He settles to his task by first defining what he considers the natural environment of the womb in a perfect state of health, in a perfectly healthy individual at the age of puberty. He asks the question, "What external conditions would now constitute its natural environment?" And his response is: "There is but one answer, viz., those conditions that elicit from it the special function which its anatomical construction and functional power adapt it to perform." He again asks: "What is this function?" and his answer is: "There can be but one reply, viz., it is *reproduction*."

Let us examine these statements. In the first answer he speaks of a "*special function*" of the womb, as if it performed only *one*, or at least one of any sort of consequence, and in the second he terms that function "*reproduction*." Now, with regard to the first position, he commits the unpardonable error of a disputant who assumes unproved premises which he knows to be in question, and with regard to the second, he teaches what appears to me to be essentially false. From time immemorial, and universally, *menstruation*—the very question in dispute, with such tremendous odds against him—is utterly ignored as a function of the uterus, whereas it is undoubtedly *the* special and essential function of that organ. Who will say that its "anatomical construction and functional power" do not adapt it to perform this office, as well as the *part* it plays in the complex process of reproduction? And if the conditions which elicit the act of menstruation are far more universal—always have been, and always will be, whether among savages or the civilized—far more abundant, and elicit it infinitely

oftener than reproductive environments ever possibly can produce their function in the part which the womb plays in it, shall we not call it a function of the womb, and, as compared with its other chief act, full as important a one, if not more so? With regard to the position that reproduction is the essential function of the uterus, I hold it to be false. If any one organ in the generative group has a right to claim superiority over another in that mixed process, it undoubtedly is the ovary. Dr. Tilt says: "In woman it has been amply shown by the successful experiments of modern observers that the ovaria are the *essential organs of reproduction*." And why should they not receive this verdict? It is at the ovaria that fecundation takes place. It is while the ovule is yet in the Graafian vesicle that this, the female germ, is brought in contact with the spermatozoa, and impregnation is accomplished. At least by far the greater balance of proof is in favor of this doctrine.

Bischoff, Wagner, Barry, and others made the discovery of the actual presence of the spermatozoa upon the ovaria.

Cazeaux says: "In the present records of our science, there is no one conclusive fact, that proves the ovule to have ever been seen in the womb of a woman prior to the tenth or twelfth day after conception."

The burthen of proof is for ovarian fecundation. It is there that the being is reproduced. Reproduction is, therefore, the essential, the peculiar function of the ovaria; and this, in the main, would be true, even if we admit that impregnation might take place elsewhere in the generative tract, inasmuch as they furnish the germ out of which (influenced by the male germ) the reproduced being is elaborated. The relation which the impregnated ovule holds to the womb is simply this: The womb is the nest in which the egg is hatched. It is the lodgment, the quarters, in which nature yields ample provisions for taking up the process of nourishing and developing the reproduced being, when the limited supply of the ovule's own support is exhausted; and finally, at term, when it has finished this peculiar work, it hands over the now considerably developed reproduction of the ovary to other fostering arms than its own, viz., the mother's—to other sources of nutrition than its own, viz., the mother's breast. But considering reproduction as a whole in a more comprehensive sense than that



of limiting it exclusively to one organ, no reasonable person will object to yielding a part, and a most important part too, in the process, to the womb. That part is as I have defined it to be. I shall not deny that reproduction is a special function of that organ, but I can hold it as only *one* of its special functions; and the only reply which I deem necessary at this point of the controversy is this—if that can be termed *the function* of an organ of a human being which, in consequence of its constant and inevitable environments, it is utterly impossible can be continuously performed, and for the plainest reasons should not be continuously performed (except with an extremely limited few), while an acknowledged *substitute* for that function, seen to be all but universal, traced to the earliest records, continuous with a remarkable periodicity through an average term of thirty years, must be denied the title of function and classed with pathological processes as a disease—then, all I have to say is, that I have yet to learn the true rationale of functions, and when I have learned it, yet to be in a quandary as to their relative ranks in importance.

But to return: The third question is this, “What special condition in the natural environment is it that seems to elicit this function of the uterus.” “Again,” he says, “there is only a solitary answer available, viz., the condition that secures the performance of the preliminary process of fecundation—in point of fact, *un male*.”

Well, no one disputes that sexual connection is a necessary preliminary step to impregnation. But, if my views be correct, fecundation or impregnation is essentially and *par excellence* reproduction, and we have seen that that process does not take place in the uterus. In view of the office which the uterus *does* fulfil, sexual connection is no more a “special condition in the natural environment” to elicit its function, than ovulation and fecundation itself. They are all absolutely necessary, and as such *special conditions*. But to follow our author: he says, “Let this condition of the natural environment be changed” (*i.e.*, prevent sexual communion), “and there must necessarily occur a corresponding modification of the natural function of the uterus—and, in time, of its natural structure; the modification of function and structure seeming to adapt the uterus to the enviroing modification, but at the same time

necessarily unfitting it in some degree for the perfect execution of the function that belonged only to its originally natural environment." "The modification of function here referred to," he says, "is menstruation. By considering the *function* (?) of menstruation as an abnormal process, we, I think, hold the end of the thread in the tangled skein, by following which the mystery of uterine pathology may be unravelled, and its knots of confusion orderly arranged." Here you have what he terms the "central idea" of his article. In support of this teaching he gives us twelve distinct arguments, to the examination of which I will now proceed.

"1st. Menstruation is the result of an interference with nature, of a thwarting of her designs, of a violation of her laws, and is preventable by obedience to her laws." Well, if this be a proof that menstruation is a pathological process, I have learned a new rule in logic—which is no more nor less than simply this, that a declaration that a thing is so (being the very question in dispute) is evidence that it is so.

"2d. In the great majority of cases it (meaning menstruation) is not latent as are other purely physiological processes, but is accompanied by unpleasant symptoms."

In reply, I say that if latency and a perfect freedom from unpleasant symptoms, are necessary qualities of a physiological process, then menstruation alone must not be passed into the realm of pathology. Surely, Dr. King will not be bold enough to wave his necromancing wand over the process of *parturition*, and blast that too with the curse of disease. Is parturition either in the brute or human being latent? Is it free from unpleasant symptoms? Are the wants—physiological wants of nature as manifested in the shapes of hunger, thirst, and sleepiness—altogether latent and entirely free from unpleasant symptoms? No—as there are legitimate discords in the highest order of music, so there are physiological discomforts and even pains in the intricate and complex workings of the animal economy.

"3d. To preserve comfort and cleanliness it (menstruation) requires during its continuance the application of an artificial appendage to the person. This requirement belongs to no *natural* emunctory." Are defecation and urination pathological processes? Is a baby's anus a natural emunctory? Is a baby's

urethra a natural emunctory? They certainly require an artificial appendage, at least among all decent and civilized beings. But inasmuch as savages approximate more nearly the "typical normality" of the human race, according to Dr. King, it is possible that these "artificial appendages" are dispensed with among them. I do not, however, believe that their use would affect the "typical standard"—otherwise than to improve it. Upon the principle which such an argument would involve, we should use no handkerchiefs for the perspiring face, because forsooth perspiration is emitted from those natural emunctories called sweat glands. We should use no napkins for the lochial discharge, for no one will deny that the uterine and vaginal canal is a natural emunctory, or that the lochial discharge is the invariable and *inevitable sequence of a physiological process*.

"4th. Menstruation is a hæmorrhage; it is attended with the rupture of blood-vessels. Blood-vessels were not made to be ruptured. No hæmorrhage is natural." Well, how shall we regard the hæmorrhage that invariably attends the parturient act? Surely plenty of blood-vessels, and more than blood-vessels, are torn in the physiological act of the womb as it casts off the now useless placenta. And what shall we say of that often and constantly recurring hæmorrhage attending the bursting of the Graafian vesicle? I do not mean the menstrual hæmorrhage of the womb, for that is the question in dispute—I mean the hæmorrhage at the ovary produced by the *physiological* laceration of the vesicular tissue and its peritoneal covering, and evidenced by the blood clot invariably found in the pelvis of the now torn vesicle? Are these physiological or pathological processes?

"5th. Although menstruation is desirable and necessary in celibate females to relieve congestion of the uterus, it still ranks second best to reproduction, which prevents abnormal congestion, and it ought no more to be considered physiological on account of its salutary effect than epistaxis, which relieves congestion of the brain, or bleeding from hæmorrhoids, which lessens portal congestion, nor, indeed, than 'vicarious menstruation' from the nose, skin, breast, stomach, lungs, etc., which are equally beneficial in depleting an over-full vascular system." The fallacy of this argument lies in the fact that there is a tremendous difference between menstruation and the several cited conditions in respect to a certain feature which of itself must

stamp a process as natural, and therefore physiological. This feature is *universality*, remarkable *periodicity*, and *continuity* of habit in the animal economy: such a feature is law. Menstruation is pre-eminently characterized by it; it is, therefore, physiological; and when Dr. King can show that epistaxis and hæmorrhoids and vicarious menstruation possess the characteristics of universality, periodicity, and a continuity through an average period of thirty years, then he will prove that epistaxis and hæmorrhoids and vicarious menstruation are physiological too.

"6th. The menstrual periods in women are analogous with the periods of œstruation ('heat' or 'rut') and ovulation in other animals. In both women and animals these epochs are the periods naturally designed for coition and successful impregnation, as evidenced, first, by the coincident discharge of ova, and, second, by the well-known greater certainty of conception taking place, when coitus occurs during that epoch. The menstrual discharge of blood has no analogue in other animals." I am willing to admit that, in general, women are pervaded with the sexual desire during the menstrual epoch; but with this addition, that they possess it full as strongly immediately before and after that period. So far as *desire* for the act is concerned, then, there is no necessity for coition being limited to the period of the flow. Reasoning theoretically, one would naturally come to the conclusion, taking a *mechanical* view of the subject, that conception would be less likely to take place during the flow than at any other time; and until we are furnished with more data than we already possess on which to base Dr. King's unqualified assertion, we shall remain contented with our theory. That conception is possible during the flow, we believe; that it is common, we do not believe. If it be a fact so well known, it is strange that writers on sterility have not ranked it among their remedies for that condition. But there is a fact well known, and that is this, that impregnation is more likely to take place immediately after the menstrual act has ceased than at any other period. The fact is testified to by both the laity and the profession, and the period is especially embraced (in accordance with professional counsel and experience) by those who are desirous of increase, and especially avoided as immi-

nent by those who wish to prevent pregnancy. My opinion in regard to the rationale of this fact is this : Impregnation is more likely to be secured at this time, because of the recent deluging with menstrual blood, by which the secretions, especially of the cervical canal, have been washed away, which secretions often prevent impregnation either by their chemical incompatibility with the vitalizing fluid or by the mechanical obstruction in the form of the firm mucous plug so often found in the canal. I am sure that many will corroborate my statement to the effect that pregnancy has often been brought about by the simple operation, on the part of the gynecologist, of removing that plug or washing away inflammatory secretions with a simple astringent wash, and advising copulation immediately after. But proof there is in abundance that impregnation is dependent on no particular period. "I know of cases," says Dr. Oldham, "which I have carefully inquired into, where impregnation occurred at the respective times of ten, twelve, and twenty-one days after the menstrual period ; and while, on the one hand, I am quite ready to admit a *greater* disposition to impregnation shortly after a menstrual period, yet I know of no facts to disprove the opinion that the human female is susceptible of impregnation at any time between her monthly periods." Now, because of no simultaneousness between menstruation and these periods of impregnation, it follows that there need be no necessary connection between menstruation and ovulation. What are the evidences bearing on this last position? Tilt says : "In three cases in which Dr. Ashwell had opportunities of examining the ovaria of women who died during the flow of the catamenia, there were no signs of the rupture of the Graafian vesicle and the escape of ovules. In one of these cases the woman had menstruated regularly for several years, and yet the ovaria were perfectly smooth ; there was neither rent nor cicatrix marking the site of either a present or former maturation, and escape of a Graafian vesicle." Again : "Dr. Ritchie, in a valuable series of papers, adduces five examples of menstruation neither caused nor accompanied by ovulation. In the first case the woman had menstruated ten days before death, yet the ovaries presented no external puncture or cicatrix, although both ovaries were filled with vesicles. In the second case menstruation had taken place thirteen days before death, but



the ovaries showed no puncture nor cicatrix, although containing numerous vesicles, one as large as a garden pea. In another case, menstruation had occurred a week before death, but there was neither opening nor scar on the surface of the ovaries. In another case still, menstruation had occurred a fortnight before death, but the ovaries showed no sign of recent ovulation. On the other hand, the ten dissections contained in the first section of Dr. Ritchie's papers, show that there may be both ova and ovulation without a vestige of menstruation." Again: "Hirsch (Schmidt's Jahrbuch, 1853, No. 2) has likewise seen a case wherein impregnation took place at the 22d day after healthy menstruation, and he observes correctly that, as the Jewish women are obliged to abstain from intercourse five days before and seven days after menstruation, that race could not be so prolific, as it is known to be, if the ovular theory of menstruation is true." Again: "The more I observe, the more I am struck by facts which cannot harmonize with the ovulation theory. I have patients in whom any unusual nervous emotion or over-exertion will bring on the menstrual flow with the usual menstrual symptoms, although they may have just recovered from this discharge. How can it be supposed that an ovule be ripened, and the dense ovarian envelope suddenly perforated by the fatigue of a dinner-party; by hearing disagreeable news, or by an altercation with a servant. The laws of ovulation are as yet imperfectly known, but I believe that it proceeds as regularly, fatally and uninterruptedly as nutrition; whereas the menstrual function shifts its periodicities, returning about the fourteenth or twenty-first day after the last epoch, whether it came at a right or a wrong time." It would thus appear in numerous instances, at least, that ovulation and menstruation have no special interdependence, and that they are not to be regarded as convertible terms. I quote from Dr. Mundé's Report on Diseases of Women for the year 1874, published in the August number of the AMERICAN JOURNAL OF OBSTETRICS. He says: "Slavjansky, whose name is well known by his researches on the *physiology* and *pathology of the ovaria*, sums up the following results obtained by him." (I cite only his third and fourth conclusions as bearing on the question under consideration. They are as follows.) "The development and ripening of the

Graafian follicles does not take place periodically, in a regular manner, *and there is no connection between it and menstruation.* Menstruation is a physiological phenomenon *unconnected* with the development and ripening of the Graafian follicle." In the light of the evidences which have been produced, the question may be fitly asked if Dr. King is warranted in taking the unqualified position that the discharge of ovules is coincident with menstruation, and basing upon that as *one premise* the doctrine that the menstrual period is naturally designed for coitu and successful impregnation. As regards the statement that menstruation has no analogue in other animals, we would suggest a closer study of sluts, monkeys, man-like apes, etc.

The seventh argument presented by Dr. King, that menstruation is a pathological process, is this : He says—"Evidence is wanting to prove that menstruation is common in women belonging to the savage races of mankind, who live more strictly in accordance with nature, untrammelled in their reproductive function by the usages of civilization. The Hindoo women as a rule do not menstruate ; with them menstruation is considered a crime." Quoting Tilt again : "The early appearance of first menstruation," says Mr. Walker on Intermarriage, "is remarkable in the Mongolic or North-Eastern broad-faced variety." "A French writer asserts that a Kalnuck or a Siberian woman of the Mongolian race is marriageable (because established) at the age of 13 ; that still farther North, and even on the confines of the Icy Sea, the Samoiedes are nubile at 11, and are frequently mothers at 12 ; that the women of Lapland begin to evince maturity (*i.e.*, by menstruation) at 12, and that the same appears to be the case with all of the races of the polar regions—as the Ostiacs, the Yakoutes, the Kamscatdales, and even the American Esquimaux." Now it may be objected that, strictly speaking, the term *savage* cannot be applied to the Mongolian race as a whole ; yet I venture to say that, as far as the habits of many of its tribes are concerned, the barbarous and beastly custom of childhood marriage or intercourse can equal that of the most savage tribes typically selected. And it will be remembered that this early marriage or intercourse, in order to prevent the pathological process of menstruation, is what Dr. King argues for.

Now for the Hindoo women. Dr. King says: "The Hindoo women, as a rule, do not menstruate; with them menstruation is considered a crime." Dr. Tilt says: "In India dishonor is attached to the parents of a girl who is not married extremely young." "The precocious use of matrimonial stimulus is, no doubt, calculated to advance the period of first menstruation; and it is to the influence of this perverse custom, strengthened by hereditary transmission of what was habitual in the parent, that Mr. Robertson ascribes the *incontestably early menstruation* of Hindoo women."

Will any one pretend to deny that menstruation is common with the North American Indians? I have travelled among five tribes of them—the Quapaws, Cherokees, Creeks, Choctaws, and Chickasaws. I was called upon in several instances to treat them professionally. With regard to the generative organs and their functions, I made, when required, the same investigations as I would with the *civilized*, and no evidence ever broke upon me that the Indian woman was different, so far as the menstrual act was concerned, from the civilized white woman. Further and weightier evidence on this point might be adduced but for want of time.

Let us examine the eighth argument. In this Dr. King says: "History does not furnish *unequivocal* evidence that menstruation was common in ancient times. True," he says, "we are told (Genesis xxxi. 35) that when Laban was searching for the images Rachel had stolen, and which she had concealed by sitting upon them, she said to him: 'Let it not displease my lord that I cannot rise up before thee, for the *custom* of women is upon me.' " "The term *custom* in the text referred to," he says, "would seem to be more or less conclusive (admitting no errors of translation) were it not for other references, in which we read (Leviticus xv. 19): 'If a woman have an issue, and the issue of her flesh be blood, she shall be put apart seven days, and whosoever touches her shall be unclean,' etc.; and again (v. 29, 30): 'She shall take two turtles and two pigeons, which the priest shall offer as an offering to *make an atonement for her* for the issue of her uncleanness.' Thus," he says, "the issue seems to have been regarded as sinful. If it had been common, or 'customary,' how numerous must have been the priests! how constant their duties! and what count-

less thousands of turtles and pigeons must have been required for the sacrificial offerings." And so Dr. King, without a deeper and more comprehensive investigation, jumps to the conclusion that the word "*custom*" made use of, either means *nothing at all*, or else it must mean an event of exceeding great rarity—so rare, indeed, that the priests might easily attend to their duties. Who ever heard or knew that the word "*custom*" was or could be applicable to exceedingly rare and desultory acts? But we are not left to guess-work in this matter. If any one will read from the 19th to the 25th verses of the same chapter of Leviticus he will see that a subject has been begun and ended, and refers to a particular class of cases. That subject was the common monthly of women. "For being in that condition she was to be put apart seven days, and everything that touched her was to be considered unclean from one to seven days." This was the only penalty for contact with an ordinary menstruating woman, and for a woman being in that condition. Now we come to the 25th verse of this same chapter, and what do we find? Why, that it begins an entirely new and different subject, and it is in connection with this and this alone that the penalty of offering up two turtles or two young pigeons is spoken of. Let us read, and see if we do not find this to be so:

"And if a woman have an issue of her blood *many days out of the time of her separation, or if it run beyond the time of her separation*; all the days of the issue of her uncleanness shall be as the days of her separation; she shall be unclean." "But if she be cleansed of her issue, then she shall *number seven days*, and after that she shall be clean." "And on the eighth day she shall take unto her two turtles, or two young pigeons, and bring them unto the priest," etc. Here is a condition of things, it is plain to see, which borders upon and was considered *disease*, and the penalty was precisely that for diseased men spoken of in the first part of the same chapter.

Bishop Patrick, and all other commentators of any note, take precisely this view. Bishop Patrick's words, as commenting on the 25th verse, are these: "As before,"—referring to the immediately preceding verses,—"As before he spoke of the *natural course* of the blood, so here of a *disease*." With the foregoing interpretations (and I take the ground that they are incontrovertible), Dr. King's plea falls to the ground, and

the word "*custom*" stands out in bold relief as having no hidden or distorted meaning, and, according to his own implied admission, is "conclusive" evidence that the word meant a regular monthly habit with women. But what will he think of the corroborative proofs of this fact in other parts of Scripture, which I will now call attention to? The prophets Isaiah, Jeremiah, and Ezekiel are supposed to have lived in somewhat ancient times. In the 22d verse of the 30th chapter of Isaiah appears the following: "Thou shalt cast them away as a *menstruous cloth*." In the 17th verse of the 1st chapter of Lamentations appear these words: "Jerusalem is as a '*menstruous*' woman among them." In the 18th chapter of Ezekiel, 6th verse, it reads thus: "Neither hath come near to a *menstruous* woman." Now what is the derivation of the word "*menstruous*?" One radical is the Greek word *μην*, signifying *month*, and undoubtedly the other is the Greek word *ῥεω*, meaning *flow*. First Latinized and then Anglicized, we have the word "*menstruous*," implying nothing else but *monthly flow*. Now coupling the proofs here furnished with those given to us by a correct interpretation of the 15th chapter of Leviticus on *issues*, and the true and only possible meaning of the word "*custom*," which Rachel used, and I think that we can safely and consistently come to the conclusion that Dr. King's statement is not true when he says, "History does not furnish unequivocal evidence that menstruation was common in ancient times."

Dr. King's ninth argument is this: "Women have been known to bear large families and enjoy good health without even menstruating at all. Can it be said that such women are sick? Must it not rather be admitted that they are enjoying a higher grade of health; that their reproductive systems are following more strictly a natural course than belongs to sterile and menstruating females?" The answer to this is as simple, plain, and conclusive, it seems to me, as the argument is specious. Instead of a few exceedingly rare exceptions in which "women have been known to bear large families and enjoy good health without even menstruating at all, who cannot point to thousands of women—nay, I may say, the majority of women—who have been known to bear large families and enjoy good health, who have menstruated regularly and continuously through their reproductive life, with the exception of the periods of gestation



and lactation? And in the face of such an incontrovertible fact, how can Dr. King have the presumption to ask the question in defence of an insignificant few non-menstruating women, "Can it be said that such women are sick?" I reply: Can it be said that my overwhelming majority of menstruating women are sick either? Does Dr. King furnish any statistics that his non-menstruating women are in better health than menstruating women? No. He has not one fact to base such an opinion upon, but, like a good plausible sophist, he tries to make it appear so by asking the question, "Must it not rather be admitted that they (the non-menstruating women) are enjoying a higher grade of health? that their reproductive systems are following more strictly a natural course than belongs to sterile and menstruating females?" I flatly answer: No, Dr. King; no such conclusion can be admitted, for you furnish no proof for it. If my menstruating women, whose name is legion, are in just as good health as your non-menstruating women, who are in such insignificant minority, it is illogical and exhibits the bent of an enthusiast to arrive at such a conclusion. But why not be more explicit on the cause of this non-menstruation? Why not be more particular on the number of children constituting those large families? Would Dr. King have us infer that these non-menstruating women were so because of being kept continuously in the family way? We certainly have a right to infer this, for he holds that menstruation is a substitute for reproduction, and ranks next best to it. In that case what a raft of children they must have been blessed with. I am inclined to think that we should find among these non-menstruating women some lacerations of the cervix, some ruptures of the perineum, some relaxation of the vagina, very likely a little or much uterine prolapse, some vagino-vesical and rectal fistulas, some cystoceles and rectoceles, many subinvolutions and their consequent hyperplasias, ulceration, etc.—all of which find their legitimate place in uterine pathology, but which, nevertheless, never ought to have found a local habitation in these non-menstruating prolific females.

*The tenth argument:* "Since procreation is natural to women only during a part of their life, the *child-bearing period must have a beginning*. If puberty, when the organs are fully developed and prepared to fulfil the procreative office,

is not the natural period for reproduction to begin, when *else* is the beginning of the child-bearing period? To postpone the reproductive act beyond its natural time is abnormal; menstruation would not occur without such postponement." The fallacy of this argument consists in the confounding of the physiological *capacity* of an organ with the necessity of its physiological *action*. Because the uterus is capable of reproduction, therefore in that first moment of its capacity it must, it ought, in order to preserve its normality, begin the work of reproduction, means Dr. King. Let us apply this principle to other organs, and witness its workings. The brain of a child, variously between the ages of five and eight years, is capacitated to perform its function of mentality to a far greater extent than it is customary to demand of it. For the best of sanitary reasons the active and systematic cultivation of its function is, as a rule, deferred until the system as a whole, the cumulative organization, is in a fit condition to bear the stress of this process. Otherwise not only this special organ, but the general system, would receive an irretrievable injury. The baby cuts its teeth long before they are put to work at their natural function of masticating and tearing hard substances of diet, Between the ages of fourteen and eighteen, the male generative organs are both in an anatomical and physiological condition to begin the mixed function of reproduction. No one can deny their physiological activity at this period, their capacity, and the promptings which these produce. Shall males be mated in this their mere boyhood? Shall they, with all their inexperience and lack of physical development generally, take upon themselves the responsibilities of the paterfamilias? Would it conduce to the preservation of the normality of either the generative organs or the system as a whole? Does the deferring of the marital rite to a somewhat later period tend to the deterioration of the male, either specially or generally? To be consistent, Dr. King must answer these questions in the affirmative.

*The eleventh argument.* Without quoting in full, it in effect says that organs affected with structural disease or derangement of function are peculiarly liable to congestion and inflammation from exposure to cold. No one will dispute that as a rule. From this as his premise, he goes on to say that "reproduc-

tion does not prevent the female from tolerating ordinary exposure, but she who is the subject of menstruation cannot enjoy the same liberty. The menstrual act, therefore, exhibits here again the qualities of a pathological process."

Here we have a plain syllogism. We have the major and minor premises and the conclusion. We deny both of these premises. With regard to the first, viz., that "reproduction does not prevent the female from tolerating ordinary exposure," it will not be denied that during the time of the *foetus in utero* there is a tolerable exemption from inflammations and congestions of the uterus. But the *carrying* period is not the whole of reproduction. According to Dr. King himself: "One of the offices of the reproductive function, and a perfectly natural one, is involution after delivery." And when we compare the whole of the functions of reproduction with menstruation, a singular parallelism appears, so far as the effect upon the uterine tissues are concerned at least. This is particularly true when the closing acts of reproduction are considered. In both instances something is expelled from the womb. In both instances blood is freely expelled. In both instances tissue change is in active progress—the mucous membrane in a state of partial fatty degeneration, according to Engelmann, in the one case, and fatty degeneration of not only that membrane, but of the muscular and connective tissue in the other—the beginning process of involution. In both instances there is more or less pain and tenderness of the organ. In both instances there are periods of rest and periods of activity. The intervals of menstruation are the periods when the forces are gathering only to explode at the menstrual epoch. The periods of gestation are the periods when the forces and conditions are maturing and culminating only to usher in the final act of reproduction—when by the process of parturition a human being is reproduced to the world. Under such circumstances and conditions, ask yourself the question, Which of the two will tolerate the greater exposure—menstruation or reproduction, with their respectively attendant uterine conditions? The verdict will be unanimous, that the condition of the womb and the patient, generally accompanying the culminating act of reproduction, is in a far more critical state than that attending and consequent upon the menstrual act. If because of the qualifying

features of the one act it must be pronounced pathological, then the other must be ranked in the same category, and if degree of danger from exposure to cold and in other respects be a measure of the pathological condition, then undoubtedly reproduction in its final consummation must be regarded as savoring far more of pathology than menstruation.

*The twelfth and last argument,* Why menstruation is a pathological process and a substitute for the physiological one of reproduction, is drawn from what Dr. King terms "the physiological office of personal beauty in the female." He thinks it "a most important factor in the promotion of creation;" thinks that "without it, in all probability, the race would become extinct." He determines the period of her life when "woman is most beautiful and most attractive to the opposite sex" to be the age of puberty. He thinks that the coincidence of female beauty and puberty "tells us plainly enough that the performance of the procreative office is now the most natural and only strictly natural course for the female economy to pursue." I am not disposed to quarrel on the question of influence of personal beauty in the female on the opposite sex; but when I am called upon to go so far as to believe that the multiplication of the human race is dependent upon it; that it is the only factor of importance in the excitement of the sexual passions, and that the period when this physiological flower displays its most dazzling and enchanting hues, and distils its most fragrant and intoxicating odors, is the ante-puberle hour just previous to the would-be menstrual gush, should the "procreative office" not be performed, I then most respectfully decline to follow, and my demurrer is built upon the following reasons: It is simply not the fact, as all experience proves, that sexual passion and sexual gratification depend pre-eminently upon the personal beauty of the female. As well might you talk of the "*physiological office*" of the choice viands of the table, and say, that upon it depends the condition of hunger, and through the latter the prevention of the starvation of the human race. The sexual passions (and as a necessary sequence the sexual act) are chiefly aroused in both the male and female in consequence of a physiological process of their respective organs of generation. In the female this process is the ripening of an ovule, or the menstrual act, with its post and ante

periods. And while under the maddening excitement, if opportunity and importunity present themselves, the most disgusting object is but too often permitted the sexual embrace. In the male the testes, like the kidneys and other glandular organs as to function, are more or less constantly performing their office of secreting the seminal fluid. The repletion of the vesiculæ seminales with this fluid prompts the function of the sexual act, and its gratification is sought in cases of emergency with anything in the shape of a human female, or, if resisted, but too frequently is it accomplished with a phantom in the realms of Morpheus. With reference to the period of woman's life when she is most beautiful and most attractive, I appeal to the world, and feel confident that they will disagree with Dr. King, when they understand, as they must do from his peculiar position upon this question, that he means by the word *puberty* the period immediately preceding the beginning of the menstrual life. In view of his whole line of argument, it is impossible to construe it into any other interpretation; but this is not what the medical world mean by the word. Universally, the menstrual act is regarded as the mark and index of the beginning of the puberic age, and it is not until after the female is fully launched upon it, thoroughly established through an indefinite period of months, that she can be regarded as having arrived at it. During this period, which may be set down as varying from six to eighteen months, the girl (for she can be called nothing else) undergoes many of those charming developments which Dr. King would limit to the ante-menstrual period. And "the remarkable changes, not only as to personal appearance, but also beauty of character and emotional susceptibility," which are simply *begun* during this transitional process (in which the subject of it can be rightly regarded as neither child nor woman), continue to unfold with increasing charms and more personal attractions even for years after the puberic age. Immediately before the beginning of menstrual life, the female, as a rule, is an awkward, ungraceful, undeveloped, ungrown, and, as such, an unattractive being—at least sexually so considered. The menstrual act is an era of beginning *changes* in all of these respects, but it requires months and even years to perfect these changes, and it is then, and only then, that woman can be said to be



“most beautiful and most attractive to the opposite sex.” But I am reminded by a paragraph in Dr. King's article that I may be doing him an injustice by attempting to decide not only the questions of “beauty” and “puberty,” but the main question of his whole article, viz., *ante-menstrual marriage* on the basis of civilization. For he says, immediately after finishing his twelfth and last argument: “I do not assert that *woman as she is* ought not to suffer the menstrual hæmorrhage; nor do I affirm that *every* female on attaining the age of puberty ought to marry and perform the reproductive office. What I do affirm, and without hesitation, is that, assuming the female to have attained an approximate or perfect development, she has inherited no tendency to disease, and been subjected to no such abnormal agencies as would affect injuriously the reproductive organs, and excluding, of course, as every physiologist must, the social, educational, and financial considerations which are allowed to influence the time of matrimonial alliance, under these circumstances that she would be a fit candidate for ante-menstrual marriage.” Here, as elsewhere in Dr. King's article, civilization (as an element of deterioration) is almost altogether excluded. For the present the civilized woman, as a rule, is unfit for ante-menstrual marriage. The questions naturally growing out of this position are, 1st, whether there be warrants for assuming that the uncivilized, the barbarian female, is in a naturally fit condition for taking upon herself that office, while the civilized one is not; and 2d, what are the therapeutical measures to be adopted, as built upon Dr. King's “new basis for uterine pathology?” In regard to the first question of barbarism *versus* civilization, we expect evidence from Dr. King, and not mere assertions. Has he produced them? No. He has, it is true, made statements about women not menstruating in ancient times—the Hindoo women, and women belonging to the savage races of mankind; but we have in our former remarks dwelt sufficiently, it seems to me, upon these *mere assertions*, to show that they should not be taken as proofs, but, on the contrary, to show that they have been disproven by positive evidence against them. Not one particle of proof is given that the immediate ante-menstrual period of the savage, the ancient, or even the backwoods female of modern times, is later, as a rule, than that of the civilized one, and

so finds her more developed and better fitted for maternity ; and there is none that such mature earlier, and are thus prepared for the same. It is all well enough to sound the grandiloquent statement, and affirm it "without hesitation," that that female is fit for such a marriage, and by so doing is acting "strictly in accordance with nature," who has obtained an "approximate or perfect development ;" "who has inherited no tendency to disease," "been subjected to no such abnormal agencies as would affect injuriously the reproductive organs." But excluding "woman as she is" among the civilized from this candidateship, and failing to produce her among the ancients and the savage races, where shall we find these ovulatory, ante-menstrual, marriageable beings, excepting in the Queen Mab regions of imagination or the gossamer weavings of hypothesis? The second question (as I have said) growing out of such teaching is—What are the therapeutical measures founded upon Dr. King's "new basis for uterine pathology?" I am aware that therapeutics are not a necessary link of pathology, and I should certainly have refrained from considering it had not Dr. King so plainly touched upon the subject in one of his eight preliminary propositions. The fifth of these propositions reads thus: "Organs that have undergone structural modifications adapting them to the modified functions entailed by modifications in the environing conditions, it is evident, can no longer be perfectly adapted to perform the original functions that belonged only to the original environing conditions. But when the new environing conditions are withdrawn and the old ones replaced, the organism will attempt to reconstruct itself back again to its original functional and structural state." Applying the plain teaching of this proposition to the so-called pathological condition of the menstruating woman, it means, in plain English, if it means anything at all, that the most natural and surest course for such a one to pursue, in order to get rid of her condition, is to enter upon the reproducing life at once, and keep it up as long as the generating powers will allow; and in view of the hazards of a manifold and painful uterine pathology which the puberle girl may run as the menstruating life advances, it strikes me (if I believed in this doctrine) that I should advise puberle, if not ante-puberle, mar-

riage, even in these civilized times, and tell the girl to take her chances. Under such convictions I would not assert, as Dr. King does, that "*woman as she is* ought to suffer the menstrual hemorrhage." I would affirm, as he does not, that "every female on attaining the age of puberty ought to marry and perform the reproductive office." But inasmuch as he avowedly does not allow this (again applying the practical teachings of his proposition), the legitimate inference would be, as the other horn of the dilemma, that we should cause femalehood to retrograde from civilization—to get back to its ancient and typical normality by uncivilizing as fast as possible. If these are not the remedial measures naturally based upon this new doctrine of uterine pathology and necessitated by Dr. King's fifth proposition, then, puerile as they are, I am at a loss to infer what they should be. I have now cursorily reviewed the twelve arguments presented by Dr. King, which, according to his statement, "ought to lead any unbiassed thinker" to the conclusion that menstruation is a pathological process.

The remainder of his article is taken up chiefly in explaining certain structural lesions of the uterus in the light of these teachings. Let us notice his stand on one or two points, and be done. On the subject of *areolar hyperplasia*, he is compelled to snag against two or three formidable facts, over which he cannot pass, from which he cannot back out, and around which he cannot navigate without resorting to metaphysical hypothesis. According to his own admission, they seemingly clash with his doctrines. These facts are, as taught by Dr. Thomas and others: 1st. That in the virgin womb areolar hyperplasia is seldom found—a feature in structural changes which plays the most important part in the whole field of uterine pathology; 2d. That in cases where the uterus has been once affected by gestation, areolar hyperplasia is exceedingly frequent; and 3d. That the almost all-pervading cause of uterine hyperplasia is sub-involution of the uterus, consequent upon pregnancy.

Let us see how he faces these facts. He says: "I am perfectly willing to admit as an undoubted fact that the 'areolar hyperplasia' of Dr. Thomas is more common during sub-involution than in nulliparous women; but I fearlessly add that the sub-involution itself is caused by the less marked, latent,

but inevitable tissue changes that have occurred prior to pregnancy, *as the result of the menstrual habit.*"

Then he quotes one of his "preliminary propositions" laid down in the beginning of his article to prove that this *must be so*. This surely is deductive philosophy with a vengeance. Now, in the name of all that is forcible in facts, candid and logical in reasoning, upon what grounds does Dr. King "fearlessly add" that sub-involution of the uterus is dependent upon and caused by tissue changes that have occurred prior to pregnancy as the result of the menstrual habit? None whatever, but the subjective premises of his favorite theory. And he in reality confesses as much when he says: "Unfortunately, writers give no definite information" as to whether "young females who become pregnant early in life are less liable to sub-involution and areolar hyperplasia than females who have menstruated ten or twenty years before becoming pregnant. In the absence of proof, I am, however, willing to leave it to future experience of gynecologists." When Dr. King can point out what those tissue changes actually are, by the microscope or otherwise, of which menstruation is said to be so prolific a cause, there will be time enough to entertain an investigation into their relationship with sub-involution. But even then we shall require for our conviction a sounder method of reasoning than that of *post hoc, propter hoc*. Another point embraces his views on "fibro-plastic tissue," said to be found in the mucous membrane of the uterus. He cites Drs. Robin and Bedford as declaring that this tissue is found there, and he presumes to explain the cause of its presence as follows: When the menstrual flow is past, the capillary blood-vessels, ruptured by the process, heal by the formation in their walls of "fibro-plastic cicatricial tissue." "Now," he says, "let the bursting and healing continue month after month and year after year, and can it be otherwise than that the internal lining of the womb shall have widely departed from its original and normal state?" Now when we consider the anatomical fact that the walls of the capillaries are a fine, transparent, homogeneous, almost structureless membrane, it is very questionable in my mind (reasoning theoretically) whether it be possible that fibro-plastic cicatricial tissue can form in it, to any significant extent at least. But granting that it does, and that the cause of it is the process described

above, it does not follow that its deposit is productive of any pathological condition, or that the cause of it is a pathological cause, any more than the constant rupturing of Graafian vesicles is a pathological process, and the deposit of fibro-plastic cicatricial tissue in the ovaries produces in them a diseased condition. Does Dr. King find in the mucous membrane of the womb of an old maid, just bordering on the change of life, a greater amount of fibro-plastic cicatricial tissue than he does in a young maid of twenty? He does not tell us. Is the virgin far advanced in her menstrual life, or after the menopause, more afflicted with diseases of the endometrium than the child-bearing woman at the same periods? We have ample evidence to the contrary; and yet, in keeping with the theory that menstruation is a pathological process, and leaves its inevitable pathological change in the womb's mucous structure, the virgin, of the two, should be the greater sufferer. In the light of such evidences what signifies, then, the fact that a small amount of fibro-plastic tissue is found in the mucous membrane of the womb? And now, as an evidence that this bursting of the capillaries in the menstrual act is not a pathological, but a physiological process, is it not a fact of some significance that the mucous membrane during and immediately after menstruation, and that of the gravid womb at term, undergo precisely the same retrograde metamorphosis, viz., that of fatty degeneration? Engelmann says: "To my mind the relation" (speaking of the relation between fatty degeneration and the rupture of the capillaries) "is one of cause and effect. Not only do I assert that the changes found to exist in the tissues are developed independently of the hemorrhage, but that they are in fact the cause of the hemorrhage." Menstruation, then, according to him, is caused by a fatty degeneration of the capillaries of the womb's mucous membrane, limited to its superficial parts. It is not a turgescence or an engorgement that effects this, "for," as Engelmann says in one of his conclusions, "the congestion of this organ cannot alone cause the hemorrhage, as we find a more marked hyperæmia, a greater turgor of the vessels in the pregnant uterus and its mucosa, and yet no hemorrhage follows." The rupture and emptying of the capillaries, then, in menstruation, is not a traumatic process. It is dependent upon a periodical substitution of fat in their walls,



whereby they become weakened, as if purposely, by this mysterious process of nature, to melt them away and accomplish the consequent hemorrhage. It does precisely the same thing at the full term of utero-gestation. In view of this peculiar process, as so markedly characterizing both, may we not be justified in regarding it as one evidence, at least, that menstruation, like parturition, is a physiological process? And whether or not fibro-plastic tissue is found in the walls of the capillaries as its natural consequence, affects not the question in the least.

Other points of interest might be dwelt upon but for want of time and space. They all more or less, in my humble judgment, exhibit the unpropitious feature of *forced deductions* from arbitrary propositions, and subjective, unsound arguments. In fine, let me conclude by commending to Dr. King for serious contemplation his own comment upon the doubtful "new basis" which he would furnish for uterine pathology, viz.: "At present, perhaps, positive experimental data are wanting to prove or disprove the ideas I have presented." The more he ponders and searches, the more he will see, I think, that he is ballooning in the mists and hazes of transcendentalism.

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## TRANSACTIONS OF THE NEW YORK OBSTETRICAL SOCIETY.

*Reported by* PAUL F. MUNDÉ, M.D., *Secretary.*

STATED MEETING, DECEMBER 15, 1874. THE PRESIDENT, DR. BYRNE, IN  
THE CHAIR.

### TRACHEOTOMY TROCAR.

DR. L. A. RODENSTEIN showed a curved trocar and canula, which was to be plunged into the trachea at once without making any incision or losing time in laying bare the trachea in any case, in which the immediate performance of tracheotomy was indicated. The canula is made of silver, and provided with small holes for the insertion of tape and the attachment of the tube around the neck of the patient. He had not yet used the instrument, but thought he should do so at the first opportunity.

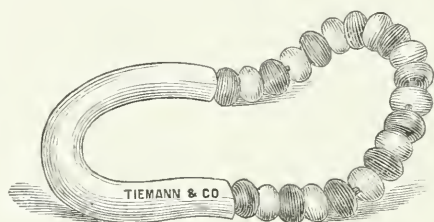
DR. POOLEY said that this idea was not a new one; that Arthur Durham in England had long ago advocated such an instrument. Although the plan had never met with general approval, still he would be inclined to employ it in an emergency.

DR. LEE said that Sir Henry Thompson had devised a similar trocar and cannula provided with lateral hooks to fix the trachea. He had tried it once, but found it so liable to choke the patient, and so difficult to fix the trachea at the moment of introduction and retain it afterwards, that he did not propose to try it again, certainly not unless he was provided with a good tenaculum.

### GALVANIC VAGINAL PESSARY.

DR. H. T. HANKS, who was present as a guest, related the case of a girl 19 years of age, of stout full habit, who had menstruated only five times since her 15th year. The vagina was tolerably patent and the uterus somewhat low. He introduced an Albert Smith pessary with the view of supporting the uterus and perhaps inducing it to menstruate regularly. Finding this ineffectual, he concluded to try the efficacy of gal-

vanic action, and, on the principle of the galvanic intra-uterine stem, he constructed a small closed lever pessary, composed of strong copper wire, on which copper and zinc beads were strung alternately. Finding this rather heavy, he removed the beads from the anterior portion of the pessary, which he covered with rubber tubing. The patient has now worn the pessary for three months, and has menstruated twice during this time. She had had general tonic treatment during all this period, but as it did not remove her amenorrhœa previous to the introduction of the galvanic vaginal pessary, Dr. Hanks is inclined to attribute the improvement to the latter. There were no local applications made. The uterus was  $2\frac{1}{2}$ " in length.



DR. BYRNE has used galvanic stem-pessaries of all kinds, and has never been able to satisfy himself that any specific galvanic effect was exercised by them other than that produced by any foreign body worn in the uterus for some time. He thinks that the superior efficacy of a galvanic *vaginal* pessary may be explained by the greater action of the acid secretion of the vagina on the galvanic elements over that exercised by the alkaline fluid of the uterus.

DR. PEASLEE thought Dr. Hanks' pessary a very ingenious idea. A possible objection to its efficacy might be the fact that the galvanic action is exercised on the vagina, whereas the menstrual flow comes from the internal surface of the uterus alone.

In answer to a question by Dr. Byrne as to his success with stem-pessaries in flexions, DR. PEASLEE said that he still uses them in appropriate cases, of which there are, to be sure, not very many, if proper care and caution is exercised in their selection. He thought that antelexions are better suited to the use of stem-pessaries than retroflexions; that an antelexion of the third degree will often be changed by the wearing of a stem for two or three months into a flexion of the first degree; that conception will often take place after the use of a stem-pessary, and lastly, that old flexions are not curable by this or any other means.

DR. BYRNE thought that the benefit derived from stem-pesaries had been overestimated; he had never been able to do more with them in antelexions than to temporarily relieve the distressing symptoms resulting from the pressure of the fundus uteri on the bladder.

CASE OF SUPERNUMERARY INCISOR.

DR. DAWSON showed a cast which he had made of the upper jaw of a girl 12 years of age, in which a small pointed tooth grew straight down between the two median incisors. He considered it a unique case. There was no hare-lip, intermaxillary bone, or other congenital deformity present.

DR. JENKINS said that he had seen an exactly similar anomaly in an adult, and DR. ROBERT F. WEIR remarked that a lady-patient of his had the same deformity.

CASE OF UTERUS SEPTUS ET VAGINA SEPTA.

DR. HENRY F. WALKER related the following case of uterine malformation. A young lady, who had menstruated for the first time in her 18th year, and whose menses returned almost every two weeks, often lasting ten days, came to him for advice. She had then been ill for six months. Deeming an examination necessary, he attempted to introduce his finger into the vagina, but finding some resistance, which he attributed to a very rigid hymen, he deferred the exploration until it could be performed under ether. He then found the following extraordinary condition: The vagina was completely divided by a firm septum extending up to the cervix; there were two entirely distinct and perfect cervices, one being somewhat less developed than the other, two external orifices and two entirely distinct uteri, into which the sound could be passed parallel to the median line to the depth of  $2\frac{1}{2}$ " ; there was no divergence of the two uteri, and their complete separation by the septum could be ascertained by introducing a sound into each uterus at the same time. Each vagina permitted the introduction of a vaginal Sims' speculum, by means of which the presence of a double cervicitis was detected, which was subjected to treatment, after which menstruation came on twice normally at regular intervals. He had thought of the possibility of each uterus menstruating by itself every fortnight as an explanation of the menorrhagia, but was more inclined to believe that both uteri menstruated together, and that the flooding was caused by the endocervicitis. The malformation is one described by Kussmaul as "uterus septus et vagina duplex" or "septa," which variety is more pro-

bable than the "uterus didelphys," in which the two uteri are perfect and entirely separated from, not touching each other.

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STATED MEETING, JANUARY 5, 1875. THE PRESIDENT, DR. BYRNE, IN THE CHAIR.

#### ARABINATE OF QUINIA.

DR. DAWSON exhibited a specimen of the arabinate of quinia, a new preparation discovered by George W. Kirchhoffer, apothecary, 603 Second Avenue, in this city. Its advantage consists solely in its somewhat improved taste, it being less bitter than the usual preparation of quinia, and therefore better adapted for administration to children. It is rubbed up with syrup, making an emulsion. The dose requires to be somewhat increased, about five grains of the arabinate being equal to three grains of the sulphate. The price is also somewhat higher. Dr. Dawson had tried it with several children, and found that they took it much more readily than the sulphate.

DR. NOEGGERATH said that quinia is best disguised in carefully prepared coffee-syrup.

DRS. REYNOLDS and THOMAS said that they give it to children either in small chocolate-drops, or preceded and followed by chocolate, generally with little difficulty.

#### CASE OF CONCEALED DIPHTHERIA.

DR. REYNOLDS read the detailed report of a case of death from concealed diphtheria; the child dying on the fourth day without the appearance of any diphtheritic membrane on the fauces, which, as well as the lips and tips of the fingers, were livid and purplish. Excessive vomiting and diarrhoea, and great tenderness over the liver, were prominent symptoms. The autopsy revealed merely an active engorgement of the mucous membrane of the stomach and five feet of the small intestine, with bright-red blood in ridges, with here and there a patch of less intense redness. He thought that the child died from the poison of diphtheria (there being both diphtheria and scarlatina in the hospital in which the child was living), but felt as though this explanation was hardly sufficient.

DR. NOEGGERATH said that he had no doubt of the correctness of Dr. Reynolds' diagnosis, for he had seen two cases of diphtheria in one family some four years ago, the younger of which presented the same livid appearance about the mouth and tips of the fingers, particularly during expiration. He explained this lividity by the destruction of the red corpuscles of the blood by the diphtheritic poison, and the consequent dearth of



oxygen in the blood. The same cause would explain the congestion of the stomach and intestines in Dr. Reynolds' case.

DR. BYRNE asked Dr. Reynolds whether he had any confidence in local applications in diphtheria.

DR. REYNOLDS said that he had, when the membrane was fresh and could be detached. The solution which he has used of late is one first employed by Dr. J. Lewis Smith, of this city. R. Ac. carbol. cryst., gr. xix. ; ferri persulph., ʒ ij. ; glycerine, ʒ ij. M. He has lately omitted the iron, because he found that it led to the formation of large intractable ulcers. In any case the membrane will return ; the local application should be made as often as three times a day.

DR. NOEGGERATH said that his experience resembled that of many other physicians ; he had frequently made local applications with great success, and again he had met with cases in which they were not only unsuccessful, but even injurious, the disease spreading more rapidly after their use. He thinks that a great deal depends on the manner of application. By scraping off the membrane, and roughly applying the local remedy, we open the vessels for the admission of the micrococci, to which diphtheria is doubtless due. The application should always be made by the physician himself in the gentlest manner possible with a soft brush, taking care not to disturb the membrane. He finds that the sesquichloride of iron causes too much swelling and congestion, and relies now principally on the concentrated carbolic acid applied with a brush.

DR. T. G. THOMAS said that he had used all the various local remedies in vogue with no satisfaction. He now uses the wine of pepsine, which seems to act as an efficient disinfectant and digests, as it were, the membrane. He makes or directs a frequent application in the form of spray, or with a syringe, as often as every hour during the day and night, giving the child a teaspoonful of the wine now and then as a tonic *ad libitum*. If the patient is old enough he is directed to use it as a gargle. He has experienced exceedingly good results from this treatment. In very young children the wine is diluted.

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STATED MEETING, JANUARY 19, 1875. THE PRESIDENT, DR. BYRNE, IN THE CHAIR.

DR. HENRY F. WALKER related

#### A CASE OF SPONTANEOUS VERSION

which he had observed some time previously. The fœtus was in the eighth month, and had been dead some three weeks. The

head was in the right iliac fossa, both arms in the vagina, the thorax presenting. By means of active uterine contractions the head was pushed down, the arms retreated, and the child was born in a vertex presentation.

#### ELECTRICITY AS AN OXYTOMIC.

DR. F. D. LENTE, of Cold Spring, who was present as a guest, related a case of puerperal eclampsia in the seventh month of pregnancy; premature delivery was induced with Barnes' dilator, the child turned and extracted up to the head. At this point the uterus was felt to be flabby, and in apprehension of post-partum hemorrhage, measures to excite uterine contractions were suggested; Dr. Lente happened to have a faradic battery at hand, which had been used with success to control obstinate vomiting in the same case, and he concluded to try it as an oxytomic, as likely to act more rapidly than ergot or any other remedy. He applied the poles to the sacrum and abdomen, and put on a strong current, producing at once such violent uterine contractions as to render the extraction of the head impossible, it being grasped by the cervix as in a vise. Only after continued steady traction and dilatation of the os with the fingers could the head be delivered. The battery was then reapplied, and firm, permanent contraction of the uterus produced. Dr. Lente wished to call attention to the superiority of the faradic current over the slow action of ergot in cases of tedious labor from inactivity of the uterine fibres and post-partum hemorrhage, and also to the danger of using too strong a current, and causing contractions, which it might be difficult to allay when desirable.

DR. BYRNE asked Dr. Lente whether he had ever used Molesworth's small dilators to distend the cervical canal in the non-pregnant uterus. He himself had never been able to dilate the cervix sufficiently at one sitting to permit the introduction of the finger.

DR. LENTE said that he had never used Molesworth's dilators in the non-pregnant womb. In one case of convulsions in the sixth month of gestation he had succeeded in dilating the cervix with Molesworth's instrument after having failed with spongetents. Finding that the contraction of the cervix was too firm to allow of its immediate dilatation, and that the part of the dilator in the uterine cavity expanded and the portion in the cervical canal remained nearly unchanged, he adopted the plan of gradually drawing the dilated tip into the cervical canal, using it as a wedge, and injecting more water into the dilator as the cervix became wider, until he had dilated it sufficiently to permit the introduction of a larger sized dilator.

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STATED MEETING, FEBRUARY 16, 1875. THE PRESIDENT, DR. BYRNE, IN THE CHAIR.

CASE OF TUBAL PREGNANCY SUCCESSFULLY TREATED BY A NEW METHOD OF OPERATION.

DR. T. G. THOMAS exhibited a foetus of the third month, which presented nothing unusual in itself, and was remarkable only for its history, which is as follows :

A lady, who had always menstruated regularly, and never been pregnant, was last unwell Oct. 25th, 1874, the menses apparently ceasing in consequence of pregnancy, various signs of which were present. In the second month a tumor became palpable in the hypogastrium. Five weeks ago, less two days, she was suddenly seized with violent pains in the left iliac region ; a physician was at once called, and found her with severe bearing-down pains, and bathed in perspiration. Repeated doses of Magendie's solution hypodermically, failed to relieve the pain until narcotism was induced. The pain returned after several days, and at repeated intervals with such intensity as to cause the husband to request to be instructed in the hypodermic use of morphine, in order to avoid the delay attending the arrival of the physician, who lived several miles away. An examination by the medical attendants revealed only an indistinct hardness in the left iliac region.

Dr. Thomas first saw the patient Feb. 4th, twelve days ago. From the history he was inclined to think the trouble renal calculus, but on examination he found a cyst to the left of the uterus, which was as large as the latter, and reached down as far as the middle of the cervix. The uterus was measured by the sound, and found to be  $3\frac{1}{2}$ " in length, and ante-latero-flexed. He, without hesitation, made the diagnosis of extra-uterine pregnancy, which was principally based on the distinct detection of the rolling of a mass within the cyst (ballotement).

He considered the violent pain to be contractions of the cyst precursory of its rupture, and advised operative interference. Dr. Sims was called in to see the case, and agreed with the opinion reported to him as having been expressed by Dr. Thomas. The family were anxious to have something done, and Dr. Thomas decided on removing the foetus. The operation was performed Sunday, Feb. 7th. Having seen in all nine cases of extra-uterine fetation, four before rupture, all of which he had diagnosed, and having therefore some experience in the matter, Dr. Thomas decided to try a plan of operating different from the main operations—gastrotony and tapping the cyst, and injecting it with medicated fluids, neither of which gives very

successful results. The operation performed in this case was the following :

By avoiding the peritoneum it was hoped that the danger of peritonitis would be removed. The patient was placed in the semi-prone position, and Sims' largest speculum introduced ; the cervix was seized with a tenaculum and pulled to the right, the left wall of the vagina with another tenaculum and drawn to the left, thus putting the left vaginal insertion sharply on the stretch. With the knife of the galvano-caustic battery (Byrne's, which worked admirably) an incision two inches in length was made from the cervix to the left ilium, six minutes being occupied in performing this part of the operation, during which no blood whatever was lost. The cyst could then be felt and was punctured, giving sudden exit to a quantity of light-pink liquor amnii, but no blood. The view frequently entertained, that the contractions are caused by the effusion of blood into the sac, is thus disproved in this case. The foetus lay transversely, with its head towards the cervix. The fingers were introduced, the foetus turned and easily extracted as far as the head, which could only be brought through the incision by seizing it with the placental forceps. The umbilical cord was broken ; traction was made on the cord, and the placenta readily followed, when a rush of blood of such violence occurred as to reduce the patient to syncope and necessitate the leaving one-half of the placenta still adherent. One ounce of undiluted liquor ferri was injected into the sac, temporarily arresting the hemorrhage ; it soon returned, however, and a roll of cotton, saturated three days before in a solution of persulphate of iron and dried, was packed into the sac, and controlled the bleeding. The patient complained of acute pain in the sac after the operation similar to that experienced before, probably due to contractions of the sac. She did well until the Thursday following, when the tampon was removed from the sac, the vaginal tampon having already been removed forty-eight hours after the operation. Carbolo-glycerine and water was injected into the sac. On the following day, Friday, the patient was much worse, and showed all the symptoms of septicæmic poisoning ; temperature  $104^{\circ}$ , dry tongue, prostration, etc., and a fatal issue appeared imminent. An elastic tube was passed up into the sac, and a weak solution of carbolic acid injected. This operation has been repeated every six hours by an assistant until the present day ; the patient has been gradually improving, the temperature is nearly normal, and the lady bids fair to recover.

Dr. THOMAS mentioned a case which he had seen with the late Dr. Henschel, in which the cyst was tapped with an explorative trocar to which a Davidson's syringe was attached

(Dieulafoy's aspirator being then unknown), and the first puncture yielded only a brain-like substance, the liquor amnii flowing out at the second attempt. It was found that the needle had, at its first introduction, pierced the brain of the fetus. It is well to remember this possibility when tapping the cyst in extra-uterine pregnancy.

DR. NOEGGERATH read a paper on

THE VESICO-VAGINAL AND VESICO-RECTAL TOUCH; A NEW METHOD OF EXAMINING THE UTERUS AND APPENDAGES.<sup>1</sup>

DR. PERRY said that he had examined the uterus through the bladder in three instances: 1. With the sound he detected a tumor in the bladder, but could not decide whether it belonged to the bladder or the uterus. Rapid dilatation of the urethra was performed with the finger, which was passed into the bladder, and discovered that the tumor arose from the uterus and projected into the bladder; 2. A case of Dr. Sims; 3. The urethra had been so much dilated by masturbation, that the finger when introduced under the clothes, passed at once into the bladder instead of the vagina. There was perfect retentive power of urine. The facility of the examination *per vesicam* was exceedingly obvious, and he should never hesitate to employ it in doubtful cases. In cases, however, where there is unusual vascularity of the urethra, or where a long-continued uterine or vaginal discharge has softened the tissues and rendered them brittle, he would hesitate to employ the method, for fear of producing laceration of the urethra.

DR. THOMAS thought that the method of investigation proposed by Dr. Noeggerath was of great value, and had a great future. He wondered that nobody had ever employed it before, its application seemed so simple and its utility so evident. He had often passed his finger into the bladder solely to explore it, but never for the diagnosis of uterine tumors. He would like to ask Dr. Noeggerath whether the method would be likely to be serviceable in differentiating fibro-cystic tumors of the uterus from ovarian tumors.

DR. NOEGGERATH said that he thought it would be useful in large uterine tumors, and when the uterus was drawn down with its normal fundus below the fundus of the bladder. In answer to a question by Dr. Thomas, whether there was inability to retain the urine after the examination *per urethram*, Dr. Noeggerath said that there was frequent micturition for several days, but no lack of retentive power. His impression is, that the manipulation is rather severe, considering that it is

<sup>1</sup> See May Number of this Journal.



employed only for the purpose of diagnosis, and that consequently it should be adopted only when absolutely indicated, not where other means will enable us to make the diagnosis with equal facility and certainty.

Dr. THOMAS said that in a case at his clinique of a woman without a vagina, the urethra had been used for copulative purposes, and there was no retentive power whatever present.

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STATED MEETING, MARCH 9, 1875. THE PRESIDENT, DR. BYRNE, IN THE CHAIR.

Dr. THOMAS related the termination of the case, and entire recovery of the patient with

#### EXTRA-UTERINE PREGNANCY

reported by him at the last meeting. With the exception of a large fecal impaction, which had to be removed by mechanical means, the patient continued to do well, the pulse never rising above 84. The other half of the placenta which had been left in the sac for fear of again producing the fearful hemorrhage which followed the removal of the first half, was cast off about a fortnight after the operation.

In answer to a question by Dr. Lusk, whether he considered the operation performed by him safer than the puncture of the sac, Dr. THOMAS said that he certainly did so consider it; that the two cases of extra-uterine pregnancy in which he had punctured the sac had both terminated fatally—one from secondary hemorrhage, the other from septicæmia. In this last case he thought that it was unfortunate that a portion of the placenta was retained, although the patient had finally recovered. He is inclined to believe that if he had boldly and rapidly removed the placenta at once, the hemorrhage would have been avoided. At the time he was afraid that too long traction might draw the sac down too far and lacerate it.

Dr. DAWSON exhibited a

#### MODIFICATION OF SIMS' SPECULUM,

devised by him, which consists in the blades being attached by hinges to the manubrium, thus permitting the former to be closed and making the instrument far more convenient to carry.

Dr. F. D. LENTE, of Cold Spring, read the following paper on

#### INCOMPLETE LACERATIONS OF THE PERINEUM.

Since so little has been said in our systematic treatises, and even our periodical literature, about lacerations, and especially incomplete lacerations of the perineum; and since the attention

of the profession has been attracted to this subject by Dr. M. D. Mann, in the October number of the AMERICAN JOURNAL OF OBSTETRICS, with an implied call for additional information and statistics, I propose to give briefly the results of my own observations in the hope that others, who may have had a larger experience in obstetrics, may follow the example, and endeavor to set this question at rest, and to establish some principle of action. For, as the matter now stands, it is a difficult question for a young practitioner to decide when he finds himself at the threshold, perhaps, of his obstetrical practice, confronted by a considerable laceration of the perineum, probably after an instrumental delivery; the matter being still more embarrassing if it has occurred in a family of influence. If he should call in neighboring consultants of larger experience, his dilemma would not likely be resolved, as a disagreement would probably be the result, and the notoriety given to the occurrence would only enhance his embarrassment. What then shall he do? He will turn over the leaves of his authorities, if he happen to have a good library, in vain. That is my experience in other difficulties besides this; and, until authors write more from their case-books, and are less guided by mere memory of experiences long past, it will continue to be so.

If the laceration extend through the sphincter ani, and through that triangular space called the perineal body, so accurately described by Dr. Thomas, and thus lay the rectum and vagina into one cavity, we may take it for granted that operative interference will sooner or later be required. Probably there would be no difference of opinion here. But what shall be done with the cases of "superficial" laceration? It is only to these that Dr. Mann has called attention.

In the first place, as regards their frequency, my own experience accords with that of others quoted by Dr. Mann. It is quite common in *primipare*, and after forceps. Moderate cases are, as he says, doubtless constantly overlooked, and I think fortunately so. Dr. Stokes, long ago remarked, in the case of the complication of pneumonia with typhoid fever, it is usually fortunate that the practitioner fails to recognize it, as he might be tempted to treat it. We have somewhat better ideas now of therapeutics, and we know that generally the pneumonia will take care of itself if we can manage the fever. So it is, in my opinion, with these lacerations.

In the next place, how does it occur? For, a knowledge of the cause may lead to prevention, and this is better than cure. The causes which are *generally* recognized we need not stop to consider. Dr. Mann enumerates three. Then he remarks:—"Many perineæ are ruptured by a careless delivery of the

shoulders after the head has passed." This is so. But many are ruptured by the shoulders which are *not* attributable to carelessness. And as too strong an assertion on this point might lead to annoying suits for malpractice, especially in neighborhoods cursed by a doctor given to fostering such suits, it is well that it should be understood that a ruptured perineum, even after an instrumental delivery, does not by any means imply any want of skill or care on the part of the accoucheur. The burden of proof in such cases ought to rest on the prosecution.

Not long since, in a prominent journal, I read an article by a physician of apparently large obstetrical experience, in which he contended that rupture was scarcely ever, if ever, attributable to the passage of the shoulders. Singularly enough, in the same number was a table containing a large array of statistics, showing that this is an exceedingly common cause; and so in my experience it is; in my forceps cases especially (and it is here that it would be most likely noticed when it occurs), it has been the shoulders which have done the damage. I have demonstrated this more than once to my assistants, that the head had passed safely, that the forceps had been removed, and that a bad laceration has then been produced by the shoulders. A good deal of stress is sometimes laid on the importance of removing the forceps before the head passes the perineum; but a little examination will show that, although this may easily be done, it is of no consequence, as the blades cannot appreciably increase the diameter of the head, as they would do were they solid instead of fenestrated.

Granting that the shoulders are the dangerous portion of the foetal body, and it is conceivable that a comparatively sharp object *should* be so, rather than a globular one—what can be done to avert the catastrophe. "Supporting the perineum," in the ordinary acceptance of the term, would only make matters worse, as it always does, by stimulating reflex action. The only feasible plan would seem to be to retard the advance of the presenting part as much as possible, and endeavor to ease it through gradually pressing it forward also, perhaps, as it is passing.

Now, as regards *what should be done when the laceration has occurred*. As I have before hinted, if moderate—that is, in linear extent—it will, except in instrumental cases, most likely be overlooked; and fortunately, as it will save the patient a good deal of worry, and the doctor also, if he be inexperienced. If it extend nearly to, or quite to the sphincter ani, and perhaps a little around one side of the anus, what shall be done? This is the point which needs to be better settled than it is. I only

design to give my own experience, and make no reference to that of others. Others have indeed said too little about it. My first case disturbed me very much, and I worried and alarmed my patient and her friends not a little by stating pretty positively that, although no danger need be apprehended, an operation at some future time would be necessary. Weeks after the accident, although the patient had had no particular care, and no unusual precautions were taken, the wound had healed, and the patient appeared to feel no inconvenience. This has been my fortunate experience ever since in a good many cases; for I do not agree with Dr. Mann that these accidents may always be prevented. Among the poor, I have not even given directions for keeping the patient on her side in order to prevent the urine and lochia from irritating the wound, because they would probably not regard them if I did, and they are seldom necessary.<sup>1</sup> It is astonishing how little suffering such a wound occasions. In one case, I remember, in which Dr. Murdock of this place assisted me, I delivered with the forceps, and showed him that not the slightest laceration had occurred from the passage of the head, and that, as the shoulders passed, the perineum was rent quite to the sphincter ani, extending a little around one side of the anus. It was rather a nice patient, a primipara, and as she had a competent nurse, I considered it best to acquaint both with the occurrence, merely to insure extra cleanliness. I might as well have kept it to myself, as the patient felt not the slightest inconvenience from it. Months after, I examined the parts carefully, in a good light, and positively could not discover a trace of the accident. Even the fourchette appeared perfect. This is of course an exceptional case. It is generally easy to see the cicatrix, if one looks for it carefully. I will briefly refer to a more astonishing case than this, though it did not occur in my own practice, but it should be put on record, since Prof. Thomas does not admit, in the last edition of his now justly celebrated treatise on Diseases of Women, that there is any well-authenticated history extant of such a case. I have remarked that it must be taken for granted that a laceration through the sphincter ani must be operated on. But even this has its exceptions. Some years ago two of my intimate professional acquaintances were interested in such an exceptional case. One was the late Prof. Geo. T. Elliot, the other Dr. John G. Perry. The vagina of this lady was the smallest and most rigid which Dr. E. had ever met with, which led him to caution her friends that laceration would probably occur. It became

<sup>1</sup> There is one precaution which is important, and which I always enjoin, the *recumbent posture* for at least nine days; but this is also insisted on in ordinary cases, though not always observed.

necessary to resort to the forceps, and although he used the smallest and lightest in his possession, and all the usual skill and care for which he was distinguished, an appalling laceration did occur, splitting the sphincter ani and the vagina *throughout its whole length to the bottom of Douglas' cul-de-sac*. He was of course horror-struck at its magnitude, and naturally solicitous to repair damages at once. But Dr. Perry, whom he strenuously urged to operate, very properly, in my opinion, although specially qualified to do so, declined to accede to his request. His own cooler judgment told him subsequently that it would not have been prudent, and the unlooked-for result proved that it would have been entirely unnecessary. The patient's bowels were not kept confined by opium, as is usually enjoined in such cases, but were solicited to act regularly by enemata. Dr. Elliot afterwards informed me that this extensive wound healed promptly, spontaneously, and thoroughly. He attributed the spontaneous healing of the vaginal portion to the firm and rigid condition of its walls, the very circumstance which doubtless led to the accident. Dr. Perry has since attended this lady in a confinement which presented no unusual difficulty, and the perineum has stood the test a second time in Paris. Her health has continued good.

I was writing this paper for occupation, while detained at the house of a patient with the most tedious case of labor which it has been my fortune to encounter, with one exception, and was interrupted just at this point by the necessity for employing the forceps, and a means was thus offered me on the spot of exemplifying some of the ideas previously expressed in this paper. Dr. Murdock, my assistant in practice, had been summoned a few hours before to assist me in the use of first Molesworth's and then Barnes' dilator for facilitating the dilatation of an unusually rebellious os, and had been allowed to depart, so I had to operate alone. The head was unusually large (the child weighed  $10\frac{1}{4}$  lbs.), and with all my care in imitating, as far as possible, the natural process, there being a good light on the parts, I saw the perineum tear slowly to the extent of an inch or more in the median line. I was almost certain that the shoulders of so large a foetus would extend this to the sphincter ani at least, but pursued the plan, the description of which, singularly enough, had been written scarcely an hour before—that is, to press the shoulders forward towards the arch of the pubes, and upward in the axis of the vagina. This was perfectly successful, as I was gratified to see, while closely observing the part, that there was no further injury inflicted. Indeed, this manoeuvre seemed to take almost all the strain off the perineum.



Of course every one of any considerable experience knows that it is not uncommon to see a short perineum resulting from these rather extensive lacerations, and causing a good deal of trouble, a want of support to the vagina, prolapsus of rectum and uterus, and retroversion, congestion, etc., and that *perineorrhaphy* is the only radical cure when these annoyances are of sufficient importance to render a resort to operation advisable. It is, however, under these circumstances one of the simplest, safest, and most successful of surgical procedures. But the ease would be far different were it done immediately after labor, which, in such cases, is apt to have been a protracted, an instrumental, and perhaps a complicated one. Drs. Joulin and Mann give unanswerable reasons, I think, for not resorting to primary operation. One recommendation of the latter we may adopt with benefit, perhaps, and without attracting the patient's attention to her condition: to have the ordinary binder broad, and let it extend well down below the trochanters, as indeed it always should do. This, if tightened daily, will restrict the movements of the lower limbs sufficiently, and thus, to some extent, prevent motion of the edges of the wound. His suggestion of what he terms the "Vienna method" in such cases as extend to, or nearly to the sphincter, is also probably a good one, viz., to apply strong *serrefines* to the edges of the laceration. It would appear from Dr. Mann's cases at the Vienna Hospital that union is more apt to occur, whether they are used or not, in private practice than in hospital, as might be anticipated.

DR. T. G. THOMAS said that he was opposed to operating at once in such cases as the one observed by Drs. Perry and Elliot, and quoted by Dr. Lente, because the patient is too much exhausted, and the union of the laceration too extensive an operation for her already enfeebled system. Smaller lacerations to or merely through the sphincter ani should be closed at once; still smaller ruptures than these last require no treatment. A general rule whether to operate immediately or follow the plan of non-intervention, can scarcely be made. The only general rule he knows of in this connection is this: There are four varieties of laceration of the perineum. 1. A slight distance into the perineum; 2. To the sphincter ani; 3. Through the sphincter ani; the last two require immediate operation; 4. Some distance up the rectum; these should not be touched, as there is little prospect of success to be expected from an immediate operation.

DR. LEISK said that in his experience the proportion of cases of success after the immediate operation in hospital practice was about one-half.

DR. LEE said that there was some difficulty in determining the exact extent of the laceration when the parts were swollen and turgid, as they usually are, immediately after labor; that he had seen an apparently small laceration appear a very large one when the swelling subsided.

DR. MUNDÉ said that in his experience, which had been quite large in this respect, he had found that, on the contrary, lacerations usually appeared larger immediately after delivery, when the perineum was distended and the vulva swollen, and that they diminished in size after a day or two when the parts had regained their normal relations. He had repeatedly seen lacerations of  $\frac{1}{2}$  to 1" in length unite by merely tying the legs together, passing no catheter, but letting the patient urinate over the perineum, and then pouring warm water over the external genital organs. He had also seen ruptures, which did not unite after immediate suture, heal by second intention during the remainder of the lying-in period. In the Lying-in Hospital at Würzburg, Bavaria, under Prof. Scanzoni, all perineal lacerations were immediately closed by thread or silk sutures, and with very good results. Dr. Mundé thought that of the cases operated on by him from  $\frac{1}{2}$  to  $\frac{2}{3}$  recovered; one, he remembers, so perfectly as to show no trace whatever of the line of union. The method of immediate operation is so universal in Germany, that Dr. Mundé was surprised on hearing that the question was still an open one in this country.

On motion of DR. NOEGGERATH, it was moved to continue the discussion at the next meeting.

STATED MEETING, MARCH 23, 1875. THE PRESIDENT, DR. BYRNE, IN THE CHAIR.

DR. NOEGGERATH opened the discussion on the

#### TREATMENT OF LACERATION OF THE PERINEUM

by reading the following paper:

After the discussion on ruptured perineum at the last meeting of this Society, it appeared that the statement made by Dr. Lente as an introduction to his paper on the lesion in question, had remained almost in *statu quo*. He remarked:

"As the matter *now* stands, it is a difficult question for a young practitioner to decide, when he finds himself at the threshold, perhaps, of his obstetric practice, confronted by a considerable laceration of the perineum."

It was therefore resolved to continue the discussion on that important topic, and I will take this occasion to contribute my

share in the matter as far as I am able to do so. I will first briefly touch upon the proceedings which have been mentioned during the discussion—proceedings for which it is claimed that they were apt to prevent rupture of the perineum.

The first, which was incidentally mentioned, has been usually designated as Ritgen's Manœuvre. It consists in the expulsion of the head by means of two fingers placed in the rectum. It was, however, recommended already in 1781, by Drs. C. L. Hofman and Hagen, and by Smellie as far back as 1754. It seems, however, not to have received the attention due to it, perhaps, from its reliable efficacy. Dr. Ahlfeld has lately again pointed out the great value of this manipulation in the *Archiv für Gynæcol.*, vol. vi., No. 2, 1874. Its principal object is to force the head into a space near the symphysis pubis, which is usually not occupied in the ordinary process of parturition. In this manner the head is lifted to a considerable extent away from the perineum. Another advantage of this proceeding consists in the fact that the perineum is kept succulent in the interval as it is during a labor-pain, in consequence of which the tissues become relaxed. With Ahlfeld quite a number of prominent obstetricians employ it with success at the present day.

The value of the second proceeding, scarifications and incisions of the perineum, although spoken of in the highest terms by some authors, has not been so successful in the hands of others. Dr. G. F. Abegg, Chief of the Royal School for Midwives at Danzig, speaks of it in the following manner: "The lateral incisions of the perineum I consider entirely useless. I used to employ them frequently in former times, but I have become convinced that in those cases where the perineum was not ruptured, it would not have suffered even without the incisions. On the other hand I have seen, after very large lateral incisions, such extensive lacerations of the perineum, that they could not have been larger without the use of the bistouri."

The able article of Dr. M. D. Mann, in the *AMERICAN JOURNAL OF OBSTETRICS*, has been the primary incitement to Dr. Lente's paper, as well as to our discussion, and it would seem that the very fact of its calling forth a discussion on this subject is proof sufficient that the proposition laid down in his paper has not had the influence with our specialists which it was expected to exercise, from its scientific value as well as from the confidence placed by the author in the recommendations resulting from its contents. This reasoning is strengthened by the tenor of the discussion at our last meeting. The members of this Society were evidently not convinced of the value of the serres-fines in the treatment of ruptured perineum. They have been

weighing the arguments *pro* and *con* of all other proceedings to the exclusion of this apparently simple method of treatment. The *à priori* reason of this fact seems to me to be founded in the conviction that such a grave and serious lesion as a deep rupture of the perineum would not find means of repair in the application of a few serres-fines.

I will here remark that the term "superficial" rupture of the perineum in the heading of Dr. Mann's article is a misnomer for the lesion which he describes, inasmuch as he designates by that name those ruptures which reach up to the sphincter ani muscle. Now, when all of the tissues of the perineum are involved, we can no longer call it a superficial rupture.

What then is the value of serres-fines in complete rupture of the perineum?

Let us first see what Prof. C. Braun, the head of the Vienna School, which seems to have monopolized their use in our days, has to say about their application. In his well-known *Klinik der Geburtshülfe und Gynæcologie* he says: We also have seen their use followed by the desired result in many cases. In other instances, however, this method of closing the rupture has no good effect at all. After describing the conditions under which they must be applied, he goes on to say: A corroding discharge or restlessness of the mother more frequently prevents the success of the method. Patients of irritable constitution do not support them at all, since the application as well as the changing of the clamps is pretty painful.

Serres-fines have been employed pretty extensively all over Germany, but apparently abandoned at the present time. Hoogeweg, of Berlin, cured 27 out of 38 cases. At the *Maternité*, in St. Petersburg, 12 were cured out of 82 cases, while Grenser had no success at all with them. Prof. Winckel employed them in 36 ruptures occurring in the Clinic at Rostock, 17 of which were healed more or less completely. According to his experience, they produce pain for a longer time than sutures, while they cut through just as readily as the latter, and where their ends are attached they are apt to cause ulceration.

Prof. B. Schultze, the successor of Hoogeweg, tried them faithfully at first, but abandoned them finally for the suture, because the results from the latter proceeding far exceeded those of the former.

With the knowledge of these facts and opinions before us, it is proper to say, that the value of serres-fines is far from being established sufficiently to recommend them as *the* measure to be adopted in the treatment of deep rupture of the perineum.

What then is the proper treatment for these cases? It has



been suggested in the paper by Dr. Lente, that it might be the best plan for the physician to ignore the existence of a rupture, partly on account of the possible injury done to the reputation of the attending physician, partly on account of the fact that a great many cases heal spontaneously. With regard to the former point, it is my firm conviction, that in framing therapeutical indications the welfare of the doctor should never be taken into consideration. Moreover, I believe that the damage done to the profession at large would be greater, if physicians were to ignore the existence of the lesion, and leave it to the patients to find out their condition afterwards—than if the accoucheur would deal fairly with every one of those that have confided their welfare into his hands. Let the physician prepare the friends and relatives of the patient for the possibility of such an occurrence, and quiet them with the assurance that he will put forth his best endeavors to avoid the accident. Should it then take place after all, he will not be blamed for ignorance or want of proper care.

True, a large number of ruptures do heal up without any interference. But all authors agree at the present time that the parts, although healed over, are not reinstated to their natural condition; that they are unfit to perform the physiological functions allotted to them. The drawbacks for the future welfare of the patient have been sufficiently pointed out again and again by writers on gynecology, that I refrain from repeating them on this occasion.

What, then, is to be done in the presence of a rupture of the perineum? Nowadays there is not a dissenting voice among the leading obstetricians. To prove this I quote the words of PROF. VON HECKER, of Munich. He says:

“In no department of the obstetric science have the views been advanced and corrected so much during the last twenty years as in the prophylactic and curative treatment of rupture of the perineum; almost all over Germany the same principles are represented, and it is fair to say that the questions on which there exist different opinions are of decidedly minor importance. In my opinion,” he goes on to state, “the right way to manage every rupture, is the application of the suture as soon as possible after delivery.”

The reason why the early operation had been abandoned for some time, as a most unreliable proceeding, is explained by the want of knowledge of those conditions that are requisite to insure success; and in the same measure as operations for other lesions of the female genital organs—that for vesico-vaginal fistula, for instance—have received an incredible impetus in consequence of our improved means of operating, in the same



measure the opinion has been generally adopted, that the immediate suture in all cases of rupture of the perineum is the only rational method of treatment. Prof. Hecker gives a report of the success of the immediate operation of forty-two ruptures, which took place among 1,584 deliveries, nine of which involved all the parts up to the anus, without injuring the sphincter; twenty-eight were cured entirely, four partially, ten did not heal up.

From the reports of the Lying-in Hospital of Dresden, published by Prof. F. WINCKEL in 1874, it appears that thirty-two ruptures, three of which reached to the sphincter, occurred in 1868 after 775 deliveries. The operation was followed by complete success in twenty-two cases, by partial success in three, while seven remained open. In 1869, among 739 confinements, forty-five ruptures took place, one of which passed completely through the sphincter ani; thirty-eight of these healed entirely by first intention, two only partially, no success in four cases, one was operated later. In 1873, 1011 women were delivered; the perineum was ruptured 115 times. In fourteen cases the lesion was not severe enough to call for interference. In 101 cases the immediate operation was performed, and apparently with full success. The report does not mention the contrary, nor does another report on the condition of the patients, when they left the hospital, from the 10th up to the 20th day after delivery, mention the existence of a single ruptured perineum.

Dr. G. F. ABEGG, of Danzig, performed the operation in all of the ruptures to the number of sixty-eight among 908 births; forty-two healed up completely, twenty-two only partially, four were not cured on account of puerperal ulceration of the vagina. Prof. SCHROEDER reports the success of the operation in eighty cases. Of these, twenty-seven healed up entirely by first intention, seventeen others were perfectly united, but granulated on the surface, fifteen were united in the deeper layers of the perineum, in fifteen the operation was unsuccessful, the result in six cases not known. He ascribes these not very favorable results to the fact that the operations were all performed by students, who did them neither with sufficient experience nor accuracy.

From the report of the Lying-in Hospital at St. Petersburg, published by Dr. E. BIDDER and Dr. W. SURIGIN, it appears that 126 ruptures were united by sutures, which occurred after 1998 deliveries. Among these sixty healed by first intention, thirty-four partially, twenty-nine were not cured, the result in three remained unknown. The unsatisfactory result is also attributed to the fact, that the operations were performed mostly by inexperienced young physicians.

The best success was obtained by PROF. VON HOLST, of Dorpat, Russia, who claims to have closed up by first intention every single case in which he performed the operation. The same good results are claimed by PROF. B. SCHULTZE, who succeeded in uniting all of the thirty deep ruptures, upon which he operated immediately after confinement, with the exception of two, where the process of healing was interfered with by puerperal ulcerations in the vagina.

From a calculation of the results of these various statistics it would appear that complete success is obtained in about seventy-five out of every 100 cases.

Now, if we recommend the immediate operation, this does not exclude the admission of contra-indications. In my opinion the only one that ought to be considered as such is a low degree of vitality of the patient. If she be reduced by hemorrhage, by a very protracted confinement, or other conditions complicating the act of labor, it would be unreasonable to insist upon immediate interference. The extent of the rupture itself should never be counted as an indication for delaying the operation. The reasons that call for an operation in a perineum ruptured up to the sphincter ani, gain in value in proportion to the extent of the rupture. It is a well-known fact that the causes of rupture must be sought more in the condition of the tissues forming the perineum, than in the duration of labor; by which I mean to indicate that a very extensive rupture far up into the vagina by no means coincides in the majority of cases with a very low amount of vitality.

Another contra-indication, which has been urged by several obstetricians, is the danger of puerperal fever spreading from the canal of the sutures, especially at the time of their removal. If this were really the case, we ought to be especially careful in New York, where we see so much of the fever almost every year. I have looked over the several works at my disposal to find an answer to this question, and I found it in the above-named report of the St. Petersburg Lying-in Hospital of Dr. Sutigin.

Of sixteen cases of rupture, where no operation was performed, 6 per cent. remained free from puerperal fever, while among twenty-six cases which were operated 14 per cent. remained healthy—and while 12 per cent. died from it among the former, only 7 per cent. had a fatal issue among the latter.

Thus patients in whom a perineal rupture is united immediately after the accident, stand the best chance in an epidemic of childbed fever. And it is reasonable to expect that the closing up of the torn edges should diminish the surface from which septic deposits may be absorbed or even developed. Hence the prevalence of puerperal fever would rather establish an indication for immediate operation than otherwise

Now after all these considerations I think I will not be expected to dilate on other minor objections that have been urged against immediate interference; the fact, for instance, that the accoucheur was usually on such an occasion not provided with the necessary surgical apparatus, or that he was not skilled enough to perform the operation. In the first instance he has to send for his pocket-case, and in the second, for a more competent surgeon.

With regard to my own experience I must say that when I began to practice in this city I was imbued with the doctrines which were taught seventy years ago, that it was useless to attempt union by an operation immediately after the accident. Of late years, however, I have operated upon every case where the general condition of the patient was such as to present no contra-indication against the use, or rather continuance of chloroform. I have made it a rule to give chloroform when the head is impinging upon the perineum in every case where I expect a rupture to take place, in order to have the operation performed before the patient is allowed to become conscious, or to spare her the pain of the lateral incisions, in case I should decide to employ them. Since I have no statistics to offer, I will state from memory, that in most instances the result was satisfactory, and that in those cases where union did not take place, this was owing, in one set of cases, to a want of reaction in the tissues involved, and on the other hand, the result of an insufficient number of sutures.

I will briefly state that, except the paring of the edges, the method of operating is the same as that employed in ruptures of old standing.

The last operation which I performed is an example of the faultiness of the recommendation to keep the bowels confined as long as possible. The patient was delivered on the 20th of September, 1874, after an easy labor of six hours. A rupture, which had existed from the last confinement, and involved even part of the sphincter ani muscle, was enlarged by this second labor so as to tear through the sphincter, and to some extent into the vagina. She was put at once under the influence of chloroform, and the operation performed in the ordinary manner, six sutures having been applied. I kept the bowels confined, because the patient had been subject to chronic diarrhœa for many months after the last labor. On the 26th I removed a few of the vaginal and perineal sutures; the parts had united completely. In the night of the 28th the patient had a spontaneous evacuation of the size of a large fist, and when I arrived to examine the parts, I found that the rupture was as bad as on the first day of the accident. By great care and the use of

irrigations, repeated every three hours, the wound in the rectum healed by granulation sufficiently to keep the patient perfectly comfortable; and up to the present time she has had very little inconvenience from the almost complete absence of the perineum.

DR. PAUL F. MUNDÉ read a

REPORT OF THE NUMBER OF CASES OF PERINEAL RUPTURE WHICH OCCURRED IN THE WÜRZBURG LYING-IN HOSPITAL FROM 1863 TO 1870, THE MANNER AND RESULT OF THEIR TREATMENT, AND THE MEANS USED FOR THEIR PREVENTION.

Among 2,560 deliveries, about 1,200 of which occurred under Dr. Mundé's personal supervision, or while he was resident physician to the hospital, there were 44 lacerations of the perineum, that is one in 58 cases = 1.72 per cent. Of these lacerations 38 occurred in primiparæ, 6 in multiparæ, 43 in vertex, one in a breech presentation. This last case was in a primipara with very narrow vulva and long perineum, in whom the breech, which had been stationary at the pelvic outlet for 24 hours, was manually extracted; notwithstanding bilateral incision of the vulvar border (episiotomy) the perineum ruptured in  $\lambda$  shape down to and on either side of the sphincter, without injuring that muscle; five sutures were applied and complete union ensued. Two of the 44 lacerations occurred in forceps cases. Of these 44 lacerations 34 were treated by immediate union with the interrupted suture, well-waxed double thread or silk being used. In one case one suture was applied, nine times, two; seven times, three; six times, four, and three times, five sutures; in eight cases the number of sutures is not noted. The legs of the patients were kept constantly tied together, they were directed to occupy the lateral decubitus as much as possible, the bowels were kept confined until the day of removal of the stitches, generally the fourth or fifth day, and then opened by copious enemata. The urine was for a time drawn twice a day, but finding that the operation of catheterization necessitated more manipulation of the parts than seemed advisable, fearing also the danger of causing cystitis by the introduction of lochial fluid into the bladder by means of the catheter, especially as a sufficient separation of the labia was inexpedient on account of the laceration, Dr. Mundé preferred to allow the patients to urinate freely over the perineum, irrigating the parts immediately afterwards with warm water, and found this method much more satisfactory. In 10 cases no treatment was adopted, either because the lacerations were too slight in extent to require it, or because a syphilitic diathe-



sis and the presence of broad condylomata on the perineum rendered a union by first intention improbable. In 11 of these 34 cases there was complete, in 7 only partial union: 18 out of 34 = 55.5 per cent., about the usual average in hospital practice.

Among 448 confinement cases of which Dr. Mundé has accurate notes, there are mentioned only nine instances of episiotomy. He is confident that this operation was performed much more frequently, probably 40 or 50 times, but was considered too unimportant to require notice except in special cases. It was customary during the time of service of Dr. Mundé's predecessor, as well as during his own, to keep the scissors close at hand with every primipara in whom the perineum appeared likely to yield to the strain occurring during the passage of the presenting part; as soon as the distention and pallor of the perineum indicated that that part had reached the point when the slightest increase of distention would result in its rupture, the blunt point of the scissors was inserted from  $\frac{1}{4}$ " to  $\frac{1}{2}$ " under the sharp border of the vulva, about 1" from the perineal raphé, and the skin and subcutaneous cellular tissue divided until the white glistening fibrous fascia appeared. This incision, if necessary, was repeated on the other side of the raphé. There can be no question that these slight incisions saved a number of perineæ; Dr. Mundé had too often seen their value to feel any hesitation in expressing this conviction. The incisions, although occasionally appearing large at the moment, diminished so much in size during puerperal involution as hardly to be noticeable; once only was it found necessary to apply a suture, and once a small branch of the common pudic artery was divided, requiring torsion. Notwithstanding bilateral episiotomy, four lacerations of the perineum occurred, one in cicatricial tissue, the residue of broad condylomata, and three in very high perineæ.

Dr. Noeggerath, in his remarks this evening, recommends very justly the method of preventing perineal laceration by supporting the perineum, and pushing the presenting part towards the symphysis by means of two fingers in the rectum—a method formerly described by Ritgen, and lately revived by Dr. Wm. Goodell, of Philadelphia, and Dr. Frederick Ahlfeld, of Leipzig. Dr. Mundé said that he had no doubt that this was the best and, perhaps, only really efficacious mode of supporting the perineum during labor, but he wished to call attention to two accidents which he had seen happen, and which should be guarded against: 1. The flexible parietal bones of the foetal cranium may be indented by too strong pressure with the two fingers in the rectum, a circumstance which happened to him



in one instance, the slight depression disappearing without subsequent evil consequences in a few days ; and 2. The laceration of the soft spongy cavernous tissue at the anterior commissure of the vulva in consequence of the too strong pressure of the head against the symphysis, and the over-distention of the soft parts by the broad foetal occiput.

He had seen this accident happen in a primipara, in whom the attending midwife supported the perineum in the usual manner by pressing the hand against the part from the outside ; notwithstanding this support a laceration of the perineum occurred, and when Dr. Mundé was called to examine and unite it, he found a profuse hemorrhage taking place, as he first thought, from the lacerated perineum. On carefully sponging off the parts, however, the blood was found to proceed from a deep fissure, nearly 2" in length, through the cavernous venous tissue to the left of the clitoris. Styptics and pressure were applied in vain, and the hemorrhage was not arrested until the whole bleeding surface was taken up *en masse* and ligated, by which time the patient had lost quite a large amount of blood ; she made a good recovery.

This case is reported by Dr. Peter Müller, in *Scanzoni's Beiträge*, vol. vii., p. 201, together with five cases of his own (three described in vol. vi., p. 148), in three of which the patient succumbed to the hemorrhage from the ruptures between the clitoris and the urethra.

In reply to an objection made by Dr. M. D. MANN to the operation of episiotomy, that the incision, besides being very painful, might become the seat of septic infection and diphtheritic deposit, Dr. Mundé said that he had performed the operation very often, never with bad results ; that small abrasions and lacerations of the vaginal entrance occur almost in every labor, from which septic infection might readily take place if there were any disposition to such an accident, and that one or two small incised episiotomy wounds would not increase that disposition ; the pain of the incision is so momentary that it is scarcely felt by the patient during the greater agony of the termination of the second stage of labor. Of course the operation should be performed only when there appears no other chance of saving the perineum ; that it is much to be preferred to a perineal rupture scarcely needs mention.

He had seen one case of *superficial* rupture of the perineum in a woman, the skin of whose perineum had been converted into cicatricial tissue by broad condylomata ; in this instance he distinctly saw the cicatricial integument crack in a longitudinal direction as the head distended the perineum, and this superficial rupture extended to double the extent of the

moderate actual complete rupture. He would ask whether it is justifiable not to treat by immediate suture a perineal laceration in a syphilitic person, or whether it should be united and the chance given it to heal. He thought the latter, for he had in Germany treated several bad sabre wounds of students, who were notoriously syphilitic, by the immediate suture, and had seen them heal by first intention.

DR. NOEGGERATH said that it was a common surgical rule not to operate in syphilitic cases, until the syphilitic taint had been at least apparently removed. This applies to secondary operations. A certain diseased condition of the arterial coats in syphilis (which has thus far, however, been observed only in the cerebral arteries), would appear to prevent union of the wound. In a fresh wound, such as laceration of the perineum, he does not think that we would be justified in refusing the operation merely on account of syphilis, although there is less chance of success. Sigmund, of Vienna, however, asserts that wounds in syphilitic persons heal quite as readily as in healthy people.

DR. BYRNE related a case of laceration 2" up the recto-vaginal septum, which occurred some three months ago during labor. The patient was too much exhausted to permit an immediate operation. The laceration has gradually united spontaneously, and the woman has now almost recovered the use of her sphincter ani.

#### CASE OF REDUCTION OF AN OLD INVERSION.

DR. DAWSON reported a case of inversion of the uterus of two years' standing, which had been produced (so the history given by the patient led one to infer) by traction on the cord by the attending physician. The uterus was reduced by taxis under ether after three attempts at intervals of about a week, the patient in the meanwhile using copious hot vaginal injections to soften the cervical constriction. The hand only was used in the reduction (Dr. White's instrument being tried once unsuccessfully), pressure being made on the left horn of the uterus, which at the last attempt suddenly slipped upwards, and was quickly followed by the cervix and the other horn. The patient made a good recovery. Dr. Dawson said that he had not been able to find any mention of the use of hot vaginal injections in the reduction of old inversion of the uterus.

DR. NOEGGERATH asked Dr. Dawson what was the title of his paper, and being informed that it was "Reduction of an Old Inversion of the Uterus," said that it should read: "Reduction of an Old Inversion of the Uterus after Dr. Noeg-

gerath's Method." He had reduced an old inversion of 13 years' duration by this method in 1858, and described it. It certainly is the most rational plan, first to reduce one horn, and then the other. He was glad to hear that the method has again been successful.

In answer to a question by Dr. Byrne, whether the horn or the cervix is first reinverted, Dr. Noeggerath said that before the horn is inverted, the cervical portion is necessarily pushed up, and distends the cervical ring; the reinverted horn then quickly slips through the distended ring of the cervix.

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STATED MEETING, APRIL 6, 1875. THE PRESIDENT, DR. BYRNE, IN THE CHAIR.

CASE OF INFANTILE PARENCHYMATOUS HEPATITIS.

Dr. M. D. MANN presented the liver of a child 16 months old. The child was brought to him on March 20th last, and was found to be suffering from jaundice. The parents were healthy people, and had a family of healthy children. This child had always been well, except a cold now and then. Two days before it was noticed to be yellow, but seemed quite well. The jaundice continuing, it was brought for treatment. On examination, the child was found to be well nourished and fully developed. The skin throughout the entire body was yellow, as well as the conjunctiva. The liver extended one inch below the ribs, and was not tender on pressure; the bowels were constipated, and the faeces brown in color; temperature in rectum, 99°; ordered a laxative; child returned on 22d in about the same condition; on 24th was worse, jaundice persistent; on 26th much worse, liver tender on pressure; temperature 101°; child had refused food, and vomited at intervals; was drowsy and cried, and was irritable when aroused. March 30th, still worse, stupid, could not be aroused, pulse slow and intermittent, some dulness on percussion over lungs posteriorly and fine râles; œdema of lungs; vomited any food which could be forced down. Since March 24th the faeces had been perfectly white, resembling those of a dog. Died April 1st.

*Autopsy* April 2d. Rigor mortis very slight; body not emaciated; lungs œdematous posteriorly; liver about normal size, of dark dusky hue, vessels of right lobe injected, left lobe yellow and normal in appearance. A cut surface looked very much like a section of a kidney, the outer, or upper portion of the section being dark blue, the central substance bright-yellow. Microscopic examination showed the dark part to be congested, and fatty degeneration to have taken place, the fat being in very small

particles. The yellow part was very fatty, the liver-cells being filled with large drops of fat. The connective tissue seemed to be swollen and more prominent than usual. The gall-bladder contained 3 iij. of thick, almost black bile. The gall-ducts were quite open, and water could easily be forced through into the intestine; a probe could also be passed.

DR. MANN remarked that the only account he could find of any such affection he had discovered in Steiner's work on *Diseases of Children*, where the disease is called "parenchymatous hepatitis with fatty degeneration," and said that it was of very rare occurrence in children. The symptoms and appearances in his case coincided exactly with those reported by Steiner.

DR. NOEGGERATH said that the case was a very interesting one to him, because he had witnessed quite similar symptoms in a lady eighteen years of age during the past week, such as jaundice, fever, coma, all of which gave a decidedly unfavorable prognosis. This patient lived in Fifty-ninth street, near Lexington avenue; Dr. Mann's case, in Thirty-eighth street, between Eighth and Ninth avenues—both localities infested with malaria, of which nature he had no doubt his own case was.

CONTINUATION OF THE DISCUSSION ON THE TREATMENT OF PERINEAL LACERATION.

DR. MANN said that, as the subject of immediate treatment of rupture of the perineum had excited some attention of late in the Society, he would like to present one of the *serre-fines*, the employment of which he had recommended in his article in the *AMERICAN JOURNAL OF OBSTETRICS* for November, 1874. This one came from Vienna. It would be seen that it was not, as Dr. Lente had said in his paper, a *very strong* *serre-fine*, the spring of this one being quite weak.

Dr. Mann did not think that Dr. Noeggerath's figures in his paper as read at the last meeting, were quite fair. He had certainly shown that the result obtained by some form of operation were much better than could be expected where we were merely content with tying the knees together and enjoining rest. But he did not think that the superiority of the suture over the *serre-fines* was so clearly established as Dr. Noeggerath would lead us to believe. Dr. Noeggerath had counted 115 cases reported by Winckel as being all cured, because nothing was said to the contrary. This was hardly fair, however, as the reports of previous years from the same institution had not shown any such results. It could hardly be expected that they would jump from 35 cures in 45 cases to 101 cures in 101 cases

(14 were so slight as not to require suture). Leaving out then these cases, the percentage of cure is reduced to 56 per cent., while by *serre-fines* the cures had been about 38 per cent. Hoogeweg, however, reported 27 cures in 35 cases, or 77 per cent. by the *serre-fines*. It was hardly fair to conclude because one man obtained no success, and another very poor results from the use of the clamps, that it was the fault of the instrument. The fault lay rather in the operator, or how could one get so much better results than another? Perhaps, also, the springs were too stiff, or they were not properly applied. At any rate, the fact remained that 77 per cent. had been cured, and what one man had done, another could do. Dr. Mann hoped that the clamps would be tried by the profession in this country, and that the subject would be discussed until some definite plan was decided on.

Dr. NOEGGERATH said that he had had some hesitation in admitting the 115 cases into his statistics, but had been led to do so from the fact that a very careful report of the condition of the genital organs of each patient was given before she left the hospital, and nothing was said about rupture. He therefore thought they could be safely counted as cured. As to *serre-fines* he thought they occupied the same position in the treatment of rupture of the perineum as they did in general surgery; twenty years ago they were in general use, but were now abandoned, because it was found that they did not unite the deeper tissues, only the skin being held in apposition, while the muscles were allowed to retract and consequently did not unite.

Dr. MANN said that he did not think the cases were parallel. In the perineum after labor the tissues were all relaxed, and if the patient were placed on her side or the knees kept together the entire surface of the wound was brought in contact. That was why we had so many cases of spontaneous union. The only danger was that they would slide or slip one on the other, and that they would gape, and this was prevented by the *serres-fines*.

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STATED MEETING, APRIL 20, 1875. THE PRESIDENT, DR. BYRNE, IN THE CHAIR.

Dr. MANN reported a case of

SARCOMA OF THE VAGINA.

On the 1st of last September (1874) a patient presented herself at my clinic at the New York Dispensary with the follow-



ing history. She was 33 years of age, had been married ten years and had had two children, and three months previously a miscarriage at the third month. The miscarriage was apparently brought on by a hard day's washing. Previous to this her health had been generally good. Flooding followed the abortion, lasting two weeks, and forcing her to keep to her bed.

Since that time she has never been all right, having had a steady burning pain in the back, and for some weeks a sharp shooting intermittent pain in the pelvic region "near the womb." She seemed to be of a very nervous temperament, and was extremely anxious for relief. On examination I found the uterus in position, slightly enlarged, and the vaginal portion nearly if not quite denuded of epithelium. There was a slight tenacious mucous discharge from the cervical canal. The patient came late, so the examination was very hastily made, and she was discharged with directions to use a vaginal injection containing the sulphate of zinc.

She returned in two days, her condition being as before. I noticed at this time a mass lying in the angle formed by the uterus with the vagina at their junction to the left and posteriorly, which mass I took to be mucus acted upon by the zinc. I brushed it away with a pledget of cotton, and noticed that the surface under it bled a little. At the next visit I found the same mass, only it was a trifle larger and not quite so easily removed, and then I recognized it to be a growth from the vaginal mucous membrane. It was about one-third of an inch long, one-eighth high, with the base a little broader. I scraped it away with a tenaculum, and cauterized the surface with nitrate of silver.

Again it returned and again I removed it, and obtained a piece large enough for microscopic examination. It was extremely soft and friable, but I finally obtained some sections which showed it to be a small round cell sarcoma. The tumor, if it can be dignified by that name, was of a yellowish-gray color, sparsely supplied with blood-vessels, and bled but slightly when cut or torn; it was about the consistency of granulation tissue, which it somewhat resembled.

The cells were found to be in a state of fatty degeneration, and from this circumstance I predicted that the growth would soon undergo molecular disintegration and result in an ulcer. I sent her to Dr. Thomas, who sent her to the Roosevelt Hospital. Dr. R. Watts, in whose ward she was, took no active measures and only relieved the pain, which grew constantly worse, by hypodermic injections of morphia. At the end of two months she returned to me. She was then in a very pitiable condition. She had nearly lost her memory, and could not talk

on any subject without wandering. She seemed to suffer intensely from great pain just above the symphysis pubis, which she described as shooting and cutting in character, and as being nearly constant and rendering sleep impossible. She was not relieved by immense doses of morphia and chloral.

An examination showed the tumor to have disappeared, and a large ulcer to have taken its place, forming a depression as large as the end of a finger. This included the whole posterior fornix vaginae and the posterior portion of the cervix; the uterus was anteverted. The ulcerated surface was yellow in color, as though bathed with pus. There was little or no discharge or fetid odor. She then went into the Presbyterian Hospital, where she remained until two weeks ago. The growth made steady progress, and the ulcer included the whole of the vaginal portion of the uterus and a large portion of the surrounding vaginal wall. The mind, which had been steadily failing, seemed to be almost gone; a sort of mania developing itself, rendering it impossible to keep her in a hospital ward. The tendency of her mind was toward lewd and sensuous ideas, as though the constant irritation of the pelvic organs carried the mind in that direction. I am informed that the pain was intermittent in its character, lasting for two or three days, and then nearly disappearing, only to return with renewed violence. When the pain was at its height the delirium was also worse.

Sarcoma affecting the vagina primarily is extremely rare; besides the cases reported by Spiegelberg and the one reported by Dr. Thomas last year, there are, I believe, no cases on record. The case reported by Dr. Thomas differed in several respects. They were both small, round-cell sarcomata, but were unlike in their seat as well as in their clinical history. I have been led to present the case on account of its rarity, believing that all such cases should be reported.

Dr. BYRNE remarked that in the ulcerating stage of sarcoma the microscopical peculiarities of the disease become lost, and that it is next to impossible to distinguish it from the same stage of cancer. He related the case of a lady forty-two years of age, who complained of constant pain in the back and slightly profuse menstruation, but on the whole experienced little distress. On examination, to his surprise he found that the posterior lip of the cervix presented the nodular, enlarged appearance of carcinoma. He advised her to come to the hospital and be operated upon. When he attempted to scrape away the diseased tissue with the curette, he was astonished to find it hard, fibrous, not removable by the scoop. Its color was grayish. He removed a small piece for microscopic examina-

tion, and found it to present all the elements of sarcoma. His impression is that this variety, fibro-sarcoma, is more common in the uterus than in other organs.

#### CASE OF SPONTANEOUS AMPUTATION IN UTERO.

DR. DAWSON exhibited the hand of a new-born infant, the two phalanges of the third finger of which were wanting. The stump was broad like an articular surface; there was no cicatricial tissue in the skin of the stump. The question was, whether this is a specimen of intra-uterine or spontaneous amputation, or of arrest of development. The probability was strongly in favor of the former anomaly.

DR. MUNDÉ said that in 1868 he had seen a case of partial spontaneous amputation of the metacarpus and first phalanx of the second and third fingers, in which the cause of the amputation was an amniotic band, as proved by the persistence of a portion of the constricting band in the palm of the hand, in a line directly continuous with the furrow on the sides and back of the hand. He had at that time studied up and written an article on the subject, which was published in the *Boston Med. and Surg. Jour.* for July 8, 1869. From what he had then seen and read, he had no doubt that this specimen was one of spontaneous amputation, even though no cicatrix was apparent on the stump; for if the malformation were due to arrest of development, the stump would not be broad at the end, but tapering like a finger, and (as was also suggested by Dr. Byrne) would be likely to show some sign of a nail. Up to 1870 there were only 140 well-authenticated cases of intra-uterine amputation reported. The large majority of cases described as such are merely cases of arrested development. (See paper by Dr. Peter Reuss, *Scanzoni's Beiträge*, vol. vi., 1870.)

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## TRANSACTIONS OF THE PHILADELPHIA OBSTETRICAL SOCIETY.

Reported by J. V. INGHAM, M.D., Secretary.

STATED MEETING, FEB. 4, 1875.

DR. WM. GOODELL exhibited a large fatty tumor of the labium, weighing one and a half pounds, recently removed by himself.

The President, DR. ALBERT H. SMITH, delivered the annual address to the Society.

### ANENCEPHALOUS FŒTUS.

DR. PARISH presented for Dr. J. W. Barr an anencephalous fœtus with the following history:

The father, æt. 45, was in the last stages of consumption.

The mother, with six previous children and all healthy, is also feeble, but not distinctly diseased.

When labor came on she firmly believed that she had reached the end of the tenth month of her pregnancy. While carrying the child she, from over-work and watching, became quite feeble. Her abdomen was unusually large.

As the membranes gave way during labor, an unusual quantity of liquor amnii escaped, followed for a short time by disappearance of labor pains.

The defective portion of the child's head was presenting, the Doctor recognizing a pulpy mass separating the two ears. There was no delay with the passage of the head after engaging, and with a little assistance the shoulders were easily delivered. The remnant of the head was almost black from congestion. The heart beat for a number of pulsations after birth, but no attempt at respiration was observed.

DR. GOODELL asked whether there was any movement by the child when the head was touched. This is said to be a diagnostic point of this malformation, pressure upon the medulla oblongata producing reflex action. He referred to a suggestion made on a previous occasion by Dr. Harris, that the excessive vesical disturbance, which is sometimes found in these cases, is due to the softened condition of the child's head, allowing it

to descend into the pelvis. There is also generally a large amount of liquor amnii in these cases. A German writer also maintains that the anencephalous condition of the foetus is due to a previous condition of hydrocephalus.

DR. HARRIS suggested as another point of diagnosis that the ears of the child before birth seem to be hard and cartilaginous, and this, too, not as the effect of pressure, for it occurs when the child is high up.

DR. LUDLOW had seen three cases, all similar to the one shown. The apparent prominence of the eyeballs was due to want of development of the ridges of the frontal bone.

The PRESIDENT asked whether, at the time of the fright referred to in the history of the case, there was any hemorrhage. In the only case he had seen, the patient had had at six and a half months a hemorrhage from placenta prævia. She fell into labor at term, the presenting placenta was easily detached, and a child of six and a half months' development was delivered. There was, however, another child of about eight and a half months' development delivered promptly and well. In this case the nutrition of the first child was affected by the hemorrhage.

DR. PARISH replied that there was no history of any hemorrhage.

DR. KELLY stated that in two cases which he had seen there was in one a history of intemperance, in the other one of syphilis.

On motion, the foetus was referred to a committee for careful examination to determine the exact condition.

This committee subsequently reported as follows:

"Your committee report that they found the anencephalous foetus, referred to them for examination, to be remarkably well developed in all its parts with such exceptions as are mentioned below.

"The general shape of the head presented an appearance suggestive of that of the frog. The soft tissues of the face were flabby; the eyeballs protruded forward; the auricles of the ears were large, pendulous, and pressed forward. The bony structures and the soft parts of the cranial vault were absent. In their stead was a flattened area of about three inches in diameter, presenting a 'raw' appearance, and covered with a thin, delicate membrane. This membrane was continuous with the integument around, but altogether different from it in appearance, and not covered with any hairy growth, as was the skin surrounding this area. On the removal of this membrane was found a flattened layer of tissue resembling connective tissue, containing fibrinous remains of clotted blood. No brain tissue



was recognized above the medulla, which presented as an eminence in the posterior half of the abnormal area.

"In our examination of the bones of the cranium, we found the basilar and condyloid portions of the occipital bone present, while the occipital portion of that bone was entirely absent, so that the foramen magnum was encircled with bone excepting at its posterior portion, where fibrous structure completed the rim.

"No portions of the parietal bones were discoverable.

"The horizontal portion of the frontal bone was present and apparently normal, the vertical portion was deficient above the line where later in life is seen the superciliary ridge.

"The squamous portions of the temporal bones were absent.

"The examination of the sphenoid bone was less satisfactory, but it seemed normal, save that the greater wings were undeveloped excepting in a narrow line contiguous to the body of that bone.

"The ethmoid seemed normal.

"A more thorough examination was not made for fear of rendering the specimen unfit for preservation.

"W. H. PARISH, }  
 "WM. GOODELL, } *Committee.*  
 "JOS. V. KELLY. }

#### FIBROID POLYPUS OF THE UTERUS.

DR. ELLWOOD WILSON presented a large tumor which he had removed from the uterus of a single lady. Three years ago she was troubled with excessive hemorrhage and pain at her menstrual periods. For this he made an examination, and found the uterus not unusually large and the vagina small, so as to require a small speculum. By the use of injections the hemorrhage ceased. She passed from under his care for eighteen months, being out of the city. On January 7th, he saw her again, and making an examination, found the vagina completely occupied with a tumor extending up into the uterus. On January 10th, he removed the tumor, which was attached to the posterior surface of the cavity of the uterus, a little to the right of the median line.

The tumor was  $2\frac{1}{2}$  inches long, its area of attachment  $3\frac{1}{4}$  by  $2\frac{1}{2}$  inches, and its weight 23 ounces.

He first drew the mass down with great tension, and then removed it with a curved knife which he has devised for this purpose. He introduced the knife, cutting first upon one side, then upon the other, giving the mass a rocking motion as he

cut, and then making torsion. The tumor was so large that the perineum was ruptured in its extraction.

This specimen was referred to a committee for microscopic examination, who subsequently reported as follows:

"The committee appointed to examine the 'Polypus of the Uterus' exhibited by Dr. Ellwood Wilson would respectfully report that this specimen is a *cystic myoma*, of the class known also as *myomatous or fibroid polypi*. The latter term is strictly appropriate if the word *polypus* is used to indicate tumors which grow within the uterus and present a more or less pendulous form.

"The minute structure consists of the usual unstriped muscular fibres and vascular tissue, which are rendered easy of dissection after maceration in a dilute (20 %) solution of nitric or other acid. The spaces or cavities presented by the growth may be looked upon as simple separations or distended fissures between the muscular fibre-cells, being entirely without epithelial lining, notwithstanding their smoothness."

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STATED MEETING, MARCH 4, 1875. DR. A. H. SMITH, PRESIDENT,  
IN THE CHAIR.

#### ABNORMAL FORMATION OF THE PLACENTAL TISSUES.

DR. CLEEMANN gave a description of a case of placenta succenturiata. After the delivery of a primiparous patient, he noticed that there was difficulty with the placenta, the membranes not slipping out with the usual manipulation. First there was a resistance, which was overcome by degrees, and there appeared a placental mass of the size of the palm of the hand, which was distinct, not bearing any sign of having been torn from the placenta proper. He thought it was a case of abnormal development of some of the villi of the chorion. This case shows the insufficiency of the examination of the placenta alone to show us whether the uterus is empty or not. It may also account for some cases of post-partum hemorrhage.

DR. GOODELL said he had never seen a case of the kind, but in this connection would like to know the views of the Society in reference to the time-honored custom of twisting the membranes in effecting their delivery.

He had given up the plan on account of the liability of a small portion of a delicate membrane being torn off in utero by the twisting.

DR. A. H. SMITH remarked that he had been in the habit of twisting the membranes gently into a thick cord, and then

making easy traction in the line of the axis of the uterus. In two cases in which the uterus had contracted upon the membranes, which had been torn from the placenta, he succeeded in extracting them by passing a soft napkin into the vagina to enable him to grasp them and then twisting them.

DRS. CLEEMANN, YARROW, and TAYLOR also expressed similar views.

DR. GOODELL asked whether any of the members of the Society who had served in the wards of Blockley Hospital had seen cases of delirium tremens occurring after labor.

DR. WILLARD replied that, as the majority of the Blockley patients come into the house several weeks before confinement, there was no opportunity of testing the question.

DR. GOODELL replied that since an injury, a slight wound, or a fracture even, will sometimes develop delirium tremens several days afterward, so in confinement, if a large surface be exposed by the denudation of the placenta, the same result might be expected to happen.

DR. KEATING cited the case of a woman who was brought into Blockley drunk and in labor, but no delirium followed.

DR. CLEEMANN suggested that as in labor the membrana decidua uterina covered the area of attachment of the placenta, there was no raw surface exposed. The case was not analogous to that of a wounded surface.

DR. GOODELL replied that it was still a moot point as to whether there was a raw surface left after the extraction of the placenta.

#### CASE OF TRISMUS NASCENTIUM.

DR. J. V. INGHAM desired to call the attention of the Society to the use of nitrite of amyl in cases of trismus, to control the spasms. He had recently in his own practice witnessed a well-marked instance of its powers in this respect. On the 29th of January, 1875, he had delivered a woman of a fine healthy female child weighing ten pounds. The cord dropped off on the sixth day, leaving a smooth non-ulcerated surface. The child slept well, nursed well, and seemed as well as possible for two days afterwards, when the nurse noticed a slight convergent strabismus, a pursing of the lips, and some spasmodic contractions of the facial muscles. The symptoms, however, disappeared in a few minutes, and the child nursed freely. The following day (the ninth after the birth), another spasm occurred, more marked than the first, and accompanied with some rigidity of the neck. An examination of the umbilicus now for a probable cause revealed an ulcerated surface partially covered with a grayish slough. This was at once touched with

a stick of nitrate of silver and covered with a poultice. There was no return of the spasm until three days later (twelfth after birth), by which time the navel had almost healed. The spasm, however, was far more severe than either of the preceding ones, the eyes were markedly convergent, the muscles of the face, neck, and finally the arms were rigidly contracted. The doctor, in obedience to positive instructions, was sent for, and arrived just in time to witness the premonitory symptoms of another spasm. Pouring a few drops of nitrite of amyl on a handkerchief, he applied it to the child's face, and after two inhalations the muscles relaxed, the eyes straightened, and the spasm was aborted. During the same night another spasm was threatened (by the commencing strabismus), but was likewise aborted by the prompt administration of the amyl. The child was then carefully watched night and day by competent nurses, instructed to administer the amyl whenever a spasm threatened to occur.

As there were great restlessness and sensitiveness to objective noises and sensations, the hydrate of chloral was ordered in gr. ij. doses every two hours until this nervous irritability was controlled. After the third dose this effect was partially accomplished.

For four days no marked change occurred, a spasm threatening every afternoon about four o'clock, but was always promptly aborted by the amyl. The child nursed fairly well, but was evidently rapidly losing flesh, and on the fifth day (seventeenth after birth), a pyæmic infiltration of the back of the right hand and the left big toe occurred in the night. Iron and quinine were administered, but without avail, for the spasms returning more and more frequently, although in each case controlled by the amyl, the child died on the twenty-first day after its birth.

The doctor then stated that although the nitrite of amyl could not save the life of the child in this case, it had saved the feelings of the family from the constant exhibition of that most horrible sight, especially to the mother, of a child in trismic spasms. On this account alone he thought that it was a very valuable remedy, and deserved a more extended trial in all cases of either trismic or tetanic spasms.

DR. GOODELL said that he had never seen but one case of trismus. At that time he was led to conclude that trismus in the infant was as much a zymotic disease as puerperal fever was in the woman. In the Rotunda in Dublin there was a fabulous number of children lost until Joseph Clark brought about changes in the ward management, with the effect of reducing the number. Various causes have been assigned for the origin of trismus. In the Bermudas it is attributed to

the cutting of the cord with a rusty pair of scissors. Putridity of the cord and want of cleanliness have been suggested. He believed in the pyæmic origin of some cases. He had already given to the Society his views of the management of the cord. After cutting it he strips it of its gelatinous contents before ligation. He does not have any putrid or offensive navels.

DR. INGHAM replied that in this case there was no suspicion of putridity of the cord, and it dropped off without ulceration.

He alluded to a local epidemic of trismus on a Southern plantation among the slaves, in which the subject had been investigated thoroughly without discovering any cause. Ventilation and cleanliness were good, and the epidemic disappeared suddenly.

DR. HORACE WILLIAMS detailed a case in which symptoms similar to that of Dr. Ingham's case set in twelve hours after delivery. These were controlled by chloral hydrate.

He had attributed these symptoms, however, to pressure upon the child's head during delivery.

The woman was a primipara of forty-three years of age. There was rigidity of the perinæum, and the delivery was effected by the forceps. There was nothing wrong about the navel.

DR. GOODELL remarked that as he understood trismus nascentium, it occurs about the time the cord drops off; viz., in about five or six days. He referred to the views of Dr. Sims, giving depression of the occipital bone as a cause.

DR. INGHAM remarked that there was no depression of the occipital bone in this case.

DR. CHESTON said that in the only case he had seen there was marked depression of the occipital bone.

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STATED MEETING, APRIL 1, 1875. DR. A. H. SMITH, PRESIDENT, IN THE CHAIR.

DR. ELLWOOD WILSON exhibited a set of curved knives which he had had made for the removal of intra-uterine polypi. They were blunt-pointed, curved, and had a lateral cutting edge of about one inch in length. The cutting edge is on the right side in one knife, and on the left in the other, to admit of cutting on both sides of the base of a tumor.

DR. GOODELL remarked that a great deal of unnecessary fear existed as to the danger involved in the cutting away of polypi of the uterus. There is cause for fear in those small polypi which dangle from the cervix, which are perhaps more properly enlargements of the intra-cervical glands. In the large polypi, which are more properly fibrous in nature, there is very little



danger of hemorrhage. He thought that these instruments of Dr. Wilson's were valuable, and he should not hesitate to use them. He was in the habit of using the scissors.

DR. DE FOREST WILLARD presented a specimen of uterine tumor with the following clinical history: The patient suffered for two years with great pain in the back and lumbar regions, with a constant discharge, so offensive in character as to make her presence unendurable to others. There was also that peculiar group of symptoms constituting what has been called the cancerous cachexia. The case had been given to him by the attending physician as one of cancer of the uterus. He was inclined to doubt this from the fact that the large diseased mass involved only the body of the uterus. The cervix and os uteri were entirely free from disease. It is rare to see so large a cancerous mass without the involvement of the cervix. He had not yet examined the specimen microscopically to verify his belief.

DR. GOODELL remarked that so rare was cancer of the body of the uterus, that he doubted the cancerous nature of this mass, but would prefer to await the result of the microscopic examination. It looked more like a sarcoma, that rare disease which deceives the physician into the belief that he is dealing with a polypus, and in which there seems to be a pedicle constricted by the os. These tumors are friable and project into the vagina.

DR. WILLARD said that he had been struck with the fact that if the nodules of the mass were prolonged, they might readily be mistaken for polypi.

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STATED MEETING, THURSDAY, MAY 6, 1875, DR. A. H. SMITH, PRESIDENT,  
IN THE CHAIR.

#### MUMMIFIED FŒTUS.

DR. MCCALL presented a specimen of a mummified fœtus with the following history:

The Society will perhaps remember that some months since (see *Transactions of the Society* for February 6, 1873) I presented for its consideration a case of blighted ovum, which had been retained in utero in a mummified state for a period of about six months. I now beg to offer a fellow to that case in the present specimen, which, although in the main attended by similar phenomena, still presents one or two points of marked difference and questions of interest.

The history of the case in brief is as follows: Mrs. —, aged twenty-four years, married five years, rather stout, though not plethoric, moving in affluence, and enjoying always the most excellent health, became pregnant with her second child

about the middle of July, 1874 (catamenia ceased on the 16th). About the middle of September following (two months), her only child, a fine boy of three years, sickened with diphtheria and died after an illness of but a few days. The shock and distress to the devoted mother was naturally sudden and excessive and her grief protracted. Prior to this event all the symptoms of pregnancy were present. After it they gradually disappeared, and until Christmas time, when she should have "quickened," she would "feel faint" upon the slightest exertion, particularly in the erect position, once or twice losing all consciousness. Three weeks after the death of her child a yellowish watery discharge came on per vaginam, which, though not profuse, was very offensive. This lasted for over five months. Her "morning sickness" lasted till the sixth month, after which she enjoyed exuberant good health, which has continued, with exception of the confinement attendant upon her after delivery, till the present day. Not having felt the motion of the *foetus* at the usual time, I was consulted and advised an examination to determine pregnancy, but having felt, a day or two after, what she supposed faint motion (probably that of intestinal gas) the mother declined an examination, and allowed the case to progress until within two weeks of her expected confinement. Being so fleshy, the natural alteration in her person of advanced pregnancy was not noticed as promptly as it otherwise would have been. At this period, however, upon consulting me for some slight ailment, my attention was called to this fact, and answers to my inquiries induced me at once to demand an examination. This was made by Dr. Getchell, of this city, and myself, and the fact became at once patent that either she was not pregnant at all or had become so very lately, as the womb was but slightly enlarged. In consultation the true cause of difficulty was surmised, and she was placed on moderate doses of ergot, twenty-four hours after which uterine contraction commenced and continued for twelve hours, when the accompanying specimen was discharged, followed a couple of hours later by the placenta. Some hemorrhage occurred subsequently, which was readily checked, and she made a rapid recovery.

The *foetus*, seemingly of about three and a half months, presents nothing worthy of note beyond the characteristics usually attending these cases. The placenta, however, is found to be much larger than the apparent development of the child will warrant, and is evidently fatty and calcareous. A microscopic examination of it detects nothing more.

While it is true that some prominent gynecologists maintain that the retention in utero for more or less time of the dead

foetus is the rule rather than the exception (in the early stages of pregnancy, at least), its expulsion being more dependent upon the vitality of the placenta than itself, still these cases of protracted uterine toleration are sufficiently uncommon to claim our interest and investigation. Hohl, Credé, and Velpeau attribute the phenomena attending them principally to some disturbance of the circulatory apparatus, from which the child derives its nourishment, gradually producing impairment of development and slow death; in the majority of their cases the cord was at fault. In the present case the cord seems normal, and did not, as in my former specimen, encircle the neck, a condition fraught with so much danger in the early stages of pregnancy. Were there any signs of placental apoplexy with hemorrhage, naturally to be expected after violent mental shock, the cause of death would be apparent. The fatty condition of the placenta will, I think, hardly account for it, since it is not so completely changed in structure as is often seen after healthy births, nor is it certain that this state existed prior to foetal death.

It seems probable to me that in this case life was lost from maternal impression in the first place, and the placenta retaining its vitality became gradually metamorphosed, assuming its fatty state, without putrefaction, later, ultimately dying in its turn, its death changes being almost imperceptible to the uterus, which, becoming thus insensibly accustomed to the presence of the foreign body, just as it occasionally does to a slow growing polypus or tumor, tolerated it until a slight interference on our part induced its expulsion.

Another point of interest worth regarding lies in the fact that in all the recorded cases that have met my attention, with the exception of one, mummification with protracted retention has always been in twin-conceptions, nature seemingly holding in abeyance her usual rules for the benefit of the still living occupant of the womb.

DR. GOODELL remarked that an interesting medico-legal question might arise under these circumstances.

A husband might be away from home for several months, when the birth of a child of such small size would give rise to very ugly suspicions.

DR. MCCALL said that in this case the pregnancy was so ardently longed for that, as soon as it was suspected, he was sent for to verify the diagnosis. It was, in fact, a red letter day with the people, and the fact caused the time to be more firmly marked in his mind. He thought the case was undoubtedly one of nine months' pregnancy lacking two weeks.

DR. J. L. LUDLOW thought with Dr. Goodell that the case

was of great medico-legal value, and should be carefully recorded.

#### HEMORRHAGIC LABORS.

DR. ELLWOOD WILSON reported the following cases:

CASE I.—Mrs. W., aged —; third pregnancy; estimated time for confinement, April 5, 1869. On the morning of April 11, about 11 o'clock, while watering her flowers with a quart watering-pot, she slipped and had a sharp pain in her side; was aroused from sleep at 4 P.M. and found herself flooding; this continued moderately through the afternoon. The nurse assured me that at 7 P.M. she was not pale, nor did she complain of exhaustion. I saw her at eight o'clock: the touch revealed the vagina filled with clots; a large amount of blood had passed. The os would barely admit the point of the finger; it was fairly soft and yielding. I passed the finger within the os, and found the clots to pass from between the membranes and the womb. They were forced out with each slight pain; there was no tension upon the bag of waters. The child presented by the vertex, but was so high that I could not detect the position. Ruptured the membranes and gave fl. 3 i. doses of wine of ergot every fifteen minutes till six doses were taken. A large amount of liquor amnii passed; it was not discolored. The womb immediately condensed and the bleeding very much diminished. Ergot was continued at half-hour intervals for six doses, when labor-pains were excited; but these pains were not decided. The bleeding ceased externally. Touched at 5 A.M. Os slightly opened, and the pains set in with bearing down efforts, and recurred every two or three minutes; the uterine tumor remained hard during the intervals of the pain, showing the patient was ergotized. The labor now progressed rapidly, and 8.26 A.M. the child was born. It was a male, and was pale, livid, relaxed, with no perceptible pulsation at the heart; under the usual treatment it soon reacted. A very large amount of coagula escaped with the placenta. More than one-half of the uterine disk of the placental surface was occupied by a coagulum.

CASE II.—Mrs. J., aged —; third pregnancy; ceased to menstruate June 23, 1872; was expected to fall in labor March 28, 1875. In the evening of March 23 she lifted her little boy, about two years old, upon her lap to kiss him "good-night." She felt a slight blow against her stomach, but gave it but little consideration; feeling drowsy, she lay down upon the sofa in her parlor and fell asleep, and did not awake until 10 o'clock,

when she found herself wet and a warm fluid passing from the vagina. She was greatly startled upon discovering that it was blood, and that her clothes were saturated with it. She immediately went to her bed-chamber, feeling faint, and, as she said, flooded dreadfully. I saw her about 11½ p.m. She was pale and much exhausted; her clothing and bed were saturated with blood, which was passing from the vagina. There were no uterine pains; the os was not opened, but the condition of the os and neck showed that she was near her term. I passed the finger through the os and ruptured the membranes; the liquor amnii passed out slowly, mixed with blood. Placed her in a horizontal position, elevating the hips, and gave wine of ergot fl. 3 ij. every fifteen minutes for four doses, which with the escape of the waters brought on uterine condensation, and the bleeding ceased externally. The wine of ergot was given in fl. 3 i. doses from time to time, so as to maintain sufficient uterine contraction to arrest the bleeding. At 2 a.m. of the 24th labor-pains were fairly established; previous to this time the os had not dilated, but it now began rapidly to do so, and the head engaged in the excavation. At 3.45 a.m. the child was born: a female, rather under the usual size. A large amount of coagula passed from the vagina after the birth of the child. I thought as much as two-thirds of the uterine surface of the placental disk was occupied by a coagulum. I carefully watched this case from the time I saw her, intending to make version if the bleeding did not cease, as soon as the condition of the os would admit of it; and, although the ergot and escape of the waters caused a very decided condensation and hardening of the uterine tumor, and arrested the bleeding externally, causing a fair reaction with the patient, there were no well-marked labor-pains until near two o'clock, but when they were established they were frequent and strong, and the delivery occurred without further interference.

CASE III.—Mrs. B., aged —; third pregnancy; ceased to menstruate April 20, 1874; her estimated time for labor was January 25, 1875. She was absolutely well throughout her pregnancy. January 23, at 5 p.m., sent me a note saying she was not very well. I saw her at 5¼; she had some slight pains at intervals of from ten to fifteen minutes; thought they were of but little account; there was no constitutional disturbance. As I desired to make a distant visit, and the nurse was with her, I requested permission to make the touch, so that I might better estimate how long I could be absent. While in the act of lying upon the side of the bed she called to me saying, "The waters have broken." In making the touch, I



felt the warm fluid gushing from the vagina, but as the os was not opened I merely ascertained that the head was presenting, and did not discover she was bleeding until I removed my hand. I directed she should be placed upon the bed as soon as possible; before this could be done she lost a large amount of blood, several quite large coagula falling upon the carpet, and she felt faint. I immediately ruptured the membranes, when a large quantity of liquor amnii escaped, mixed with blood. I gave wine of ergot, fl. 3 ij., and repeated the dose in fifteen minutes. This brought on contractions. The os opened nicely, the bleeding ceased externally, but she still felt faint, and although the os was soft and well-opened, and the head fully engaged in the excavation, she appeared without the power to bear down. The forceps were at once applied, and the child delivered without difficulty; it was a female, still-born. Several large coagula passed from the vagina, following the birth of the child,—I suppose as much as a pint and a half; fully one-half of the uterine surface of the placental disk was occupied by a coagulum. Child presented vertex first; was born at 6 P.M., forty-five minutes from the time I first saw her, and about three hours from the commencement of the first recognized pains. I could not account for this accident, except it was caused by the change of position. The amount of blood lost was estimated at fully two quarts.

DR. LUDLOW asked whether any of these cases had shown any predisposition to hemorrhage.

DR. WILSON replied that they had not.

In answer to several questions, Dr. Wilson further stated that he preferred using the wine of ergot because it was less nauseating to the patient.

He had used Squibbs' fluid extract of ergot, but did not consider it better than the well-prepared wine.

In cases of placenta prævia, he dilates the os, detaches the thinnest portion of the placenta, carries his hand between it and the uterus, passes it up, and delivers the child by version.

DR. GOODELL asked what measures he adopted in cases where the os is undilatable.

DR. WILSON replied that in all cases he used ergot. He never failed to find the os uteri dilatable. He had seen twenty-nine cases of placenta prævia, partial and complete, with but one death. The woman who died had been delivered safely, and was doing well, but was found out of bed and dead the next morning. Her attendant was drunk.

DR. STEWART suggested that in case the uterus was hard and rigid at about the eighth month of gestation, with great hemor-

rhage and no dilatation possible, the ergot would be of no service.

DR. WILSON replied that he has taken his seat and waited hours until the uterus was dilatable, giving ergot all the time.

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STATED MEETING, THURSDAY, JUNE 3, 1875. DR. A. H. SMITH,  
PRESIDENT, IN THE CHAIR.

DR. KELLY read the following history of a

CASE OF POST-MORTEM DELIVERY OF A LIVING CHILD.

I attended Mrs. M. (aged at death forty years) in her sixth labor, March 22, 1871, and in her seventh labor, May 27, 1873. Both labors were unusually rapid, but her last lying-in was a protracted one, consequent on a fright received the third day after delivery. She suffered from pain in the region of the heart, with violent action of that organ, and it was many weeks before she was able to resume her household duties. Her eighth pregnancy was expected to have terminated about the 20th of April, 1875. In the fifth month of this last pregnancy I was called to her and found her threatened with abortion, the symptoms of which passed by under the use of opium and quiet. My attention was also directed to a difficulty of breathing which she experienced, as well as a laryngeal catarrh which had annoyed her some time. On auscultation the respiratory murmur was found to be normal. All over the front of the chest there was heard a soft systolic murmur, with greatest distinctness over the region of the heart, though not localized over any particular valve. Posteriorly there was heard a loud, rough murmur (also systolic), following the region of the spinal column, with greatest intensity in the interscapular space, and faintly heard low down in the lumbar spine. It was also heard over both scapulæ.

A probable diagnosis of thoracic aneurism was made, though there was no aphonia, no evidence of a tumor, nor any hæmoptysis. I saw her subsequently at intervals, and always substantiated the above physical signs, and in addition, at my last visit (made one week previous to her death), I detected dulness over the upper third of the sternum, and on making pressure over the supersternal notch a marked upheaving impulse was communicated to the finger.

At midnight of April 13th, one week previous to her expected confinement, I received a hasty summons to see her, the messenger stating that "she was smothering."

I hastened to her residence, which was about six squares from my own, and on entering her room was quite horrified to find

her dead. She was still warm, but there was no heart-beat or respiratory murmur, and frothy mucus oozed from her mouth and nostrils. On listening over the uterus, I thought I heard a feeble heart-beat of the child, and immediately made a vaginal examination, from which I learned that the vagina was soft and dilatable, that the os admitted two fingers, that the head lay at the superior strait, and that the membranes were intact.

I immediately informed the husband that though there was no doubt as to the death of the mother, there was a possibility of the child being still alive, and expressed my willingness to try to effect delivery, to which he gave consent.

I greased my hand thoroughly and passed it up the vagina, bored through the os, which gave way nicely, ruptured the membranes, and seizing both feet successively, I performed version and extraction, with little more difficulty than I have experienced when executing this manœuvre within the living pelvis. The child was a female of average size and almost still. The heart beat slowly and feebly, but there was no attempt at breathing until one hour's faithful work with artificial respiration had been given it. In another hour, however, respiration was fully established, and the child has since done well.

I have endeavored to compute with as much accuracy as possible the length of time which elapsed between the death of the mother and the delivery of the child, and think that about fifteen (15) minutes would be a fair estimate.

There was no autopsy of the mother.

Dr. E. WILSON referred to a case in his practice in which a woman, who had suffered from hemorrhages of the lungs, was taken with a profuse hemorrhage and died in his presence. The touch revealed the os undilated and the cervix intact.

He asked permission to perform the Cæsarean operation, but was refused.

Dr. GOODELL referred to a paper of Dr. Aveling, in which he states that live children have been born one-half hour after the death of the mother. Out of forty cases, several were delivered later than the one of Dr. Kelly.

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## QUARTERLY REPORT ON OBSTETRICS AND DISEASES OF WOMEN AND CHILDREN.

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CHRONIC HYPERPLASTIC ENDOMETRITIS. By R. OLSHAUSEN. (*Arch. für Gynäk.*, viii., 1.)—Many cases of uterine hæmorrhage are set down as functional, even after a careful examination of the uterus, from the apparent absence of any organic cause. In these cases the flooding is very commonly due, Olshausen believes, to peculiar chronic morbid changes in the mucous membrane. This disease is marked by anatomical and clinical characteristics, which are not to be confounded with those of any other condition. It was described by French authors 30 or 40 years ago, but has been nearly forgotten, and is to-day very little understood or recognized in either England or Germany.

It is generally met with during the child-bearing age, or may be found even later. Occasionally it comes on quite suddenly in previously healthy women, but, as a rule, its advent is gradual, rather as increased menstruation, until it becomes continuous, lasting for months or years together. In bad cases it sometimes stops for months, this being due probably to the great anæmia which has been induced.

When the discharge is continuous it often assumes the character of a bloody serous fluid rather than a true hæmorrhage. Sometimes it is reduced to a very slight, scarcely recognizable, serous discharge, but is otherwise bloody. There is very seldom any pain, the principal suffering being due directly to the effects of the anæmia, such as loss of appetite, weakness, sleeplessness, or even œdema of the lower extremities.

Palpation of the uterus discovers little. The cervix is sometimes soft; the os and lower portions of the canal gaping. The position in all my cases was normal. The volume of the organ was not distinctly increased, though cases have been reported where the uterus was enlarged, and one case where it reached the length of the third month of pregnancy, *i.e.*, 12 cm. The sound causes little pain, but its withdrawal is always followed by hæmorrhage. The uterus is easily dilated by laminaria or sponge tents, and with the production of little pain. On introducing the finger the cervical canal is found normal, the folds of course smoothed out by the pressure.

In some cases we feel nothing abnormal near the os internum, but in others we meet with soft sponge-like masses, which seem to extend down from the uterine walls above to the os internum, even causing this to gap. If, however, nothing is found here, further introduction of the finger fails to show us any distinct lesion, such as polyp-like growth, unless we succeed in reaching the fundus, where we can almost always distinctly feel how much thickened, softened and uneven the mucous membrane is. In particularly favorable cases this can be felt over the whole endometrium. The irregularities are not generally felt on the sides, because they have been smoothed out by the pressure of the laminaria, and this is one reason why the disease has been so long overlooked.

The treatment consists in simply taking off the diseased mucous membrane, and this is best done with Sims' curette. The operation with Récamier's curette is generally and justly condemned by German writers as rough and dangerous. With Sims' instrument, however, the results are most satisfactory. On drawing the curette once or twice lightly over the mucous membrane, we generally get a piece of the diseased tissue, and if a little more force is used, always succeed in getting out large pieces of greater or less thickness. Even if the finger has been unable to discover any unevenness, we are able to feel them distinctly by means of the curette, even in slightly marked cases; and still more distinctly can we feel the softness of the mucous membrane by pressing the curette against the fundus. In many cases after a thorough scraping, we can feel that there is left a uterine wall with numerous very fine but small prominences. The operation is attended by very little bleeding, which ceases after a day or so. The pain is very slight and no anæsthesia necessary. We continue the scraping until no more masses come. Sims' curette is nearly perfect. Récamier's should never be used, and Kinstelle's is only an unfortunate modification of Sims'. *Pathological Anatomy.* The masses taken from the womb show at a glance that we are dealing with a diseased mucous membrane. They are commonly from 2-5 mm. thick and of different sizes, up to that of a finger-nail, all about alike in appearance. They do not show the grain-like flexibility of pieces broken off from a uterine carcinoma, being less yellow and more transparent; but still resemble the surface of a mucous polyp. They do not show any cyst formation, such as we commonly see in large mucous polyps; only once have I met with any formation of a cystic character. Never have I seen anything to lead me to think that the growths are pediculated, erect or independent. The microscopic examination shows a greatly hypertrophied mucous membrane, with increase of all its elements, dilated follicles, enlarged blood-vessels, and great cell infiltration of all the connective tissue. In spots, if we are lucky, we can see the lining epithelium of the uterus unchanged. Running up to the epithelium are large ectatic blood-vessels filled with coagula, and near them great numbers of white blood-corpuscles, quite round and fresh looking; while around the enlarged follicles there are many spindle cells arranged in regular lines. These prove the chronicity of the affection. Cross sections of the glands show them to be round and normal. The dilatation of the follicles differs in degree, but never amounts to a cystic formation visible to the naked eye. There is never any appearance of a decidua-like formation.—O. then presents the histories of nine cases where he applied the curette, in most of them two or three times, but with final good results. He then goes on to say, in brief, that this trouble may be confounded with several others. Polypus growths are distinguished from the fact that they are generally confined to a small part of the endometrium, while this disease is spread over a large surface. In none of the cases was there anything like an expanded formation of mucous polypi, as Beigel has supposed to be possible; again, we never find the cystic dilatation of the follicles, and lastly while the mucous polypi are generally found in the cervix, endometritis polyposa stops with the os internum, leaving the canal free. The affection most strongly resembles the broad-based elevated molluscum of the corpus uteri, as Virchow has described it. The difference consists in the large masses of dilated glands found in the last named affection, which are absent in endomet. polyp. The molluscum occurs very commonly in conjunction with pediculated polypi of the cervix or corpus uteri. The post-mortem appearance is also quite different. We must also distinguish it from sarcoma of the cavity. If the symptoms are not very different, as they generally are, the diagnosis is best made by the microscope, which can



be done in every case. A sceptic might say, that we take out with our curette normal pieces of mucous membrane. This is not the case, for the amount of force used is not sufficient. In one or two cases small bits of normal membrane were removed, and it required a very considerable amount of force.

The reason why the disease has been overlooked is, as has already been stated, the flattening of the masses by the tent and the consequent overlooking them, but that which has most to do with hindering a general reception of the ideas of Récamier, Nélaton and others, is the operative procedure recommended by way of treatment, viz., the scraping off of the mucous membrane.

This has on all sides been described as rough, barbarous and dangerous, but has generally been condemned untried. This comes from the use of Récamier's curette, with which there is some danger of perforating the uterine wall (R. did it himself three times); with Sims' instrument it can scarcely occur, even if the operator be very rough, otherwise there is little danger, inflammatory reaction very seldom following. Light cauterization of the mucous membrane is not sufficient to destroy the disease, and if we use the powerful cauterizing agents, the danger is much greater than in using the curette. Neither does the tent accomplish the object. The disease is very apt to recur, and two or more operations may be necessary to effect a complete cure.<sup>1</sup>

M. D. M.

**PATHOLOGICAL ANTEFLEXION OF THE UTERUS AND PARAMETRITIS POSTERIOR.**—Under this title B. S. SCHULTZE contributes a long and interesting article to the *Archiv für Gyn.*, viii., 1. He first corrects a statement made by Schroeder to the effect, that he (Schultze) considered all antelexions to be normal, and shows that he never has held or published any such views. He then discusses the question as to what the normal position of the uterus is, and compares the different views and opinions which have lately been put forward. He concludes by reasserting his belief previously published, that when the bladder is empty, which it generally is when we examine, the uterus is in a decidedly anteverted, slightly flexed position. If this is so, it becomes much more difficult to distinguish between normal and pathological antelexion. This cannot be done in most cases by simply determining the position of the organ, neither by the symptoms, as they may depend on other and undiscovered causes. To make a correct diagnosis we must determine three points, first, the flexibility of the corpus uteri at its junction with the cervix. This is done by conjoined manipulation, by which we discover whether the flexion is easily reduced.

The second point is to examine when the bladder is full. In a normally placed, normally flexed uterus the antelexion is only strongly marked when the bladder is quite or nearly empty, but in pathological conditions it is persistent, regardless of the state of the bladder.

The third point to be made out, is the condition of the folds of Douglas, the folds of peritoneum which are at the sides of Douglas' cul-de-sac, and which cover the retractores uteri muscles. We have a variety of causes producing antelexion, such as old metritis at the level of the os int., tumors, etc., but by far the most common is rigidity and shortening of the so-called folds of Douglas. When, to test the normal flexibility of the uterine body,

<sup>1</sup> In New York, Thomas' copper wire curette has now quite taken the place of Sims' instrument. It is simply a loop of copper wire without cutting edge; it is safer and quite as effective as Sims' steel instrument. It is commonly used for diagnostic purposes, without previous dilatation of the os; passing it once or twice over the membrane will show whether there are any growths in the uterus.

M. D. M.

we place the finger, introduced by way of the vagina or, better still, the rectum, on the posterior side of the uterus near the point of flexion, and then bend the finger forwards, we feel the great distensibility of these folds which fix the uterus posteriorly at the normal line of flexion.

This stretching, which occurs with the passage of every large fecal mass, is entirely without pain. If, however, in this manœuvre, the finger meets with any resistance, then we know that the distensibility of these folds is diminished. A more careful examination will then show these same folds shortened often unequally, perhaps thickened and painful on motion or any attempt at stretching.

If the shortening of the folds is very great, the cervix takes a position similar to that found in retroversion, only higher and further back. If we succeed in touching the corpus uteri, the bladder being empty, we shall find it lying nearly parallel with the vagina, and the flexion to be very sharp. The mobility of the corpus on the cervix may remain undisturbed for some time; the retroposition of the uterus, however, prevents the filled bladder from replacing it, and it remains in permanent ante flexion.

As soon as we have become acquainted with the details of the condition and the difference between this and normal ante flexion, by examining a number of cases, it is not necessary to touch the folds of D. and the angle of flexion itself in every case (this being accomplished generally by rectal examination alone), in order to recognize the abnormal posterior fixation, which is of necessity followed by the pathological ante flexion. We recognize this fixation by the facts, that the cervix is very high and, perhaps, to one side, by the decreased mobility of the uterus, when pressure is made on the cervix and by the pain which this attempt at movement causes, and thus we distinguish the commonest kind of pathological ante flexion from the normal.

Out of 250 patients in 1874, whose cases were carefully noted, there were seventy-two with ante flexion or version dependent on this posterior fixation. In only two of these cases was the fixation due to peritoneal adhesions. In the other seventy the ante flexion was certainly due to rigidity and shortening of the folds of Douglas.

That the flexion is due to the fixation, is shown by their decreasing, *pari passu*; and in some patients complete relaxation of the folds has been followed by retroversion, and the ante flexion returned and became fixed with recurring posterior fixation.

The cause of this fixation is probably the chronic atrophic parametritis described by Freund, affecting the connective tissue contained in Douglas' fold. Inflammatory hyperplasia of the pelvic connective tissue resulting in cicatricial shrinking, is a form of the process which has not been as yet described by any one but Freund. The history of the cases, the absence of large exudations and of febrile symptoms, the chronicity of the course, all distinguish it from the ordinarily described forms of parametritis. The causes may, in some cases, be the same as in the other form of the disease, but most generally it is due to mechanical causes, such as constipation, with the accumulation and passage of very large fecal masses, connection when the penis is relatively too long, and in other cases, too forcible and too deep an introduction of that organ. These opinions are, moreover, founded on well ascertained facts. If the shrinking of the folds is very marked, constipation and painful evacuation are the result. In one case so great was the obstruction from this cause, that colotomy was necessary. If the contraction differs on the two sides, the result will be torsion of the uterus.

If, now, in opposition to our old ideas, not the bending of the uterus on itself, but the stability of this flexion is the abnormality, and if this fixation

depends not on changes in the uterine tissue proper, but in abnormal inflammatory thickening of the posterior uterine supports, and if, again, the dysmenorrhœa and sterility are not due to the changes in form and position of the organ, but are the result of the parametritis which renders the ante-flexion stabile, then the indications for treatment are all toward the removing of the inflammation and its resulting lesions. M. D. M.

**WEDGE-FLAPPED EXCISION OF THE CERVIX UTERI AND ITS APPLICATION.**—Dr. M. Marckwald (*Arch. für Gyn.*, viii., 1) describes an operation under this name, which was devised and has been practised for many years in a large number of cases by Prof. Simon in his clinic at Heidelberg. In twenty operations seen by Dr. M., it has never been followed by any bad symptoms. The patient is put in Simon's position (see last edition of Thomas.—*REP.*), and the vagina dilated by his dilators. One lip is then seized by a double-hooked forceps and the whole organ dragged down and held. The os is then slit on both sides to the outside edge. The length of the cervix should be measured before the uterus is dragged down, in order to avoid cutting too deep, it being very difficult to tell the point of the vaginal insertion after the uterus is displaced. With a cervix of normal length the cut would be  $\frac{3}{4}$  to  $1\frac{1}{2}$  cm. deep. Beginning on the lip which is not held by the forceps, a semi-circular wedge-shaped piece is excised. The base of the wedge is formed by two cuts parallel to the contour of the os, one being near each edge. The internal cut diverges slightly from the direction of the cervical canal in its depth, and the external converges to meet it. The other lip is excised in the same way, and the two excised pieces, if placed together, would thus make a hollow truncated cone with a somewhat elliptical base. The depth of the excision varies from that of the length of the vaginal portion even as far as the os internum. The edges are brought together with fine Chinese silk sutures, each side being sewed as soon as cut. The hæmorrhage is sometimes pretty severe, but is always controlled by the sutures. Secondary hæmorrhage is not to be feared. If the hypertrophy of the cervix is very great, the base of the wedge must be commenced very high, and even a semi-circular amputation may be done. The operation has the following advantages:

- 1st. It is entirely without danger from infection, etc.
- 2d. No secondary hæmorrhage need be feared.
- 3d. A wide os is left which cannot contract.
- 4th. It is applicable not only to hypertrophy and elongation, but to simple stenosis of the cervical canal. A permanent enlargement can thus be made, something which cannot be attained by any other operation.
- 5th. It is possible by extending the operation to enlarge even the os internum.
- 6th. The operation is quickly and easily done, and healing is complete in eight days, no long continuing ulcer being left.

There are three classes of cases in which this operation can be employed:

- 1st. Stenosis of the cervical canal and, in particular, of the os externum.
- 2d. Elongation and hypertrophy of the vaginal portion, either with or without narrowing of the canal.
- 3d. Elongation of the supra-vaginal portion of the cervix. In the first class it is applicable not only in those cases which are primary and depending on a deformity of the organ, but in another set of cases where the contraction is secondary, due to inflammatory action in the mucous membrane of the cervix; and also in a third class of accidental or organic strictures, such as result from caustic applications, labor, etc. In all these cases it takes the place of the previous methods of treatment, such as forcible or gradual dilatation, or the cutting

method introduced by Simpson. In cases of primary stenosis with ante-flexion there can be used a combination of this operation with that recommended by Sims: take out a wedge from the posterior lip, and sew the anterior to a point low down in the vagina. In one case where Simon did this operation, the result was very satisfactory. In the second class of cases where this operation is called for, cases of hypertrophy and elongation, either from simple hyperplasia or follicular hypertrophy, many operations have been devised to diminish the danger of this amputation, such as hemorrhage, infection, etc. The galvano-cautery, *ecrasur*, Faltenstock's instrument (Scanzoni; similar to tonsillotome.—*REP.*), etc., have been used, but all have their special dangers. Sims introduced the idea of covering the stump by a plastic operation, and thus getting primary union, an idea which has been his starting point for many later methods of operation. Hegar modified Sims' procedure, and Simon also adopted some changes, but both failed to accomplish the desired end. In this new operation, however, all danger is removed. In cases of hypertrophic elongation of the cervix this operation takes the place of simple amputation. In case of true prolapsus with slight elongation of the cervix, it is very well to precede the operation of kolporrhaphia. With tumors of the cervix this operation can be employed, but must be modified to suit the case. M. D. M.

ON CHILDREN BORN MUCH BEFORE THEIR TIME WHICH CONTINUE TO LIVE. By DR. AHLFELD. (*Arch. für Gyn.*, viii., 1.)—After detailing the history of a number of children born in the sixth and seventh month of pregnancy who lived, three of which were met with in his own experience, A. goes on to say that children which, according to the common saying, are untimely, under proper conditions can both live and flourish. The attendant, knowing this, must treat every fœtus which moves or breathes as a being whose life with proper care can be prolonged. As the child does not produce heat enough for itself, it is very necessary to place it where it will maintain a proper temperature. Perhaps the best way is to wrap it in cotton, leaving only the nose and mouth free. A double tub, with water between, which is kept at an even temperature, is the best means, but is not always attainable. The child must be put in a warm place at once after it is born, as any delay may cost it its life. Warm baths frequently repeated seem to be of great use, as allowing of proper motions in a convenient medium, by which the individual heat-production is stimulated. The baths must be warmer than ordinary, and we may take the temperature of the amniotic fluid as a standard. The food must be given at regular intervals, say every hour, and the child awakened for the purpose. Breast milk is the best food, and in the later months a little Malaga wine may be added. For the development of the lungs, it is necessary to awaken the child and make it cry by some slight irritation from time to time. As it gets older it must be taught to nurse. If it gets through the first few months in safety, it is still dangerous to take it into the open air, as there is great danger of lung affections. All draughts must be avoided and sudden changes of temperature of the room. The greatest care on the part of the nurse is necessary throughout the whole. M. D. M.

THE PNEUMONIA OF PREGNANT AND PUERPERAL WOMEN.—At a meeting of the Obstetrical Section of the New York Academy of Medicine, held April 19th, 1875 (the subject for discussion being Puerperal Pneumonia), DR. PAUL F. MUNDÉ said that in none of the books and periodicals at his disposal had he been able to find any mention of "puerperal pneumonia," pneumonia occurring primarily and idiopathically during the



puerperal state. He had himself seen one case of double pneumonia in a puerpera, coming on at the fifth day after confinement, if he remembered rightly, and going on to resolution and recovery without any untoward symptoms, and without differing in the least from the disease as seen in non-puerperal women or in men. He remembers, that the case was looked upon with some apprehension (because it occurred in a puerpera) by Scanzoni, who feared an unfavorable termination from this complication, but gave no reasons for his apprehension. This was the only case of puerperal pneumonia which occurred among some 1200 cases of confinement under Dr. Mundé's care, indeed among 1300 additional, in all 2500 cases. The extreme rarity of its occurrence is doubtless owing, as remarked by some of the gentlemen present, to the absence of exposure to cold and external injurious atmospheric influences, usually producing the disease, enjoyed by puerperal women. As far as he is aware, primary pneumonia in a puerperal woman differs in no respect whatever from the disease as seen in non-pregnant women and in men. Cases of secondary puerperal pneumonia in the course of septicæmia and metropéritonitis are more common than the idiopathic primary forms, although not as frequent as generally supposed.

The pneumonia of *pregnant* women, however, differs materially in one great particular from ordinary pneumonia, viz., it is vastly more fatal. A number of investigations have been made on this subject, although there are really only two papers, which in a measure cover the ground and seek to properly explain the reasons of this increased fatality. These papers are by Gusserow (*Pneumonia in Pregnant Women*) and Wernich (*On Severe Pulmonary Affections during Pregnancy*), from which latter paper the data here communicated are drawn. The causes for the excessive danger and fatality of pneumonia during pregnancy are manifold and all closely connected :

1. *The diminished atmospheric capacity of the thoracic cavity and the lungs* consequent upon the encroachment by the increasing size of the gravid uterus. [Investigations by Gerhardt, Dohrn, (who found the lung capacity greater at the twelfth and fourteenth day after confinement than during pregnancy,) Küchenmeister, Fabius and Wintrich (who deny the diminution of vital lung-capacity during pregnancy).] A consequent greater or lesser sluggishness of the venous pulmonary circulation.

2. *Disturbance of the circulatory apparatus.*

This depends on various conditions :

- a. On defective innervation of the heart. According to Jürgensen, deaths from pneumonia result from cardiac insufficiency. During pregnancy this cardiac insufficiency is especially marked, and depends partly on,

- b. An enlargement of the heart constantly observed during pregnancy (Menière, Larcher, Ducrest, Duroziez), probably an excentric hypertrophy or dilatation, not concentric enlargement, in consequence of which the heart is less powerful during pregnancy, and

- c. On an anæmic condition of the blood during gestation, which entails cerebral anæmia and defective innervation of the heart. This point is still open to discussion, although Spiegelberg and Gscheidlen have lately disproved the anæmic theory of the blood of pregnant animals by experiments on sluts.

All these conditions, the diminished atmospheric lung-capacity, the cardiac insufficiency dependent on ventricular dilatation, general and cerebral anæmia and consequent defective innervation of the heart, together tend to produce a sluggishness of the venous pulmonary circulation, a venous pulmonary engorgement. Supposing, now, that a croupous pneumonia, an inflammation of the lung-tissue with the usual exudation is produced



by the ordinary exciting causes, which act as well on a pregnant woman as on persons in usual health, it is evident that the obstruction to the pulmonary circulation and the danger will be much greater in this already crowded and imperfectly functioning lung, than under ordinary circumstances; œdema of the lung supervenes and causes death. Jürgensen says: "Pregnant women and women in labor, who perish from pulmonary disease, generally die with the symptoms of pulmonary œdema."

The treatment of pneumonia during pregnancy does not differ materially from that adopted in ordinary cases. Some authors (Späth, Châtelain) recommend the apparently plausible plan of relieving the pulmonary engorgement by emptying the uterus of its contents, by inducing premature labor. Gusserow has compiled twenty-one cases, in which labor was prematurely induced for pulmonary disease, and of these 15=71.3% died, surely not a very promising result, and not likely to bring the practice into favor. Spontaneous miscarriage during the disease, be it understood, is not as fatal as when labor is artificially induced. A second therapeutical procedure is venesection, which, although of course not influencing the course of the disease, is supposed to afford, and actually does afford, temporary relief. In one case in the sixth month of gestation observed by Wernich, general blood-letting was employed to relieve the intense dyspnœa. This it did, but, quite unexpectedly and equally unintentionally, labor pains set in, the dyspnœa again became excessive, (reaching its acme with each uterine contraction), the child was rapidly extracted with the forceps, but rapid collapse came on, and mother and child died in a few hours. In a second case, also sixth month, the application of wet cups to the thorax was likewise followed by labor pains; the mother, however, although she suffered for two days after confinement from cerebral anæmia, as indicated by constant hallucinations, recovered.

The danger of local or general blood-letting is thus also apparent.

Our remedies consist chiefly in agents calculated to increase and sustain the power of the heart, above all digitalis, combined perhaps with quinine and mild narcotics. Local remedies, merely sinapisms, cold water compresses, poultices. Distressing dyspnœa and cyanosis may, however, produce a vital indication for venesection, and Wernich proposes to counteract the collapse liable to follow after the blood-letting by performing transfusion, when the strength shows signs of failing.

NOTE.—In my *Report on Diseases of Women* for 1874, published in the last number of this Journal, I stated that, prior to the year 1874, I had been able to find only two publications on the *rapid and forcible dilatation of the female urethra*, both in 1872, one by Hybord, of Paris, the other by Simonin, of Nancy. I expressly remarked, also, that "doubtless some physicians, both at home and abroad, may have practised this operation now and then," thinking it very likely that a manipulation, apparently so simple and obvious, might have been performed occasionally by some operators for a number of years back, without being reported. Since then I have been informed from two sources (Dr. H. Otis Hyatt, of Kinston, N. C., and Dr. Landon B. Edwards, Editor of the *Virginia Medical Monthly*), that Dr. Henry T. Bahnsen, of Salem, N. C., had practised the forcible dilatation of the urethra in three cases for spasmodic stricture of the urethra, from 1869 to 1872, with perfect success. The Transactions of the Medical Society of North Carolina, for 1872, contain a detailed account of these cases. The forcible dilatation was also practised by Dr. R. H. Gale, of Louisville, Ky., in May, 1872, and by Dr. T. H. White, of Richmond, Va., in August, 1872 (*Virginia Medical Monthly*, August, 1874). I take pleasure in rectifying my unintentional omission, and trust that Dr. Bahnsen may not, among the host of published and unpublished records of medical experience, find a successful competitor for the claim of priority for this valuable and beneficial manipulation.

P. F. M.

## REVIEWS AND NOTICES OF BOOKS.

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CONTRIBUTIONS TO THE MECHANISM OF NATURAL AND MORBID PARTURITION, INCLUDING THAT OF PLACENTA PRÆVIA. With an Appendix. By J. MATHEWS DUNCAN. Edinburgh: Adam & Charles Black. 1875. pp. 468.

THIS volume contains the various papers written by Dr. Duncan, on the highly interesting and still but little explored field of the physiology and mechanism of natural parturition, and the nature, causes, and mechanism of many deviations from normal labor. Dr. Duncan is the only English exponent of note of these questions, which have been investigated to some extent in Germany, principally by Schatz, of Rostock, and Lahr, of Marburg. The explanations and deductions of these gentlemen, however, are often so abstruse and complicated as to have been more or less incomprehensible to us, for which reason we confess our preference for the plain, distinct statements and arguments of Prof. Duncan, a perusal of whose work cannot fail to open up many hitherto concealed and unexplained facts to the mind of the general practitioner. The investigations pursued by Duncan, Schatz, Lahr, Schroeder, and others, have already raised Obstetrics to the scientific rank it is entitled to hold, and have opened up a wide field of new and interesting research, wider even and no less important than that of its sister department, Gynecology.

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CYCLOPÆDIA OF THE PRACTICE OF MEDICINE. Edited by DR. H. VON ZIEMMSEN. VOL. X. DISEASES OF THE FEMALE SEXUAL ORGANS. By PROF. CARL SCHROEDER. AMERICAN EDITION, edited by Albert H. Buck, translated by Edward W. Schauffler, Leonard Wheeler, Wm. L. Richardson, Edward B. Bronson, J. Haven Emerson, and Paul F. Mundé. Wm. Wood & Co., New York. Pp. 575.

Want of space compels us to postpone the detailed review, which this book merits, to the next number.

ZEITSCHRIFT FÜR GEBURTSHÜLFE UND FRAUENKRANKHEITEN—  
JOURNAL OF OBSTETRICS AND GYNECOLOGY. Published by E.  
MARTIN and H. FASBENDER, under the auspices of the Berlin  
Gynecological Society. Vol. I., Part 1. Stuttgart: Ferd.  
Enke. Pp. 223.

Although the actual necessity of a new journal in our specialty is not apparent to us—the excellent *Archiv für Gynäkologie* and the scarcely less excellent *Beiträge zur Geburtshülfe*, published by the Berlin Obstetrical Society, sufficiently supplying the wants of the profession in Germany—still we must admit that if the present number of the new Journal is a precursor in point of excellence of the future numbers, there can be no doubt of its meeting with hearty encouragement and proving a perfect success. One suggestion we wish to make, however, viz., the articles in this number are all from the pens of Martin and his present or ex-assistants, and,—although no fault can be found with their quality, still it would seem to us, as though a slight admixture of the opinions held by men bred in other schools might be beneficial in the future.

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ON PARALYSIS FROM BRAIN DISEASE IN ITS COMMON FORMS. By  
H. CHARLTON BASTIAN, M.A., M.D., F.R.S., etc., etc. With  
illustrations. New York: D. Appleton & Co. 1875. Pp.  
340.

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TRANSACTIONS OF THE COLLEGE OF PHYSICIANS OF PHILADELPHIA.  
Third Series, Vol. I. 1875.

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THE CHOLERA EPIDEMIC OF 1873 IN THE UNITED STATES. By JOHN  
M. WOODWORTH, M.D., U.S.A. Containing also a HISTORY OF  
THE TRAVELS OF ASIATIC CHOLERA IN ASIA AND EUROPE. By  
JOHN C. PETERS, M.D., of New York; and IN NORTH AMERICA.  
By ELY McCLELLAN, M.D., U.S.A., and a very extensive BIB-  
LIOGRAPHY OF CHOLERA. By JOHN S. BILLINGS, M.D., U.S.A.  
Govt. Printing Office, Washington. Pp. 1025.

NOTE.—I wish to correct, without delay and most decidedly, an erroneous impression which an otherwise very complimentary notice of the AMERICAN JOURNAL OF OBSTETRICS in the *Edinburgh Medical Journal* for August of this year may possibly have conveyed to the minds of the profession, viz., that I “rejoice in the useful work of translating good German articles into English,” thereby implying that this JOURNAL is more or less devoted to the reproduction of translations from the German. This has not been nor will it be the case, as long as I remain its editor. As I announced in my editorial in August, 1874, “verbatim translations will appear as rare exceptions,” and I now state that since then no translation (or reproduction in English) of any already-published foreign article has appeared under “Original Communications; indeed, there was no space left for such if I had desired to publish them.” Abstracts of German articles are not translations; of these many have been and still will be brought under their proper heading. The only

article contributed directly from Germany to this JOURNAL since May, 1874, was the one on "Hypodermic Injections of Ergotine in Fibroid Tumors of the Uterus," by Prof. Hildebrandt, in the February No., 1875; this paper was promised me fully nine months before it arrived, and was an entirely original paper in the handwriting of the author. A paper on the same subject by the same author appeared in the Berlin *Beiträge zur Geburtshülfe* several months previously; but, as a comparison will at once show, was less complete than, and differed in many respects from, the one sent to me.—Dr. Engelmann's article on "The Mucous Membrane of the Uterus," in the May No. of this year, although based on researches made in Germany, an account of which had already appeared in a German journal several years before, was an entirely new paper with additions, contributed by Dr. E. himself, from St. Louis.—The article on "Dermoid Tumors of the Ovary" in this number was sent me in advance-sheets before its expected publication in the Berlin *Beiträge zur Geburtshülfe*, for which journal it was intended, with the request of the author, that it be translated and published in this JOURNAL simultaneously with its appearance in Berlin. This I consented to do, and hurriedly rendered the paper into English. Surely this (a simultaneous publication in two languages at the request of the author) is not a translation of an already published article! (At the time of going to press the number of the Berlin journal containing Dr. Pauly's article had not reached us; there can be no doubt, therefore, of the simultaneous publication.) Dr. Serdukoff's article, also in this number, was sent me from Russia in English, which (as our esteemed contemporary doubtless knows from previous experience) needed some slight polishing. Of course, I am always pleased to receive good original articles from foreign sources, and shall, as hitherto, translate them myself, if written in French or German, with which languages I am familiar, provided my time permit; if in other languages, I will have them translated. A reproduction, as an original paper, of an article about to be published abroad in another tongue can, however, take place in this JOURNAL only under the conditions imposed on Dr. Pauly's article, that is, the appearance of the translation *simultaneously* with that of the original.—I have thought it worth while to enter into this somewhat lengthy, and perhaps unnecessary explanation, because I do not wish the JOURNAL OF OBSTETRICS to rest under a false imputation, and because I am desirous that its readers and the profession at large should know, once for all, that its chief aim is to publish *good* ORIGINAL articles (for the liberal contribution of which I have to thank the profession), not second-hand versions of foreign papers, no matter how excellent they may be.

P. F. M.

CONTRIBUTIONS HAVE BEEN RECEIVED from DRs. SAMUEL HOWE, of Boston, on "Milk Fever;" W. R. GILLETTE, New York, on a "Case of Hydrocephalic Fœtus;" F. H. RANKIN, N. Y., on a "Polypoid Tumor of the Left Labium Majus;" J. L. MARTYN, Oxford, Iowa, on a "Case of Central Rupture of the Perineum."

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ORIGINAL COMMUNICATIONS.

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MILK FEVER.

A TREATISE TO SHOW THE RELATION WHICH EXISTS BETWEEN THE RISE  
IN TEMPERATURE ON THE THIRD TO FIFTH DAY, AND THE  
BEGINNING OF THE MILK SECRETION.

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Graduation Thesis presented to the Harvard Medical School,

BY

SAMUEL HOWE, M.D., Cambridge, Mass.

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(With 8 Curve Tables.)

THE affection commonly known as milk fever has for a long time been treated of in the works on midwifery, but in scarcely any has the true nature of the disease been thoroughly discussed, or has its real bearing on the secretion of milk been fully settled, and even now, great diversity of opinion as regards it exists.

Before giving the results of my own investigations (which I was able to make through the kindness of the visiting physicians and house officers of the Boston Lying-In Hospital, by allowing the records of their cases to be overlooked and studied) I will bring forward some of the principal views of the writers on this branch of medicine, taking first the English and American authors.

In former times, the term milk fever was used to designate



any fever, puerperal being excluded, which occurred during the first week of the lying-in state. The ephemeral, weed and milk fevers were considered by some authors to be one and the same.

Tyler Smith considered the so-called milk fever as not connected with the breasts alone, but says that the state of the breasts incident to the establishment of the milk, and the condition of the internal surface of the uterus, often produce considerable constitutional disturbance and fever known as milk fever, ephemeral or weed. Hodge and Rigby are of very much the same opinion concerning this affection. Cazeaux says that it is a rare thing to find any fever with the coming of the milk, but sometimes that state of things does exist. He also adds that it is wrong to suppose that this fever necessarily shows some hidden inflammation. Ramsbotham, in speaking of milk fever, which he separates from weed as an entirely different affection, says that it is due to the congestion of the breasts, and that it is much less common than formerly, on account of the more efficient method of treating them. Churchill also separates the two.

Bedford, in his *Midwifery*, in speaking of the condition of the breasts at the beginning of lactation, says that the fever which often accompanies this change is called milk fever, and is due to the violent congestion of the mammae.

Meigs, in his account of inflammation of the breasts, speaks of milk fever in connection with this.

Byford considers that milk fever and congestion of the breasts are synonymous terms. All these authors merely mention the name milk fever, without giving any facts which would go to show that there was any real fever in connection with the arriving of the milk. All speak of the affection in a very loose and confused way, no opinion being given to show what the fever is, or whether the feverish action is due to milk, or to some other cause.

In looking up the subject in the foreign works on obstetrics, I found much more had been written, but that there was great diversity of opinion as regards its true nature, and that even now, the matter is far from settled.

The most exhaustive treatise on the subject I could find, was written by Dr. Justus Schramm, in *Scanzoni's Beiträge*,

Würzburg, 1868, in which the subject is very thoroughly discussed, and the opinions of the most distinguished authors quoted. I have taken from this work some of the opinions of various writers which he quotes. They are as follows:

Jörg, in 1809, gives the following idea as to the real nature of the so-called milk fever. He considers it chiefly a true wound fever, and divides the causes into four classes:

1st. The wounding of the genitals, for the genitals are always more or less torn and bruised during child-birth, and that these of themselves give rise to a certain amount of fever, which he considers a true wound fever.

2d. That indiscretions in diet will give rise to a feverish action.

3d. That fever may be excited by taking cold.

4th. He considers that the congestion of the breasts, which frequently occurs at the coming of the milk, and which is much increased by the milk being secreted too fast, so that it is dammed up and causes the gland-tissue and vessels to stretch, will give rise to fever, which fever he considers a true wound fever, and that sore nipples also will give rise to the same results.

Carus, in 1820, in speaking of the so-called milk fever, considers it as a many-sided affection which is excited by many causes; namely, after-pains, wounds of the mucous membrane about the genitals, errors in diet, sore nipples, restlessness, etc.

Froriep, in 1832, states that milk fever occurs only in those women who put the child to their breast late; that is, after the milk has come, or in those who do not nurse their child at all.

Neumann, in 1839, does not think that the secretion of milk gives rise to any fever, but that this so-called milk fever is due to the stretching and irritation of the skin, brought about by the congestion and enlargement of the mammary glands, which the over-abundant secretion of milk causes.

Kiwisch, in 1841, speaking of the fever which often occurs about three to five days after confinement, says the bruising and injury of the genital apparatus gives rise to fever which is known as milk fever; but he adds that he thinks it has not direct connection with the secretion of milk, and that fever may also be excited by certain atmospheric influences,

for at certain periods these never constant fevers seldom or never appear.

Berndt, in 1846, considers that the milk fever is not the result of topical injury, but is due to the change in the whole system connected with the milk secretion.

Levret thinks that the milk fever is a physiological condition, the consequence of a change in the whole system brought on by the beginning of the milk secretion.

Winckel gives very much the same opinion as Kiwisch, except as regards the atmospheric influences; these he does not mention.

Swieten and Eisenmann consider the milk fever a true wound fever, caused by the separation of the placenta from the uterus.

Locock, in 1843, attributes the fever to too nourishing diet, too warm an atmosphere in the lying-in room, to too great exertion and fatigue in nursing, and also to violent mental excitement. This view is also held by Weissner in 1846.

Of French authors, Jacquemier, in 1846, says: "Les phénomènes dependent de la stagnation de la retention du lait dans les vaisseaux galactophores avec ou sans irritation inflammatoire de ceux-ci."

Velpeau, in 1854, gives the same opinion.

Scanzoni, in 1855, considers the fever due to an insufficient emptying of the milk-vessels, and thinks the term milk fever not a bad one.

Hope, in 1862, says that the fever known as milk fever is not connected alone with the secretion of the milk, but that it is much more likely to be due to a sudden rush of blood to the surface.

Grenser, in 1863, in speaking of the breasts, says that especially when the child is put to the breast late, or there are errors in diet, and when the gland is small and ill-prepared to carry on the work, the beginning of the secretion of milk is attended with fever.

Winckel, who first made investigations with the thermometer on the physiology and pathology of childbed, and showed the importance of its use, both as regards diagnosis and prognosis, in his work, "Pathologie und Therapie des Wochenbetts," and also in a former article on the very subject, considers the term milk

fever unscientific and worthless, inasmuch as it is used for a fever which is produced by many different causes; and he adds that the secretion of milk is not in any way connected with the rise in temperature, but that it is due to the bruising and tearing of the genitals, etc., but in this article he says, that the curve makes it evident that the increase and diminution in the amount of the milk secretion is followed by a corresponding rise and fall of the mercury.

Schramm, from whose article the opinions of many of these authors are taken, and whose investigations will be given before those which I have made, considers that the pure milk fever, which is due to a congestion of the glands of the breast, is attendant upon the coming of the milk, and is rather a rare affection.

Schroeder, in his manual of midwifery, and also in an article on milk fever, in his work, "*Schwangerschaft, Geburt und Wochenbett*," written in 1867, considers the term milk fever a very good one, and much better than mastitis parenchymatosa non-suppurativa, which explains the nature of the affection. He says, that there is always a rise in temperature, connected with the coming of the milk, of a few fractions of a degree C., which is not due to any injury of the genitals, and that occasionally the breasts are very much congested, hard and painful, at the coming of the milk, when the temperature may rise as high as 104° F., 40° C., or even more, and that this rise is due to this congestion; and in speaking of the very case brought forward by Winckel, for he finishes the article with the clinical report of a case which he thinks would be considered a true case of milk fever, but which is explained as a case of wound fever, he thinks it was in reality a good example of this very milk fever; for in this case, while the breasts were congested, hard and painful, the nipples both excoriated, and in one a fissure, the temperature was as high as 104° F., 40° C., and the small ulcer, which Winckel found on the anterior lip of the cervix of the uterus, one of the principal causes of the high temperature, he (Schroeder) considers had very little to do with it, for, according to his experience, these ulcers do not give rise to any marked increase in pulse and temperature.

Winckel, in defending his opinion concerning this very case, says that Schroeder forgot that this woman had been suffering

from endometritis, and that he thought this was also one of the causes of high temperature.

In the *Boston Medical and Surgical Journal*, for February 13, 1873, there is an abstract of an article on the nature of puerperal fever, from the *Archives Générales de Médecine*, by Dr. A. D'Espine. In this the author denies the existence of milk fever as such, but asserts that the fever which occurs is due to the absorption of the lochia by the wounding and laceration of the genital canal, and that small ulcers will be found which are points of absorption.

Schramm, in making his investigations as regards the temperature during the lying-in state, and more especially concerning the rise connected with the secretion of milk, takes the temperature morning and night both in the axilla and vagina. The results obtained by him are as follows: He finds that the temperature is increased with the coming of the milk, excluding those cases in which the rise of temperature was due to some other cause. When the breasts are hard and congested, the rise will be as high as  $102.7^{\circ}$  F.,  $39.4^{\circ}$  C.

The temperature in primiparæ was found to be higher than in multiparæ, and the daily fluctuations greater. These observations are confirmed by those made by Wolf in 1866, except with regard to the daily fluctuations being greater in primiparæ than in multiparæ. He thinks that his results are more likely to be correct, for in the woman who is confined for the first time the organism is more sensitive, and the fluctuations of temperature are therefore more likely to be greater.

It occurred to Schramm, that if the rise in temperature was due to the formation of milk, that in those rare cases where there was no milk at all, there would be no rise on the third day. In his one hundred and eight cases there was one woman who had no milk; in this case there was no rise in temperature. Wolf found this also to be the case.

From so small a number of cases no rule can be, with any accuracy, established, and further investigation is necessary.

As it has been clearly shown by the observations of Wolf and Schramm that with the arrival of the milk there is an increase in temperature, it has also been shown that with a larger secretion the temperature rose higher than when the secretion was small, and that when it was wanting there was no rise.



Schramm thought that if the temperature were taken after the milk was drawn out, it would be found to be lower; for he says that many authors consider the irritation of the milk-vessels the cause of increased temperature. He, therefore, in a few cases, took the temperature before and after nursing, and found that his conjecture was correct.

He mentions a case of Hecker, of a woman who lost her child, and the milk was drawn with a pump; the temperature fell from  $39.7^{\circ}\text{C.}$ ,  $103^{\circ}\text{F.}$ , to  $37.8^{\circ}\text{C.}$ ,  $99.6^{\circ}\text{F.}$

He also gives a case of his own, where the temperature fell from  $38.4^{\circ}\text{C.}$ ,  $101.1^{\circ}\text{F.}$ , to  $37.1^{\circ}\text{C.}$ ,  $98.7^{\circ}\text{F.}$  These were exceptional cases. He found that if there was a large secretion of milk, good nipples, and a strong nursing child, the temperature fell after nursing from .1–.5 of a degree C., .2–.9° F.; but that it was necessary to take into consideration the time of day, as there always is a regular rise towards night.

He also discovered that when the milk was in small amount, and the child greedy, the nursing was followed by a rise instead of a fall. This he explains by the fact that a hungry child, sucking vigorously, will cause a certain amount of nervous irritation. Sore nipples are well known to give rise to a greater temperature, but Schramm thinks that this is over-stated by authors, for he found in no case with simply sore nipples any exhaustive fever.

He then gives three cases of pure milk fever, the temperature rising to about  $40.5^{\circ}\text{C.}$ ,  $104.9^{\circ}\text{F.}$ , the breasts very full and painful, but no other lesion could be found.

He finishes the article with a discussion as to the cause of the rise of temperature with the secretion of the milk, but does not make it as clear as he might, for he distinctly states that he does not think the irritation caused by the stretching of the gland, and due to too great an accumulation of milk, can give rise to the temperature, but says that his views coincide with those of Scanzoni, Velpeau, and others, who are of the opinion that the rise in temperature and pulse are due to a congestion of the gland, caused by a large secretion of milk, and his experiments show that when the milk is drawn off the temperature falls.

He speaks of the relation which exists between mother and child: as long as both act together, the condition of the breasts

is perfect, and the temperature keeps about normal; but if from any cause the milk is secreted too fast, and the breast becomes full and hard, it immediately rises.

Taking these views, I am unable to say whether he thinks the fever is due to too large secretion, or the accumulation of milk in the gland.

I am inclined myself to think it is due to both causes, and that either one will give rise to an increase in temperature and pulse.

The idea that the milk is formed in the blood, and that the retention of it gave rise to fever, has been overthrown by the experiments of Wills in 1850, for he shows that the glands of the breast alone form milk.

If this rise is caused by the wounds and laceration of the genitals, Schramm thinks that it is strange that when the breasts are inflamed and the milk secreted in too great quantities, the temperature should rise, and when the condition is relieved, it should immediately fall.

Schramm concludes by speaking of the so-called febricula theory, which explains this fever as due to certain attacks of a slight contagious fever, although, he says, the febriculæ do occur in lying-in hospitals, for he has noticed when the wards were crowded, the temperature is often higher. He thinks it is false to classify the milk fever with these. He considers under the term febriculæ, indiscretions in diet and restlessness of the child, and leaves out those which occur in connection with wounds of the genitals which are to be considered under the name of wound fever, and also those due to congestion of the breast known as milk fever. He wishes it to be distinctly understood when he speaks of milk fever, that he does not mean every febrile action which occurs during the lying-in state, but considers this affection in its pure form rare, two to four cases in a hundred, but that often we have it in connection with wound fever and febriculæ, etc.

I have given a rough abstract of the three distinguished authors on this subject: Winckel, on the one hand, strongly objected not only to the term, but considered that the condition of the breasts and secretion of the milk have very little to do with any febrile movement; and on the other, Schroeder and Schramm thought the secretion of the milk is almost al-

ways attended with a slight rise in temperature, and occasionally this rise is very considerable.

I will give the conclusions to which I have been able to arrive from studying the record of over one hundred cases. Having given the opinions of the most celebrated authors both of this country and of Europe as regards this so-called milk fever, and having shown how various and different are their views, I wish in the rest of this article to see how far the investigations which I have made correspond to, and in what way they differ from them, and also to prove, if possible, that if such a fever does exist, what is its real origin.

For convenience, these authorities may be divided into two distinct classes: those who consider that the so-called milk fever is connected with some inflammation of the genitals due to laceration or bruises at the time of birth, and those who attribute it to the congestion of the breast, due to the establishment of the secretion of milk. Although most of these authors are not at all clear in their definition of the nature of the disease, I think that in a general way they can be divided into these two classes.

Many other reasons have also been given by some of them, but these will be discussed hereafter. D'Espine and Winckel may be thought as at the head of the first set, and Schroeder and Schramm, the leaders of the other.

The opinions of the English and American writers on this subject may be considered worthless as far as any exact or correct proof goes, for they have in no case followed the matter up thoroughly, and speak only in a general way.

Those investigations only which have been made with the thermometer can be considered as conclusive, since the slight fevers which are common during the lying-in state can only be detected by this means, for not unfrequently the pulse may never rise above 80, while the temperature may be as high as  $103^{\circ}$  or more. This mode of investigation was first used by Winckel, although Hecker was the first to make use of it in connection with this subject. Thinking that this was the only way to arrive at any thorough conclusion concerning the subject (for I began looking the matter over before I obtained any of the foreign works), it occurred to me that an average of the temperature in a hundred cases would show whether there was any rise with the beginning of the milk secretion. I then

obtained permission to examine the books of the Lying-in Hospital, where the temperature is taken morning and evening, in every case of labor for the first ten days. I took an average of daily fluctuations in one hundred and eight cases, all being excluded which were fatal or were connected with any serious trouble. All reports were of women who were able to leave the hospital in about the allotted time of two weeks. I found that the temperature was the highest on the third and fourth days, and that the milk on an average was secreted on these very days. For instance, the milk, as a rule, comes about the third day, and the breasts were fullest on the fourth. The temperature on the evening of the fourth day, was, I found,  $101.16^{\circ}$  F.,  $38.42^{\circ}$  C. This was the highest during the whole ten days.

In regard to daily fluctuation, which Schroeder and Schramm showed, I found that at first there was very little; one degree F., but gradually diminished to between  $\frac{1}{4}$  and  $\frac{1}{2}$  degree. (See Curve I.) I do not wish to show by the table that the milk alone caused a rise of two degrees, for among these cases there were many other causes for this increase, namely, wounds and laceration of the mucous membrane of the genital apparatus, after-pains, sore nipples, indiscretions in diet and mental emotion; but I think that this point is clearly shown—that although there are many causes for this rise, that the additional cause, namely, the coming of the milk, will show itself in a further rise in temperature. It may be argued that an increase of temperature observed on the fourth day in this large number of cases is really due to the laceration of the genitals, and in other causes necessarily incident to labor, rather than to the beginning of the secretion of milk and the change in the condition of the breasts; but I think that the cases I shall cite hereafter, in which these lesions and lacerations were entirely wanting or very slight, will show that in those cases there was a marked increase of temperature, which can hardly be attributed to anything but the changes which were going on in the breasts.

As regards the normal increase of temperature with the arrival of the milk, I am unable to give any conclusion, drawn from an average of sufficient cases to make the point clear, for it is not common to find many cases which are perfectly normal all the way through; and if we follow the opinion of some authors who consider that there is almost no case of labor in

which there was no injury done to the vagina or mouth of the uterus, it seems impossible to say which cases are strictly normal and which abnormal. The tables in the works of Wolf and Schramm make the rise in temperature connected with the secretion of milk a few fractions of a degree C.

I was able to collect, out of the one hundred cases, eighteen in which the coming of the milk was accompanied with a rise on an average of  $103^{\circ}$  F.,  $39.4^{\circ}$  C. In some of the cases the temperature rose much higher,  $105^{\circ}$  F.,  $40.5^{\circ}$  C., but these cases were not what might be considered pure, that is, there were often causes which came into play to make this increase, but they were either so slight that they could be neglected, or they came at some other time during the lying-in state, so that they could be easily separated. I selected these cases, because at the time of the high temperature the milk was in considerable amount, and the breasts full, slightly painful, and when this condition of things was relieved, the temperature immediately fell. To illustrate this point, I will give two or three cases which I think will show that, with a large secretion of milk and congested breasts, the temperature will rise, and as the congestion diminishes, it falls.

Emma P., 19; American; primipara; medium size; dark; labor natural; child, boy,  $6\frac{1}{2}$  lbs., born at 3 o'clock A.M., Oct. 30th; breast medium size, nipples well developed.

At 9 A.M. the patient was comfortable. Flowing ordinary.

October 30th, morning. Pulse 84. Tem.  $99.7$ .

“ “ evening, “ 80. “  $98.4$ .

“ 31st, morning. “ 80. “  $98.3$ .

“ “ evening. “ 80. “  $99$ .

November 1st, morning. Pulse 80. Tem.  $99.7$ . Slight amount of milk; no abdominal tenderness; flowing all right; passes water without any difficulty.

November 1st, evening. Pulse 84. Tem.  $102.3$ . Milk comes freely; child nurses well; breasts full, very slight pain or tenderness about them.

November 2d, morning. Pulse 84. Tem.  $99$ . Breasts not so full; milk free; no congestion about glands; no great rise in temperature. Oleum ricini  $\bar{3}$  ss.

November 2d, evening. Pulse 88. Tem.  $100.5$ . Medicine operated once.



November 3d, morning.	Pulse 92.	Tem. 100.
“ “ evening.	“ 84.	“ 100.7.
“ 4th, morning.	“ 84.	“ 98.7.
“ “ evening.	“ 68.	“ 100.
“ 5th, morning.	“ 76.	“ 99.
“ “ evening.	“ 67.	“ 99.5.
“ 6th, morning.	“ 72.	“ 98.7.
“ “ evening.	“ 60.	“ 98.7.
“ 7th, morning.	“ 80.	“ 99.5.
“ “ evening.	“ 80.	“ 99.7.
“ 8th, morning.	“ 88.	“ 99.
“ “ evening.	“ 64.	“ 98.7.

Patient sat up, was discharged four days after this, well.  
(Curve II.)

In the above case it is shown that with the establishment of the milk the rise in temperature was higher at this period than at any time during the ten days. The breasts were full of milk and the gland-tissue was stretched, but as soon as the child had relieved it, and the gland became accustomed to the work it had to do, the variations of the thermometer were like those in a common case of labor.

I will give another illustration of the same point.

Mary F., born in Portugal; 35; small woman, very dark complexion; breasts large, nipples well developed; multipara; labor very short; flowing ordinary; child, male, 7 lbs., born December 15th, at 1 A.M.

Dec. 15th, morning. Pulse 64. Tem. 100. Res. 18.

“ “ evening. “ 64. “ 98.4. “ 14.

December 16th, morning. Pulse 76. Tem. 99.2. Res. 16.  
Uterus a little large; fl. extr. ergot  $\frac{5}{8}$  ss. every three hours.

Dec. 16th, evening. Pulse 52. Tem. 98.7. Res. 16.

“ 17th, morning. “ 76. “ 99.6. “ 18.

December 17th, evening. Pulse 56. Tem. 99. Res. 17.  
Uterus much smaller; stop ergot; no milk yet.

December 18th, morning. Pulse 80. Tem. 99.5. Res. 16.  
No operation since confinement. Oleum ricini  $\frac{5}{8}$  ss.

December 18th, evening. Pulse 88. Tem. 99.1. Res. 14.  
Oil operated four times.

Dec. 19th, morning. Pulse 85. Tem. 99.3. Res. 16.

December 19th, evening. Pulse 120. Tem. 103.8. Res. 24.  
Breasts full and hard; milk comes freely; child nurses well.

Dec. 20th, morning. Pulse 88. Tem. 100.2. Res. 20.

" " evening. " 88. " 99.8. " 22.

" 21st, morning. " 76. " 98.8. " 16.

" " evening. " 64. " 99.1. " 18.

" 22d, morning. " 72. " 99.6. " 20.

" " evening. " 80. " 99. " 18.

" 23d, morning. " 80. " 99.3. " 18.

" " evening. " 56. " 98.4. " 14.

" 24th, morning. " 88. " 99.4. " 18.

" " evening. " 72. " 99. " 18.

Patient sat up, was discharged well. (Curve III.)

In this case there was no abdominal tenderness or after-pain. There was no reason to which to attribute the rise of temperature, but to the congestion of the breasts on the coming of the milk. To this set of cases I will add one more to illustrate the same point; it differs from the others, in that the patient had but very little milk and never enough fully to satisfy her child, although she was a very strong, healthy woman.

M. G., 23; American by birth; primipara; large, powerful woman; labor normal; child, female, eight pounds. Born May 12th, at 11.20 p.m. May 13th, morning. Pulse 64. Tem. 100.5. Flowing all right; patient comfortable; passed a good night.

May 13th, evening. Pulse 68. Tem. 90.3.

" 14th, morning. " 76. " 99.3.

" " evening. " 80. " 100.

" 15th, morning. " 76. " 98.7.

May 15th, evening. Pulse 80. Tem. 101.5. Slight amount of milk; breasts quite full.

May 16th, morning. Pulse 104. Tem. 100.3. Child nurses well; not enough milk.

May 16th, evening. Pulse 108. Tem. 100.7.

May 17th, morning. Pulse 98. Tem. 102. Milk in considerable amount, but hardly enough for child.

May 17th, evening. Pulse 88. Tem. 101.

" 18th, morning. " 84. " 98.7. (Curve IV.)

Temperature was not taken after this; the child had to be fed all the time a little; sat up on the 22d. In these three

cases, I think that it is pretty clearly shown that the coming of the milk and the congestion of the mammary gland are accompanied with a rising temperature, and that the fuller the breasts are, the higher the temperature will rise.

I have only shown by these cases that at the coming of the milk secretion there is an increase of temperature; let us now see if congestion takes place after the milk secretion has been fully established, from any cause, death or sickness of the child, and if there will also be a rise in temperature in proportion to this congestion.

Some authors consider this fever as due entirely to the beginning of the milk secretion, and a peculiar physical change which the body undergoes at the beginning of this new process, and not due (Levret and Berndt) to a congestion of the breasts or an irritation produced by too great stretching of the galactophorous vessels, which is caused by the too rapidly secreted milk.

The following case is that of a woman who was prematurely delivered at six and a half months:

Elizabeth P. Multipara; American, 26; patient strained herself lifting a wash-tub; water came away two days after; child, female,  $3\frac{1}{2}$  pounds; born at 12 m., March 14th.

March 14th, evening. Pulse 100. Tem. 98.7.

March 15th, morning. Pulse 80. Tem. 98.7. Feels nicely; flowing all right.

March 15th, evening. Pulse 96. Tem. 98.7.

" 16th, morning. " 96. " 98.7.

March 16th, evening. Pulse 88. Tem. 98.7. Complained of bad headache.

March 17th, morning. Pulse 88. Tem. 98.5. Oleum ricini  $\frac{5}{8}$  ss. Milk comes a little.

March 17th, evening. Pulse 84. Tem. 99.5. Milk comes freely; nurses other children; enough to nurse two children.

March 18th, morning. Pulse 92. Tem. 99.

" " evening. " 88. " 99.5.

March 19th, morning. Pulse 92. Tem. 99.5. Concludes to dry up her milk and stop nursing.

March 19th, evening. Pulse 88. Tem. 101. Breasts full.

March 20th, morning. Pulse 88. Tem. 100.4.

March 20th, evening. Pulse 80. Tem. 101. Breasts slightly

painful; magnesiae sulph.  $\frac{3}{4}$  ss.; breasts rubbed with camphorated oil. To take as little liquid as possible.

March 21st, morning. Pulse 84. Tem. 98.5. Breasts all right; no pain; milk slight in amount; running out.

March 21st, evening. Pulse 84. Tem. 99.5.

“ 22d, morning. “ 88. “ not taken.

“ “ evening. “ 84. 98.5. (Curve V.)

This case shows very plainly that although there was a slight rise with the beginning of the milk secretion, when, after a few days, the woman concluded to stop nursing, congestion which the over-distended breasts caused, produced a much higher temperature than took place at the beginning of the secretion of the milk. Scanzoni says, that he has noticed the same fact, that the woman who suddenly weans her child, or whose child has died, is very apt to have a certain amount of congestion and pain about her breasts, with a corresponding rise in temperature. He considers the rise to be due to the stretching of the milk-vessels by the dammed-up milk which is not drawn off, and does not run out of its own accord fast enough. Now we have seen that the beginning of the milk secretion is attended with an increase in temperature, and that the irritation of the over-loaded breasts is also accompanied by a rise corresponding to the severity of the irritation. Let us see if the opinions of Schramm are borne out by further investigation, namely, that if this congestion and irritation of the breasts is due to an overloading of the milk-vessels, the alleviation of this condition will be followed by a corresponding fall of temperature.

Schramm, as I have before stated, took the temperature in a certain number of cases before and after nursing, and found there was a certain slight fall.

In the clinical reports which I have, this was not done; but I have collected two or three cases which will show this point.

These cases are those of women whose children, on account of sickness, refused the breast, which on that account became too full and congested, causing considerable pain (the milk was removed by rubbing with a salve and a cathartic was given, the amount of milk was very much lessened, and the congestion reduced at once); or they are cases in which the congestion of the glands was so great that serious trouble was

feared, and the breasts were strapped so that a firm wall was formed round the gland, and the stretched tissue relieved. After this the principal cause of irritation was taken away.

Margaret McM. American; primipara; 21 years old. Breasts normal in size, nipples well developed; labor normal; child, girl, 9½ lbs.; born the 16th of Nov.

The patient passed through the first seven days of the lying-in state without any serious trouble, although there were at times considerable abdominal tenderness and offensive lochia; but on the sixth and seventh days the temperature was about normal. The report begins on the eighth day.

Nov. 23d, morning. Pulse 84. Tem. 98.8.

Nov. 23d, evening. Pulse 84. Tem. 90.4. Child not very well; breasts all right; no abdominal tenderness; lochia not offensive.

Nov. 24th, morning. Pulse 100. Tem. 98.6. Child very sick, will not nurse; breasts full; magnesia sulph. ʒ ss. given.

Nov. 24th, evening. Pulse 108. Tem. 103.5. Breasts very full, hard and painful; medicine does not operate; to be repeated; breasts to be rubbed with camphorated oil.

Nov. 25th, morning. Pulse 92. Tem. 100. Medicine operated once; breasts all right; no pain or fulness.

Nov. 25th, evening. Pulse 92. Tem. 99. Patient sat up the next day; no more trouble with the breasts; child nurses a little.

In this case the emptying of the breasts was followed by an immediate fall in temperature. The rubbing acted as a pump and withdrew all the milk from the gland. The fall was from 103.5° F., 39.7° C., to 100° F., 37.7° C. = 2° C., or 3½° F.

I add this case of a patient who had a very rapid delivery and a very severe attack of after-pains on the second and third days:

Margaret L. American; 18 years old; multipara; child, male, 9 lbs.

On the eleventh day I begin the report.

May 8th, morning. Pulse 80. Tem. 98.5. Patient had had some trouble with breasts and nipples before, which had been relieved by cathartics.

May 8th, evening. Pulse 72. Tem. 98.5.



May 9th, morning. Pulse 68. Tem. 100. Right nipple sore, but better, that breast not used. Patient sat up.

May 10th, morning. Pulse 76. Tem. 99. Doing nicely. Does not nurse with right breast; breast quite full.

May 10th, evening. Pulse 96. Tem. 103. Breast very painful and hard; to be rubbed with camphorated oil; magnesia sulph.  $\frac{5}{8}$  ss.

May 11th, morning. Pulse 80. Tem. 98.5. Feels nicely; breasts soft, not painful.

May 11th, evening. Pulse 80. Tem. 98.9. In this case, although the child was not taken away, yet on account of the nipple of one breast it was not put to it. The breast on that account was full and hard; but as soon as relieved the temperature fell.

Of those cases in which congestion of the breasts was so great that they were strapped in order to prevent any further trouble, and in that way the tension relieved, I will give two.

Bridget II. Irish; 23; multipara. Labor rather long in first stage; very short in second. Child, girl,  $6\frac{1}{2}$  lbs.; born April 14th, at 7.45 A.M. 11 A.M., Pulse 88. Tem. 99.5. Slight after-pains.

April 14th, evening. Pulse 60. Tem. 100.

“ 15th, morning. “ 56. “ 98.5.

“ “ evening. “ 60. “ 98.5.

“ 16th, morning. “ 60. “ 98.5.

April 16th, evening. Pulse 64. Tem. 99. Slight amount of milk.

April 17th, morning. Pulse 72. Tem. 101. Afterwards 103.5. Breasts very full and hard; patient chilly; breasts strapped.

April 17th, evening. Pulse 100. Tem. 102. Breasts much better.

The rest of the case is not of any particular value to report. This next case also shows this point very well; the fall of temperature is greater.

Charlotte J. Swede; 31; primipara (?); labor normal; breasts small; nipples well developed; child, boy,  $8\frac{1}{2}$  lbs.; born 11.45 P.M., Sept. 10th.

Sept. 11th, morning. Pulse 64. Tem. 99.5.

“ “ evening. “ 60. “ 98.5.

Sept. 12th, morning. Pulse 64. Tem. 98.75.

Sept. 12th, evening. Pulse 72. Tem. 99.5. Slight amount of milk.

Sept. 13th, morning. Pulse 96. Tem. 101. Breasts very full; rubbed with camphorated oil.

Sept. 13th, evening. Pulse 104. Tem. 102. Breasts fuller, and quite painful.

Sept. 14th, morning. Pulse 100. Tem. 102. Breasts still full, and more painful.

Sept. 14th, evening. Pulse 96. Tem. 102.5. Breasts full and hard; so much pain that they were strapped; Rochelle powder given.

Sept. 15th, morning. Pulse 80. Tem. 98.5. No pain in breasts; not at all hard; milk free.

Sept. 15th, evening. Pulse 76. Tem. 101. Breasts full again, but not so much as before.

Sept. 16th, morning. Pulse 80. Tem. 100.3. Breasts rather better.

Sept. 16th, evening. Pulse 68. Tem. 100.5.

" 17th, morning. " 76. " 101.

Sept. 17th, evening. Pulse 80. Tem. 98.5. Breasts all right.

Sept. 18th, morning. Pulse 64. Tem. 98.5.

" " evening. " 72. " 99.7.

" 19th, morning. " 76. " 99.3.

Sept. 19th, evening. Pulse 68. Tem. 99.5. Patient sat up. Discharged on the 24th, well.

These four cases will, I think, confirm the opinion of Schramm, although they are exceptional; nevertheless, they show the principle that the irritation and stretching which the breasts experience when very much congested, will cause a certain amount of febrile movement, which will be immediately checked when this state of things is relieved.

Sore nipples are mentioned in all books on obstetrics as cause for high fever, and sometimes a good deal of mental anxiety and excitement are connected with them, for the pain is often very severe.

In relation to this subject, I should like to say that I find from examination of the cases of labor set down in the records of the Lying-In Hospital, that sore nipples (I mean by this ex-

pression all the numerous diseases to which the nipples are subject during lactation) are in themselves alone not connected with any inflammation of the breasts, and are not accompanied with any marked fever. The temperature may be as high as 100–101, but no very marked increase, such as is often connected with inflammation of the breasts.

The mental excitement is often accompanied with a very marked fever, for the pain is often so severe as to make the woman cry out, and even sometimes to be delirious; this is mentioned in the text-books, but is very rare.

In these cases the temperature is very high, but it must not be considered as due to the lesion about the nipples directly, but to the peculiar mental condition which is brought about by acute pain. I will add that these occur only in hysterical women. The slight fever, which I have mentioned as connected with the nipples alone, is a true wound fever. The more severe fever, which is due to the breasts being inflamed, cannot be thought of as a pure wound fever, but as one that has other causes than that of merely diseased nipples.

The third form, namely, the fever due to mental excitement, is a peculiar febrile movement, which is mentioned by many authors, but by none in a very satisfactory manner.

Some authorities, as I have said, speak of a rise in temperature connected with violent mental emotion. Under this head hysteria can, I presume, be classed; but none give any cases to illustrate the fact, and pass it over in a superficial manner. I wish to report two or three cases in regard to this very point, since a knowledge of this fact might be useful, and a help to diagnosis.

I will state, before giving the cases, what I wish shown, namely, that a very high pulse-temperature may be brought about by mental excitement, or by hysteria, which is not connected with any other trouble, and which will often pass off with great rapidity. The fever, if it occurred from any other cause, would probably be fatal, but under these circumstances it is of no consequence; the diagnosis is therefore important. The first case is as follows:

Mary L. 20; Irish; primipara. Always strong and well. Child, girl; 7 lbs.; born 3.30 p.m., Nov. 30th. Labor normal; breasts well developed, nipples prominent.

Nov. 30th, evening. Pulse 88. Tem. 100.8.

Dec. 1st, morning. " 68. " 98.9.

" " evening. " 72. " 100.4.

Dec. 2d, morning. Pulse 124. Tem. 103.9. Patient complained of great abdominal tenderness, pain, and is chilly; very nervous, frightened, thinks she is going to die. Case diagnosticated as hysteria. The abdominal tenderness was in reality very slight, less than is often present after labor.

Dec. 2d, evening. Pulse 120. Tem. 105. About the same, complaining of every kind of ache and pain.

Dec. 3d, morning. Pulse 116. Tem. 103.8. Oleum ricini,  $\frac{5}{5}$  ss.

Dec. 3d, evening. Pulse 116. Tem. 105. About the same; not very much milk; child nurses well.

Dec. 4th, morning. Pulse 100. Tem. 103.2.

Dec. 4th, evening. Pulse 100. Tem. 102.5. Much better; feels nicely.

Dec. 5th, morning. Pulse 88. Tem. 100.9.

Dec. 5th, evening. Pulse 88. Tem. 100.2. Nipples slightly painful in nursing; used nipple shield.

Dec. 6th, morning. Pulse 80. Tem. 100.2.

" " evening. " 87. " 101.6.

" 7th, morning. " 80. " 100.4.

" " evening. " 84. " 101.6.

" 8th, morning. " 68. " 101.1.

" " evening. " 88. " 101.8.

Dec. 9th, morning. Pulse 120. Tem. 105.4. Patient very nervous and hysterical; complains of great tenderness about abdomen. On examination, this was found wanting.

Dec. 9th, evening. Pulse 120. Tem. 105.4. About the same.

Dec. 10th, morning. Pulse 92. Tem. 100.8. All right.

Dec. 10th, evening. Pulse 92. Tem. 102.8.

Dec. 11th, morning. Pulse 80. Tem. 98.2. All right, sat up. Discharged on the 16th, well. The second case shows this much better.

Kate N. 22; Irish; primipara; labor normal; child, girl, 7½ lbs.; born at 5.45 P.M., Sept. 25th. Flowing considerable.

Sept. 25th, evening. Pulse 76. Tem. 101.

" 26th, morning. " 71. " 99.

Sept. 26th, evening. Pulse 72. Tem. 99.5.

" 27th, morning. " 72. " 100.

" " evening. " 96. " 102.

Sept. 28th, morning. Pulse 108. Tem. 102.5. Olenm ricini  $\frac{3}{4}$  ss. given.

Sept. 28th, evening. Pulse 102. Tem. 105.3. Oil operated twice; patient very nervous and hysterical; nipples slightly painful.

Sept. 29th, morning. Pulse 92. Tem. 100. All right. Afterwards had an hysterical attack. Pulse 120. Tem. 104.5. Complained of pain and tenderness of abdomen; hysterical.

Sept. 29th, evening. Pulse 128. Tem. 106.8. Patient very nervous; greatly excited; no pain or tenderness; potassii bromidum, gr. xxx.; very little milk.

Sept. 30th, morning. Pulse 112. Tem. 105.8. Pulv. Doveri gr. x. last night to quiet the patient.

Sept. 30th, evening. Pulse 112. Tem. 105.8. Patient still very nervous; potassii bromidum to be repeated.

Oct. 1st, morning. Pulse 96. Tem. 101.5. Much better. Potassii bromidum, gr. x. every three hours.

Oct. 1st, evening. Pulse 108. Tem. 103.

" 2d, morning. " 92. " 102.

" " evening. " 100. " 102.

Oct. 3d, morning. Pulse 132. Tem. 105.8. Patient very hysterical again; got out of bed twice, out of her mind.

Oct. 3d, evening. Pulse 128. Tem. 105.5.

Oct. 4th, morning. Pulse 88. Tem. 98.8. All right; feels nicely.

Oct. 4th, evening. Pulse 104. Tem. 101.8.

Oct. 5th, morning. Pulse 108. Tem. 101.8. Stop potassii bromidum.

Oct. 6th, morning. Pulse 88. Tem. 99. (Curve VI.) Sat up; was discharged on the 10th.

In these cases there was nothing to which to attribute this high pulse and temperature, but the mental condition.

The breasts, etc., were all right.

In another case the temperature rose as high as 106° F., due to the same trouble, hysteria. I will now conclude the report, by giving two pure cases of milk fever, for in them the breasts were the only organs affected. The uterus was all right, the



nipples sound, no nervous symptoms, no wounds of the genitals as far as could be ascertained.

Eliza J. W. 28; American; multipara; 5th child; labor normal, quite short; breasts about normal size; nipples well developed; child, male, 9 lbs.; born 6.50 p.m., Oct. 14th.

Oct. 15th, morning. Pulse 76. Tem. 98.5.

“ “ evening. “ 80. “ 99.

“ 16th, morning. “ 76. “ 99.8.

“ “ evening. “ 84. “ 99.5.

Oct. 17th, morning. Pulse 120. Tem. 105. Breasts very hard; not much pain; a cloth soaked in camphorated oil applied; magnesia sulph.  $\frac{5}{8}$  ss.

Oct. 17th, evening. Pulse 120. Tem. 104.5. Medicine did not operate. Rochelle powder with magnesia sulph.  $\frac{5}{8}$  j.; milk comes freely; child nurses well; breasts still very hard; not much pain.

Oct. 18th. Pulse 92. Tem. 100.5. Rochelle powder operated three times.

Oct. 18th, evening. Pulse 100. Tem. 101.8. Although the child nurses well, still the milk is secreted too fast, and the breasts quite hard, but not so much so; milk runs out.

Oct. 19th, morning. Pulse 88. Tem. 100.3.

Oct. 19th, evening. Pulse 72. Tem. 100.8. Breasts soft; milk free.

Oct. 20th, morning. Pulse 76. Tem. 100.

“ “ evening. “ 80. “ 99.

“ 21st, morning. “ 76. “ 100.

“ “ evening. “ 88. “ 98.5.

“ 22d, morning. “ 84. “ 99.3.

“ “ evening. “ 80. “ 99.8.

“ 23d, morning. “ 82. “ 99.8.

“ “ evening. “ 84. “ 100.5.

Sat up next day; discharged the 28th, well. (Curve VII.)

In this case, after the milk had been fully established, the temperature fell and there was no further trouble.

Margaret H. 18; American; primipara; labor normal; breasts large; nipples well developed; child, boy, nine pounds; born 10.15 a.m., Oct. 15th.

Oct. 15th, morning. Pulse 64. Tem. 100.

“ “ evening. “ 68. “ 100.8

Oct. 16th, morning. Pulse 60. Tem. 99.5.

" " evening. " 60. " 100.

" 17th, morning. " 76. " 99.8.

Oct. 17th, evening. Pulse 92. Tem. 102.8. Milk came in slight amount.

Oct. 18th, morning. Pulse 104. Tem. 103.5. Oleum ricini  $\bar{5}$  ss.; milk more abundant; breasts full.

Oct. 18th, evening. Pulse 112. Tem. 105. Oil did not operate; Rochelle powder operates twice; breasts very full and hard; camphorated oil.

Oct. 19th, morning. Pulse 104. Tem. 101.8. Breasts much better.

Oct. 19th, evening. Pulse 96. Tem. 103.8. Breasts fuller.

Oct. 20th, morning. Pulse 88. Tem. 100.8. Breasts nicely.

Oct. 20th, evening. Pulse 80. Tem. 101.5.

" 21st, morning. " 80. " 99.8.

" " evening. " 76. " 100.8.

" 22d, morning. " 64. " 99.8.

" " evening. " 60. " 100.

Oct. 23d, morning. Pulse 72. Tem. 99.8. Child went away.

Oct. 23d, evening. Pulse 80. Tem. 100.3. Breasts full; Rochelle powder, with magnesia sulph.  $\bar{5}$  j.

Oct. 24th, morning. Pulse 84. Tem. 101. Cathartic operated three times; breasts still full.

Oct. 24th, evening. Pulse 72. Tem. 101.5. Breasts quite painful; sat up. (Curve VIII.)

Discharged well on the 29th.

These cases are remarkably good illustrations of the pure milk fever, the mastitis parenchymatosa non suppurativa of Schroeder. It is rare to find these cases, for, in the pure form, they occur only two to four times in a hundred cases.

In regard to the febriculæ theory, of which Hecker speaks in 1861, I am able to say nothing, for this point is not shown in the cases I have obtained.

As to the fever caused by small ulcers of the genital canal, I am unable to say anything, except that Winckel and D'Espine consider them the causes of high fever, and that Schroeder thinks they have very little to do with it.

The theory that the milk fever was due to a metastasis, has been shown to be false by Gascogne, for milk injected into the veins does not produce fever.

I will not take up the opinions of the authorities I have quoted and discuss them, for I hope I have been able to show that the milk fever is a real and not an imaginary affection, and that it is due to the changes which go on in the breasts.

The theory of the wound fever is well proved, and it will not be necessary to discuss it further, for it is well known that a fever may arise from the injury done to the genitals during labor, but that this is the sole cause for fever during the lying-in state, I think I have shown not to be true.

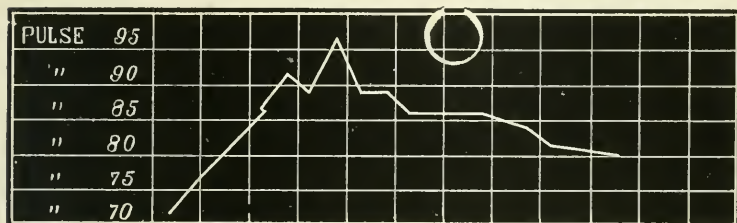
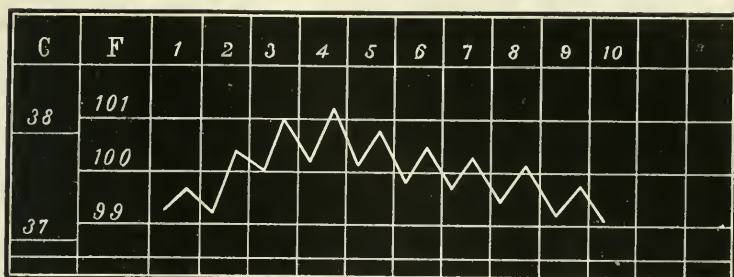
In finishing this paper, I wish to state what conclusion I have arrived at. That there is such an affection as milk fever, I fully believe, and I think it is due to two causes which are as follows: It has been shown that at the coming of the milk there is always a slight rise in temperature, which may become considerable, and which I think is due to the sudden development of this new function, for there is a congestion of the glands, which are very richly supplied with nerves and vessels, causing a certain amount of nervous action which will give rise to an increase of temperature; this increases as the congestion increases, but if the child draws the milk immediately as soon as it is formed, the pulse and temperature fall. If this state of things is not relieved, however, a new force comes into action, the milk accumulates in the gland vessels, stretching the skin until it becomes tense, and the fever increases instead of diminishing, not on account of the congestion of the breasts, but owing to the stretching and irritation which this produces when the glands are fullest and the breasts hardest; then the temperature is at its maximum.

I would wish to say, that I do not consider every rise in temperature during the lying-in state due to this, but I consider that the milk fever in a pure form is rare, for it is hardly fair to consider that the establishment of a natural function should give rise to any febrile disturbance. I may say, however, that the fever which occurs often during the first week of childbed, is not a simple one at all, but is due to a combination of circumstances, namely, wounds of the genitals, congestion of the breasts, indiscretions in diet, mental disturbance, etc.

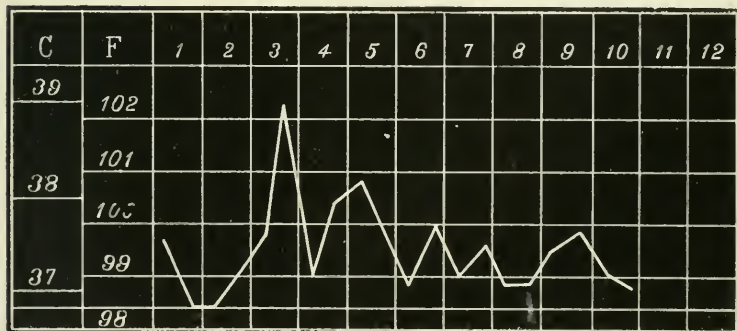
I will conclude by saying that the term *milk fever* is not only useful, but necessary.

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CURVE I.



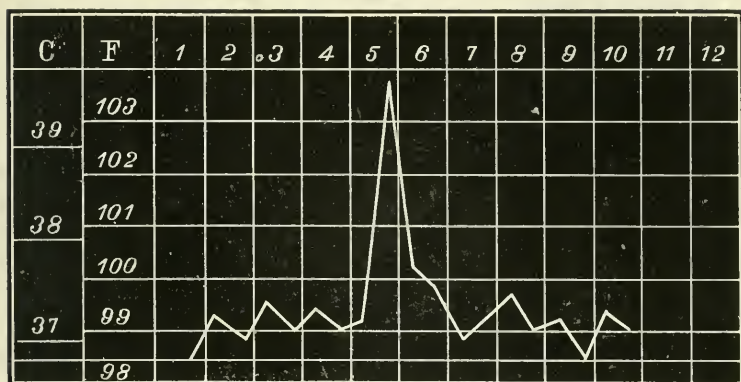
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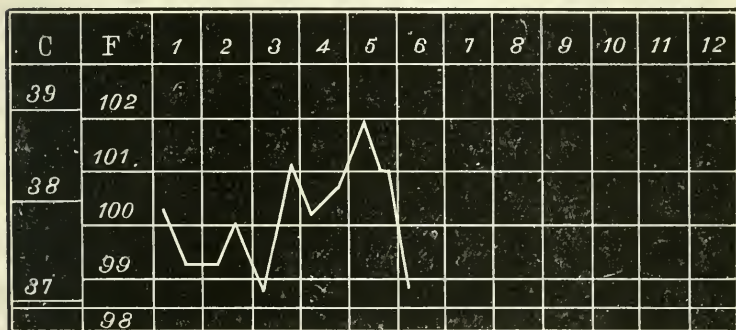




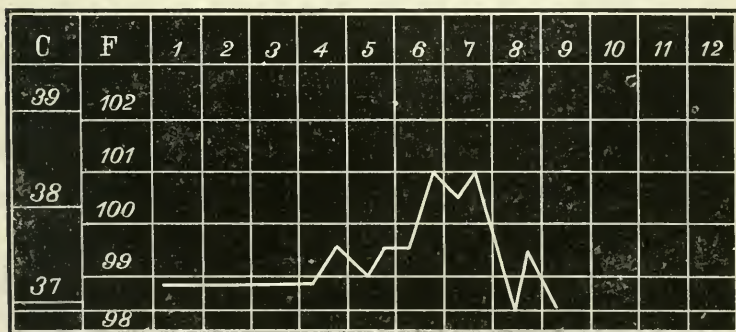
CURVE III.



CURVE IV.

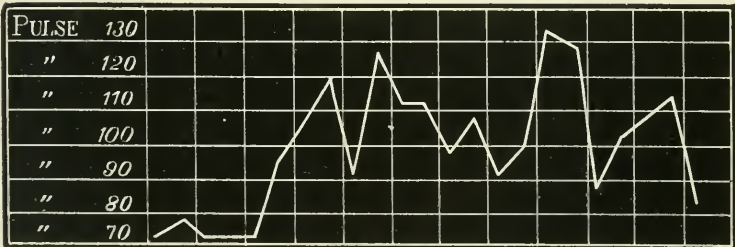
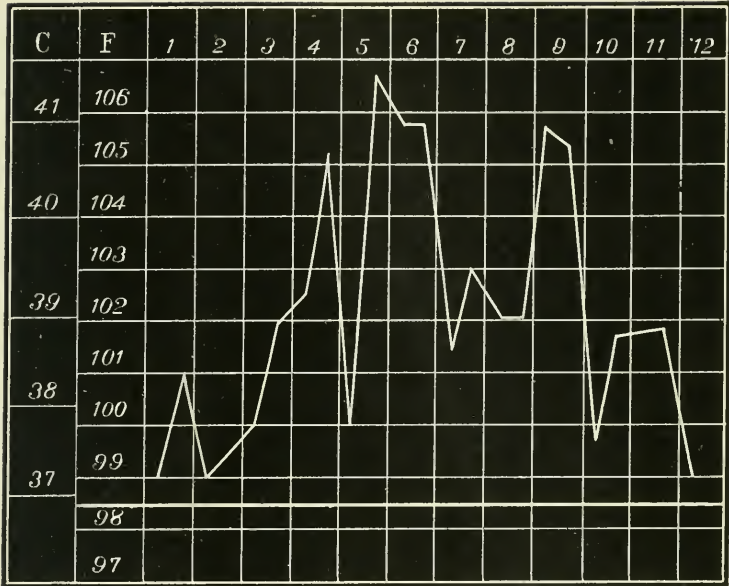


CURVE V.



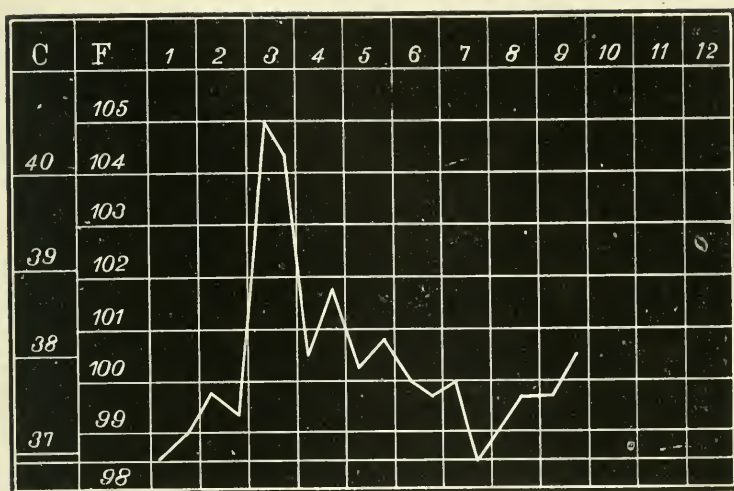


CURVE VI.

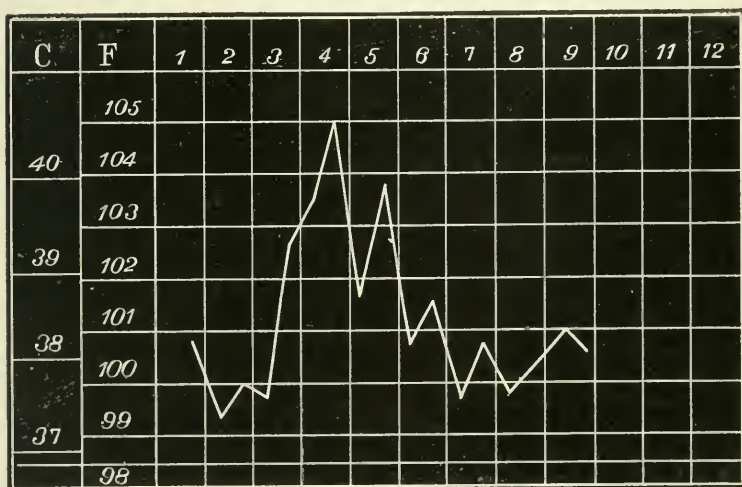




CURVE VII.



CURVE VIII.







## ON MASTURBATION AND HYSTERIA IN YOUNG CHILDREN.

REMARKS BEFORE THE MEDICAL JOURNAL ASSOCIATION OF N. Y., IN  
NOVEMBER, 1875.

BY A. JACOBI, M.D.,

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GREATER accuracy of observation, and increasing knowledge of a large number of physiological facts have, of late years, had a peculiar influence on the current pathological views concerning the nervous system. More than ever, local and circumscribed causes have been sought for, and found, for distant or general nervous symptoms; and the tendency to localize disease—the great achievement in modern diagnostics—has, in my opinion, contributed much to develop the disposition on the part of medical men, to overlook general causes over an actual or alleged local anomaly. I am afraid that in many cases the charm of positiveness has given rise to serious mistakes. For instance, I believe that the doctrine of reflex spasms and particularly that of reflex paralysis will be greatly reduced in influence when the cases will be more thoroughly studied. This class of cases has been recruited principally from nerve affections connected with diseases of the genito-urinary organs. A number of years ago the cases of “reflex paralysis” depending on a disease of the kidneys were quite numerous indeed. Since they were carefully studied on the post mortem table, it grew evident that the consecutive nervous symptoms resulted from the direct contiguous transmission of the morbid process along the nerves upwards until it reached the spinal cord.

Thus the diagnosis of reflex paralysis had to be changed into that of an ascending neuritis and myelitis. As the medical mind cannot be satisfied until all the links between cause and effect are clearly seen, we ought to take exception to the readiness with which nervous symptoms are apparently explained. If there is in this connection any subject in regard to which I still

—in spite of the vigorous and sagacious efforts of such observers as Drs. L. A. Sayre and F. N. Otis—hesitate to fully accept their views, it is the frequency of (neuralgic, spastic and) paralytic symptoms depending upon phimosis.<sup>1</sup> But lately (*Medical Record*, Oct. 16th, 1875), Dr. J. H. Hunt related such a “case of partial paralysis from reflex action caused by adherent prepuce,” in which, I must admit, I do not find, upon close observation, the proof of the postulation set forth in the title of the paper. The patient was a boy of six years, with a peculiar staggering walk, who seemed unable to properly control the lower extremities, and therefore was subject to frequent falls. He was nervous, and exhibited a twitching of the muscles of the face and extremities. His penis was found erected. He started and screamed in his sleep. The tongue, in the attempt of protruding, rolled about in the mouth. Articulation and intellect were below the average, in fact the boy was considered “idiotic” in the neighborhood. After circumcision, improvement took place rapidly.

The case yields but a few points. The boy’s intellect was below the average, so that people thought him “idiotic.” Besides, the child suffered from St. Vitus’ dance, in which all the voluntary muscles participated, according to the above history. If there is any sickness, where a child of six years can make the impression of stupidity or idiocy, it is chorea minor, with its utter want of control over locomotion or articulation, and its emotional aberrations. Thus nothing, perhaps, remains except a case of protracted chorea, which “improved rapidly after circumcision.” I ought not to go into a further criticism of the case, but I must say that no attempt has been made to explain the chorea by its habitual causes, and not even a statement is made of the condition of the heart, or of the spine, or concerning the previous occurrence, or non-occurrence, of acute articular rheumatism, or of the habit of masturbation so easily contracted when the phimosis is marked enough to prove an annoyance and irritation, and frequently given up when the source of constant irritation has been removed.

<sup>1</sup> C. Mauriac (Étude sur les neuralgies symptomatiques de l’orché-épididymite blennorrhagique, Paris, 1870) claims—but admits no more—that “no less than four per cent. have reflex *neuralgia*, spinal, lumbo-abdominal, intercostal, sciatic, crural or visceral.”

It is this habit of masturbation in the infant and child, to which I here desire to draw attention. To what extent it is practised in more advanced years, and how it interferes with a robust physical, mental and moral development of adolescence, is but too well known to both the physician and the pedagogue. But it has often appeared to me that its frequent occurrence in the quite young is by no means fully appreciated. There are some cases in which the diagnosis can be made from the condition of the parts when suspicion has been aroused. Tumefaction of prepuce and glans penis, and redness and œdematous appearance of the former, swelling of the labia majora, redness of the introitus vaginae, moisture of the labia and vagina from over-secretion of the glands of Bartholin and the muciparous glands surrounding the urethra, are sometimes conclusive, when orgasmic excitement has been observed. But these cases are by no means very frequent. Accidental or morbid swelling of the parts, and over-secretion from other causes may obscure the diagnosis. The majority of diagnoses can be made from careful observation of the child on the part of its attendants. A girl of three years was sent by a medical friend, because of a peculiar form of slight convulsive affections which were reported by the mother to have lasted a considerable time. These attacks would come in irregular intervals, frequently when the child was playing upon the floor or crouched upon a chair. Redness in the face and slight twitching about the eyes, with a deep sigh now and then, were all the symptoms enumerated. The child was well built, pale, the face was a little bloated. There was no history of a single violent eclamptic attack, or of any previous disease, or of premature teething and ossification of the cranial sutures and fontanelles, no history of epilepsy in the family, no effect of vermifuges and sedatives which had been administered. But for a long time the baby had lost her former spirits, was not noisy, but rather peevish and listless, alternately. A few questions and answers sufficed to convince me of the presence of masturbation in the little girl. She never had an attack of any kind when asleep. She never had one when walking about, or when playing and tossing about. She always had them when sitting down. She was very apt to keep her thighs closely joined, or to cross her legs. She moved and rubbed her limbs violently, got purple in the face,

began to perspire, to twitch about her eyes, which often looked excited, and lean back exhausted, sighing, or breathing hurriedly. To insure the diagnosis, the temperature of the rectum was taken and found normal; the urine examined, and no albumen found. Such cases are by no means rare. A number of them have been observed by me, which exhibited the same symptoms to a greater or lesser degree. Local changes of the parts are sometimes absent, although the habit be of long duration. The consequences, however, as far as general health is concerned are (*cæteris paribus*) the same. Amongst the prominent symptoms, beside the general expression of anæmia, and bloatedness, I ought to mention the change of temper. Not infrequently have I been puzzled about the presence of either masturbation or general anæmia from other causes, or premonitory symptoms of meningitis. The children are quiet, morose, fretful, peevish, sleep restlessly, or sometimes too soundly; appetite variable; bowels often constipated; pulse sometimes a little accelerated, frequently slow, and not rarely irregular; expression of headache round their brows; skin sometimes very dry, flabby, and lifeless, sometimes inclined to perspiration, sometimes shining with accumulated sebum. In children of from six to eight years I have now and then met with comedones, as in the adolescent, in whom they are a frequent accompaniment of masturbation.

A little boy of my acquaintance had frequently been noticed suddenly to leave his toys, walk through the room dreamily, sit down under the chandelier, stare upwards and commence a kneading exercise—by means of his two little fists—directed against his privates, which his nurse thought very cunning, indeed. The symptoms were described as about the same as those reported of the little girl mentioned above. In his case the penis had frequently been found erected, and continued to have that tendency until his vesical catarrh and frequent micturition were relieved.

The causes of masturbation, no matter whether considered as an acquired habit or a disease, are very various indeed. I have positive knowledge of cases in which the habit was contracted by the treatment of infants, both male and female, at the hands of nurses or servants. The pleasurable sensation experienced by the victim, on the genital organs being gently tickled and



excited, and when unruly, their greater manageableness resulting therefrom, caused a morbid desire leading to onanism in its most violent form. Protracted gentle rubbing, or beating of the gluteal regions in infants, as also in later years (6 to 10), of riding on horseback, I have seen followed by erection of the penis and orgasmic excitement. Pressure of the parts and high temperature are injuries. Thus act feather-beds and soft furniture; thus also warm and heavy first pantaloons, the delight of the boy and pride of the mother. They may prove hurtful from more than one cause. Not only are the parts kept too hot and are sometimes compressed, but the frequent micturition of the child necessitates the frequent and protracted handling of the penis. The young child is but clumsy and the reverse of adroit. It takes him time to disentangle the organ. Frequently in the streets and gardens have I seen sympathizing little friends, mostly of the other sex, and then somewhat older, or servants, busy with rendering the required aid in the emission of the urine. The good habit of washing young children in cold water is not always unattended with a certain degree of danger. There are some which are unduly excited by it at bedtime. In adults I have frequently observed, that, while cold washing of the whole body—genital organs included—would be attended with good results in the morning, it—or the cold shower-baths—just before going to bed would lead to excitement and nocturnal emissions. This observation has induced me to pay some attention to the possible ill effect of the same dietetic treatment, in the evening, in the cases of such children who were known as, or suspected of, being addicted to the morbid habit. Local irritation of the genito-urinary organs is amongst the most common causes of masturbation. Most of their morbid processes have amongst their principal symptoms peripheral hyperæsthesia, or pain in the glans penis, which, like many neuralgic affections, are partly relieved by handling or pressing, and thereby constitute the possibility of contracting a habit. Nor are affections in those organs rare. One of the first affections of the newly born is an anomaly of the kidney. The uric infarctus of the first few weeks is a fertile source of gravel or stone, both in the kidneys or bladder, and of nephritis. In fact the large majority of vesical calculi—not at all rare in the young—were renal first, and descended with, or without, accompanying symp-

toms from the renal pelvis. In six out of forty necropsies of children under a year, made in a certain period in the same public institution, I found renal calculi. Although this figure cannot by far be taken as anything like a true proportion, it proves, nevertheless, that stone in children quite young is not rare at all. Besides, the experience of specialists has always proven that the histories of vesical calculi date in many cases far back into early childhood. Nor is gravel by any means very rare. Many of the hard screaming spells of babies, so willingly attributed to flatulency, naughtiness, or "teething," find on close examination their easy explanation in the condition of the urine. Catarrh of the bladder is a very frequent affection; even a number of cases of incontinence of urine take their origin from that condition of the mucous membrane, with its subsequent effect upon the nerve distributions. Sometimes it is itself the progenitor of a ureteric or renal catarrh; more frequently, particularly in girls, it is the effect of a catarrh of the external parts. For leucorrhœa—from a number of causes—is a very frequent affection. In the very young, the presence of hardened vernix in the vagina and cervix, in the somewhat older, of decomposed urine, or foreign bodies, or exposure, or an accidental abrasion, result in a catarrh of the vagina, with all its harmful privileges upon the condition of neighboring mucous membranes and nerves. Generally, long duration makes it a disagreeable and uncomfortable affection, both in itself and its possible consequences. Urethral catarrh in the young male is rare, but balanitis and balanoposthitis are very frequent, and ready causes of irritation. Their sources are both evident and numerous. The prepuce is long and frequently narrow. It need not even be very long, but still its lower blade and the surface of the glans penis are apt to be moist with fresh or ammoniacal urine. When it is narrow, the facilities for that discomfort are the greater; when it is very much so, the prepuce will often be pouched out during the emission of the urine, and micturition be effected drop by drop. The sebaceous follicles of the inside of the prepuce are very large—as they are all over the embryo and very young infant—and smegma very copious indeed. Thus its decomposition and the formation of irritating fat-acids are but the matter of a short time. If I add to all this the sometimes irregular attachment of the pre-

puce to the glans, with the formation of local pouches for smegma or urine, the number of possible or probable injuries appears large enough, without my believing in the alleged occurrence of severe nerve disorders depending on the sole presence of the embryonic or infant phimosis or epithelial conglutination of glans penis and prepuce.

The intimate correlation of the branches of the pudendal plexus (n. hæmorrhoidalis med. and inf. for bladder, vagina, musc. levator and sphincter ani ext. and int.; and n. pudendus for the pelvic cavity, with n. perinæalis for perinæum and mm. transversi penis, bulbo-cavernos, sph. ani. ext., scrotum, labia majora and vestibulum vaginae—and n. penis dorsalis for penis and clitoris) explains why abnormal conditions of the lower portion of the intestinal tract are amongst the frequent causes of genito-urinary irritation. The frequency of this connection is well known to those who have to deal with affections of the rectum or prostata in the adult. In the child the intestinal irritation is mostly of two kinds, consisting either in the presence of worms (often ascaris, more frequently oxyuris), or constipation, the frequent cause of seminal emissions in the predisposed adult. In regard to the effect of constipation, it matters not whether it be produced by incompatible food, or rachitical insufficiency of the muscular layer, or by the inflections of the (in the embryo and young infant) normally elongated and curved colon descendens. In all of these cases the effect upon the neighboring organs, particularly the nerves, is the same, and irritation of the glans penis can sometimes be explained by no better cause; for fissure of the anus, although not impossible, and sometimes met with, is rare.

The conditions of the urine, with its constant and direct effect upon the nerves of the urinary organs, ought not to be lost sight of in this connection; of the effect of stone, gravel, and mucus I spoke above. The changes taking place in consequence of improper food or medicines are no less important. Large quantities of meat, eggs, spice, salt, beer, are injurious. Cantharides and other irritants ought to be avoided or carefully handled. Alkaline salts are by no means indifferent. Most of them irritate the kidneys, some dangerously so in certain cases. Nitrate and chlorate of potassa and soda belong to that class, emphatically. Fatal cases of overdoses of the chlorates do not

appear in the latest text-books on *materia medica* and therapeutics, nor have I met with any in the journals. While, then, postponing the elaborate communication of such cases as not belonging here, I shall but say that they exist. Congestion of the kidneys with over-secretion I have observed frequently, nephritis with fatal termination once, and am cognizant of more cases. I believe I am justified, perhaps, in my impression, that the frequency with which kidney diseases in children have come to my knowledge during the last ten years may have its source in the indiscriminate use of the chlorates—amongst the public—for catarrhal and diphtheritic affections of the pharynx.

To sum up, I should say that all causes resulting in direct or indirect irritation of the nerves of the genito-urinary organs are apt to give rise to masturbation in the young. It is understood that I do not speak of the large class of cases occurring amongst school children and the inmates of institutions. From many of them the same tale is told of the prevalence of the dangerous habit amongst the children.

The treatment of infants and children affected with the morbid tendency is indicated by its causes. No uniformity exists in either. The nurse requires watching as well as the baby. Excessive phimosis, circumcision. Balanitis and balanoposthitis, cleanliness and astringents. Stones and gravel, mostly alkaline salts, the majority being uric in the beginning. Vesical catarrh, alkalies, tannin, cubeb, hyoseyamus, injections according to circumstances. Constipation, its appropriate treatment, dietetic, antirrachitical, roborant, laxative (injections). Worms, anthelmintics. The acquired nervous derangement, bromide of ammonium or potassium. Anstie administers the bromide of potassium rather in "fierce activity of mind and body" than in the effects of masturbation. My observations as to its beneficial effects are more favorable, however. Lupulin and camphor have proved very serviceable in my hands. Regulations as to feeding are self-evident after I have given a list of substances which prove injurious. Regular bathing and constant occupation under careful supervision are urgent requisites. The children must not be permitted to sit on the floor too long. When the symptoms of an attack exhibit themselves, take them up, and occupy their body and mind. Force is often required. They must not remain in bed after waking up; in some cases I had

them taken up from their sleep every morning, and kept them awake all day, so as to secure sound and uninterrupted sleep all night. Infibulation, as advised by Celsus, might be replaced by an artificial sore of the surface of the penis. At all events, there are many cases which exert to the utmost both the watchfulness of the attendants and the ingenuity of the medical adviser.

Important amongst the therapeutical indications are those referring to the general influences produced upon the whole nervous system by the constant irritation of a large number of peripheric nerves. The symptoms of irritation are soothed and relieved by the above-mentioned sedatives; those of masturbation, and exhaustion resulting therefrom, by a general roborant treatment and nerve tonics, amongst which I place strychnia foremost, iron and arsenic next. The affections in which they are principally indicated, are neuroses, either of the nerve centres, such as epilepsy and chorea magna, or of a peripheric nerve, or a number of nerves, or nerve complexes. The form in which peripheric nerves are generally affected, is that of hyperæsthesia or neuralgia, terms which are not used as identical, because medical men have agreed to employ the latter, where the sensations are changed for a longer term, or where a positive lesion can be detected in the nerve itself.

Neuralgias are seldom treated of amongst the nervous diseases of infants and children, although the inherited element in the etiology of neuralgia, amongst adults, is acknowledged to be very powerful, indeed. Thus Anstie found twenty-four out of a hundred cases of neuralgia depending upon hereditary transmission. A number of causes resulting in neuralgias, it is true, are not found at the age of which I am here treating. Thus a number of lesions belonging to advanced age, such as atrophy, tumefaction, hyperæmic condition of the neuroglion, visible dilatation and elongation of the blood-vessels, cicatrices and indurations, tumors; the abuse of alcohol, of changing climates, of various traumatic and mechanical influences, are out of the question. Nor are the causes so much emphasized by Anstie, such as an excess in religious training, and compulsory employment of the mind with transcendental subjects, to be accused of interfering with the healthy development of the nerve-tissue of early childhood. But there are a large number



of young children, in whom a neuropathic disposition is well marked. Hereditary influences, acquired diseases of the bones, so much more frequent in the young than in the old, and even irritation and exhaustion by masturbation, play an important part. If close observers, such as Valleix or Eulenburg, have but a small percentage of neuralgias amongst the young, it is because the class of patients they had to deal with were not of the mixed character yielding accurate and complete statistics. For cases of neuralgias are by no means rare; particularly hemicrania and spinal irritation are frequently met with.

Hemicrania is by no means rare. In girls and boys, particularly the former, I have often observed it. The variety which has been called angioparalytic, and depends upon, or is complicated with, enlargement of blood-vessels, may not be frequent. In fact I have seen but very few cases under ten years, in which that symptom required, and was benefited by, generous administration of ergot. But the angiospastic variety, with contracted blood-vessels, pale face, and generally neurotic appearance, is really frequent. It results from a number of causes. It is one of those forms of nerve trouble which is inherited perhaps as frequently as any other. Neurotic mothers, both thin and fat, are liable to transmit it to their young daughters. Congenital chlorosis, consisting in anomalous smallness of the arterial blood-vessels, is a frequent cause. Observation through many years of a permanent practice has yielded many cases, in which I followed children from early childhood to adult age. Their congenital condition resulted in nervousness and trigeminal neuralgia in the child, and chlorosis and hysteria in the young woman, with a bad prognosis until the climacteric years will put a partial end to the consecutive symptoms. Some cases were the result of anæmia and masturbation, some depended upon malaria, or slow convalescence from acute diseases, or protracted intestinal catarrh. But the worst and earliest cases I have seen resulted from masturbation. The children, both male and female, look anæmic, some thin, some bloated, are fretful, and changeable.

The attack of trigeminal (mostly supraorbital) neuralgia was sometimes unilateral, sometimes bilateral, and could last

for days, and amount to such agony in some instances, that the suspicion of cerebral disease was aroused. In a few cases the suppression of masturbation was the principal promoter of recovery. In others medicinal treatment proved effective, where the bad habit had resulted in a thorough lowering of the substance and function of the nerves. Iron administered for a long period, together with a roborant diet, belladonna in frequent and small doses, nitrite of amyl (inhaled during the attacks, as in the spastic hemicrania of adults; not internally, where I have found it absolutely inert) and protracted application of a mild galvanic current will prove successful. I should not leave this subject, however, without mentioning the effect of strychnia in speedily restoring the impaired nerve power, provided the doses are not too small, and the mode of administration the appropriate one. A child of five years ought not to take less than  $\frac{1}{24}$ th part of a grain in the course of a day, of either the sulphate or nitrate. Larger doses are frequently not only tolerated, but required. The best mode of its administration, however, is not by the mouth, but subcutaneous. A single daily dose of a twentieth part of a grain of the sulphate of strychnia in water will fully suffice.

Its beneficial effect upon the nerve-centres explains its power over the other form of neurosis not infrequently found in children—"spinal irritation," a term which for thirty years had an ontological existence in our vocabulary. After it had been given a bonâ-fide place amongst elementary ailments, modern medicine has ceased to look upon it as a unit. There are cases in which it is found to be due to a simple irradiation from cardialgia, others in which it depends on contagious and miasmatic diseases, typhoid and intermittent fevers; or on dyscrasic conditions, such as tuberculosis or scurvy; or on deficient sanguification, from anæmia, hydræmia, or smallness of arteries; or on venous obstruction superinduced by diseases of the lungs, heart, or abdomen; or on congestion of the spinal cord and surroundings depending upon improper innervation; or on anæmia of the spinal cord; or on a neuralgia of the skin, or muscle, or bones, or meninges. It is evident, from the long list of causes, that the cases of "spinal irritation" must be more frequent in the adult than in the young. But in these, this form of neuralgia

is by no means rare. Every cause tending to lower the status of the sensitive nerves of skin, muscle, spinous processes, or spinal meninges, will result in permanent hyperæsthesia, or local neuralgia. Where the presence of other causes of exhaustion in the child—such as enumerated above—can be excluded, the suspicion that the incompetency and faulty function of the sensitive nerves are due to masturbation, will frequently be well founded.

(To be concluded in the next number.)

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THE HISTORY OF CASES IN WHICH GESTATION AND PARTURITION HAVE BEEN INTERFERED WITH BY  
UTERINE FIBROIDS.

By T. GAILLARD THOMAS, M.D.

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THE existence of fibrous tumors of the uterus of any of the three varieties, submucous, subserous, or interstitial, interferes with conception, but no variety of such growths entirely prevents it. When utero-gestation and parturition are complicated by the presence of large fibroids, several unfortunate results may be the consequence. My experience furnishes me with cases illustrating the possibility of the following:—repeated abortions, dangerous post-partum hemorrhage, repeated malpresentations of the fœtus, obstruction of the outlet of the uterus, and obstruction of the pelvis. Some of these I here record.

CASE I.—*Repeated abortions produced by the pressure of a submucous fibroid.*

Mrs. B., a perfectly healthy woman, who had suffered from no physical ailment before her marriage except menorrhagia, sent for me on account of a threatened abortion. She gave me the following history: she had been married three years, and during that time had had two abortions about the end of the third month without assignable cause. She had now for the third time advanced to the same period, and, although she had kept very quiet, symptoms of abortion were again

showing themselves. In spite of the resort on my part to the usual remedies for this accident, the uterus soon cast off the foetal mass, and I supposed that the case was at an end. On the next day, however, a very free sanguineous flow taking place, I discovered, upon vaginal touch, a round mass presenting at the os, which I took for a portion of a second child. Traction upon this delivered a fibrous polypus, the pedicle of which I twisted off.

This polypus was as large as a small hen's egg, and appeared to have been the cause of former menorrhagia and the more recent abortion, since both of these supposed results disappeared after its removal. It had, in all probability, existed for years as a submucous fibroid, and gradually had become extruded into the uterine cavity and pediculated.

CASE II.—The second of the accidents which I have mentioned is very well illustrated by the following unfortunate case:

In the year 1861 I was called in great haste, by two prominent physicians of this city, to Mrs. B., of whom the following history was given to me. She had been delivered, seven hours before I saw her, of a male child, after a somewhat tedious labor, but one which did not require the assistance of art. During delivery of the placenta, the attending physician noticed the presence of a uterine fibroid situated near one horn of the uterus, and as large as a cocoanut. After the delivery of the placenta, some difficulty was found in causing firm, tonic uterine contraction, but in an hour and a half the physician felt warranted in leaving his patient. In two hours from that time he was called to her on account of active post-partum hemorrhage. At once calling to his assistance a brother practitioner, they had employed intelligently and systematically all of the hæmostatic means generally resorted to under such circumstances. Their efforts had, however, proved ineffectual, and the hemorrhage had steadily continued until I saw her.

Upon my arrival, I found the patient very much exsanguinated, and in a condition of profound syncope from which she was roused with difficulty. Within twenty minutes after my arrival at the house she died. Among other efforts which I made, I carried a lump of ice to the fundus uteri, passing it

over the walls of the uterus, and irritating them with my fingers. In doing this I discovered jutting into the uterine cavity the large interstitial fibroid to which allusion has already been made. I at first supposed this to be partial inversion of the uterus, but conjoined manipulation readily proved this idea to be erroneous.

This case I have always looked back upon as one which would have been peculiarly suited for the practice of the intra-uterine injection of the persulphate of iron, which has been suggested since the period of its occurrence. This remedy I look upon as one which should be employed with the greatest circumspection, and only in cases which have proved uncontrollable by methods which are ordinarily effectual and attended by less danger. In the case just related, however, had I known of it, I should not have hesitated to employ it, as I have several times done in cases of desperate character with the happiest results.

It is probable that a large uterine fibroid, bearing the relation which this did to the uterus, results in hemorrhage by interfering mechanically with firm uterine contraction and ligation of vessels at the seat of the placenta. Hemorrhage once being established it is difficult to make a uterus, even not thus encumbered, contract with a normal degree of power; much greater would this difficulty be in the case of one whose uterus was invaded by one of these neoplasms.

CASE III.—*Repeated breech and footling presentation, due to the presence of two large fibroids near the fundus uteri.*

Mrs. N., aged 23, who before marriage had been for many years under my professional care, sent for me three months after marriage, and gave me the following history. While walking in the streets at night, holding her husband's arm, she had struck her foot against an inequality in the sidewalk and been thrown suddenly forwards. She saved herself from falling by a violent effort, and after the occurrence was taken with very severe pelvic pain, which existed at the time I saw her.

Upon palpating the abdomen, I discovered over the uterus a tumor as large as the head of a child of twelve months of age. It was movable, sensitive to pressure, but not susceptible of



careful examination, as pelvic peritonitis of acute character existed.

As soon as the peritoneal inflammation passed away, which it did very slowly and after creating a great deal of suffering, I examined the tumor with care, and found it to be a large interstitial fibroid. It had in all probability existed long before I discovered its presence, and it is likely that the sudden succussion given it by the fall had broken adhesions which it had formed with the pelvic viscera, and, from this, peritonitis had ensued.

Very soon after her recovery from this attack Mrs. N. became pregnant, and I attended her in labor. The child presented by the breech, and it was with great difficulty that I delivered it alive. I was assisted in this by Dr. H. F. Walker, and very fortunate was it that I had his aid, for before the delivery of the placenta a very severe hemorrhage came on. This was soon controlled by manual delivery of the placenta, and the introduction of ice into the uterine cavity, and Mrs. N. entirely recovered.

It is highly probable that the two chief factors which result in the accommodation of the breech of the child in the fundus uteri and the head in the cervical zone, are, first, the fact that the reflex movements of the child, excited by contact of the delicate surfaces of its feet with the uterine wall, cause it to accommodate its largest part to that part of the uterus which gives it most room; and second, that the more capacious fundal region more readily receives the largest extremity of the foetal ovoid, which is the breech with the thighs and legs folded upon the abdomen. Thus the unborn child, father to the man, endeavors to make the best of the circumstances in contact with which he is called upon to live.

Upon passing my hand into the uterus of Mrs. N. to remove the placenta, I was struck by the fact that the largest portion of the uterine cavity was, on account of the tumor above, unquestionably that situated just above the cervix, and that in this, the largest portion of the foetal ovoid would be most readily accommodated. Upon this, I predicted that the subsequent labors of this lady would be marked by presentation of the breech or feet. Mrs. N. bore two more children; both presented by the breech, and both children were delivered alive.

After the first delivery post-partum hemorrhage was so carefully guarded against, as the child passed out of the pelvis, that it did not again occur.

CASE IV.—*Obstruction to the outlet of the pelvis. Delivery by manual means.*

I was called by the late Dr. James L. Brown to see Mrs. M., a woman belonging to the laboring class, who, after having borne four children, was now in labor with the fifth. Since the birth of her fourth child a large fibroid had developed in connection with the uterine horns, which had filled the whole pelvis and obstructed it so that the child could not pass.

Upon careful examination I found that I could, by the application of great force, so far push up the tumor as to render the passage of a dead child possible. I say a dead child, because through the narrow channel existing between the tumor and the symphysis pubis, it was manifest that the delivery of a living one would be entirely impossible. It being decided to sacrifice the child for the sake of the mother, I proceeded to deliver. The child presenting by the breech, I pushed the tumor firmly up and brought down the feet; with considerable effort I delivered as far as the head. This part would not pass, and fearing that it would be torn off and left in situ, I passed up the perforator, and after infinite trouble and the expenditure of much time I succeeded in passing it through the palatine arch, diminishing the size of the head and delivering.

The woman recovered without an unfavorable symptom, and I have never heard of her since.

CASE V.—*Utero-gestation advanced to the fourth and a half or fifth month. Abortion produced on account of complete pelvic obstruction.*

Mrs. K., of Paterson, N. J., called to consult me by the advice of Dr. Joseph M. Turner, of Brooklyn. She had for a length of time been under the care of Dr. Turner for a large uterine fibroid which filled the cavity of the pelvis, lifting the cervix uteri up to the level of the upper edge of the symphysis pubis and extending upwards a little above the umbilicus. It

was so amalgamated with the uterus that this organ could not be distinguished from it by conjoined manipulation, and the elevation of the uterus was so great as to render the use of the uterine sound impracticable. Mrs. K. had been married several months, and at the time that I first saw her was in the third month of pregnancy. The question for the decision of which Dr. Turner had referred her to me, was this: Whether, in the obstructed condition of the pelvis, it would be safe to let the process of gestation continue? As there was no immediate necessity for interference, I advised that the case should be kept under observation; that if the tumor rose from the pelvis and the cervix advanced into it, gestation should be allowed to proceed; while on the other hand, if the uterus ascended as it increased in size, and the tumor, instead of leaving the pelvic outlet freer, obstructed it more completely, I recommended that an abortion should be induced.

Mrs. K. after this left Brooklyn, went to Paterson, and was not again seen by Dr. Turner until the fourth and a half or fifth month of pregnancy. At this time he was very much startled to find that the uterus had risen so high and the pelvis become so completely obstructed by the tumor, that he was entirely unable to touch the os. Under these circumstances it was decided that Mrs. K. should at once remove to Brooklyn, and that I should see her in consultation with Dr. Turner, in the hope that under the influence of an anæsthetic I might be able to reach the os, and produce an abortion.

Accordingly we met in Brooklyn, Drs. Henry C. Turner and James B. Hunter being likewise present. By placing the patient on the left side and pressing upwards very firmly through the narrow channel left between the tumor and the pubes, I succeeded in touching the os, introducing an elastic whalebone probe into the uterine cavity, and finally in leaving a tent in the cervical canal.

In forty-eight hours labor came on, and Dr. Turner succeeded in delivering a child nine inches in length, through a channel, which seemed to the touch entirely inadequate to its passage. The placenta was brought away by traction and pressure, and the mother recovered.

In this case, had abortion not been produced, Cæsarean section would have been the only hope for the patient.

CASE VI.—*Obstruction to the outlet of the uterus by a large sessile fibroid. Delivery accomplished by Cæsarean section.*

I was called on the 16th of March, 1874, by Dr. H. T. Hanks, to see Mrs. M., the wife of a laboring man, about thirty years of age, and the mother of five children, her last two labors having been complicated ; in one the funis and arm presenting, the other requiring version. Her youngest child was four years of age, and since its birth a large uterine fibroid had developed itself in the lower segment of the uterus. Several months previous she had consulted me at my clinique at the College of Physicians and Surgeons, when the diagnosis of a fibrous tumor situated in the right anterior wall of the cervix uteri was made, and she was warned of the dangerous consequences which would result from it if she became pregnant, and had been urged in case of such an occurrence to make a report of it at once to her physician. This she neglected to do and Dr. Hanks was called to her only after labor had set in.

I saw her after she had been in labor several hours. The waters had flowed away five and a half hours before, the labor pains were becoming feeble, the pulse and temperature were good, and the funis was hanging cold and pulseless between the woman's thighs. The fœtal heart had ceased to beat some time before. Upon examining by touch, the cervix uteri was found occupied by a very large and hard fibrous tumor, which was attached low down along the posterior and lateral walls of the lower segment of the uterus, closing the canal except along the anterior wall. Through the small opening here existing two fingers could be passed, after the whole hand had been carried into the vagina (the patient being anæsthetized), and the head of the child be touched. The head was considerably elevated above the brim of the pelvis, and even after introduction of the hand into the vagina, could be touched with difficulty.

Upon consultation with Drs. Hanks, Clark, Bullard and Jones it was decided to make trial first of version, and then in case of failure of craniotomy and embryotomy. It was found impossible to deliver by either of these procedures, and Cæsarean section was practised as a last resort.

The patient, under the influence of ether, was put upon a table before a window admitting a strong light, and an incision made down to the uterus. As soon as all flow of blood from this was checked, the uterus was incised and the child rapidly and easily delivered. The placenta likewise was withdrawn through this opening.

Every one present was struck by the ease and rapidity of the removal of the child, and the fact that had it been alive at the time of operation no cause for its death would have been found in the surgical procedure.

After removal of the child and placenta, the uterus was by pressure and the hypodermic use of ergot forced into firm contraction, and the incision in its walls carefully closed by interrupted silver suture. No fluid or blood having entered the peritoneal sac, the abdominal wound was immediately closed by silver suture and the patient put to bed. The child was removed in just three minutes from the time the first incision was made, and the wound closed and dressed, and the patient in bed in twenty minutes from the time of commencement. She passed a very comfortable night, being kept free from pain by morphine, a physician being constantly in attendance. On the following morning she was perfectly comfortable; the mind was clear; and no bad symptoms presented themselves until twenty-four hours after the operation, when acute peritonitis developed itself with great suddenness and violence, and after a duration of two days destroyed the patient's life.

While closing this article a note reaches me from Dr. I. M. Heard, of West Point, Miss., relating so curious a case bearing on this subject that I append it.

"On the night of Nov. 18, 1874, at the request of a friend, I was called in the Prairie to see a negro woman, aged about thirty-five years, in labor. I found two negro midwives in attendance, who reported that the woman had been in labor for twenty-four hours, and that her womb was down. On examination I found that a large fibrous tumor (as large as an ordinary child's head) had been delivered. I found it attached to the anterior lip of the mouth of the uterus by a firm, broad attachment (one by two inches). The os was dilated and the child's head presented naturally. I immediately drew



off a very large quantity of urine, being compelled to use a male catheter, owing to the situation of the tumor. The pains were coming on regularly, but not with sufficient force to expel the child, which was not in an unusual posture. The bowels were very much constipated, and I gave directions for their evacuation; and being very tired, and not well, I made myself as comfortable as circumstances would admit on the floor before the fire for the remainder of the night. Next morning (the bowels having sufficiently moved) the patient being in the same condition as regards labor, I gave her ergot freely, and a living healthy child was delivered, the tumor occupying a position immediately in front of the pubes. After delivering the placenta, and allowing a suitable time for the patient to rest, I carefully oiled the tumor, and reduced it to its original position in the pelvis.

“The third day, being called in the neighborhood, I found my patient up with her baby, and attending to her usual work (doing nothing), and saw she felt no inconvenience whatever. Up to this date (about ten months afterwards) she has continued very well. She reports that she discovered this tumor fifteen years ago, and that this was her third child since its appearance, but it never came down before. She also states that she has been treated by midwives repeatedly for prolapsus, and has used pessaries for its relief.”

The Doctor, in a subsequent communication, describes the tumor as irregular in shape, of the size of a child's head, attached to the cervix uteri by a broad and strong attachment. While the child was being delivered the tumor was pushed upward, and rested upon the pubes.

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Fig. 3.

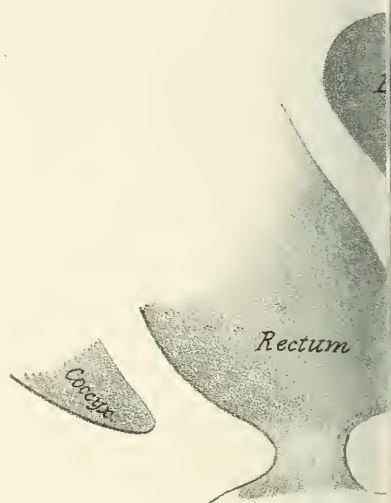
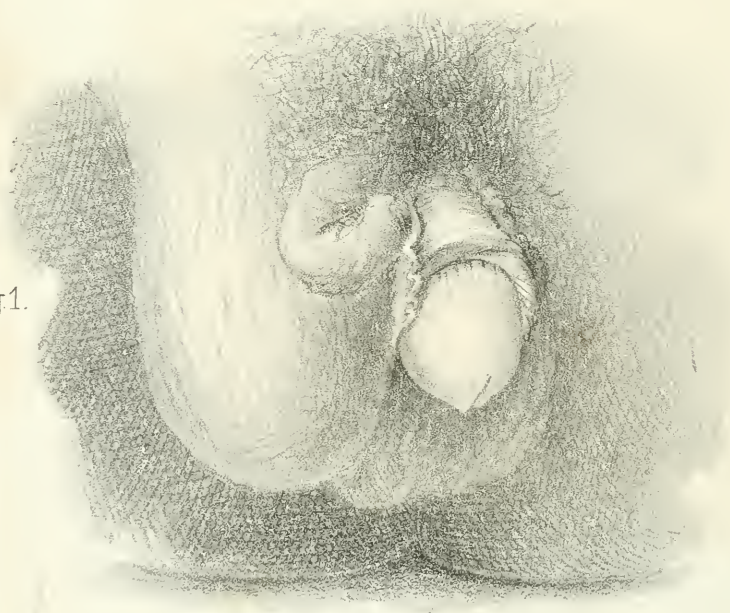


Fig1.



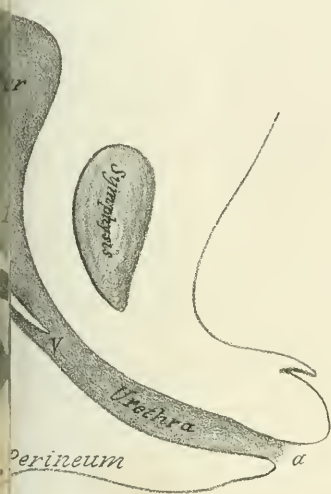


Fig. 4.

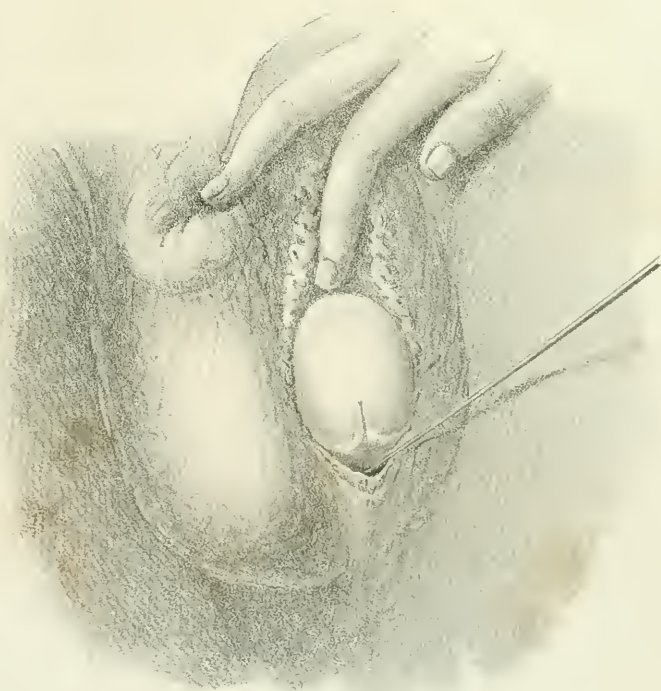


Fig. 2.





## A CASE OF PRESUMPTIVE TRUE LATERAL HERMAPHRODISM.

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Reported by

PAUL F. MUNDÉ, M.D.

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(With 1 Plate.)

At the meeting of the New York Obstetrical Society, held October 5th, 1875, I exhibited before the Society an individual in male attire, who claims to possess—with what right it is partly the object of this paper to show—the characteristics of both the male and the female sex united in his person, and to be a unique instance of that anomaly known as *hermaphrodisia vera lateralis*, true lateral hermaphrodisism (one lateral half of the body containing the germinal gland of the female, the ovary, and presenting female peculiarities; the other half containing the germinal gland of the male, the testicle, and showing male attributes).

Inasmuch as this individual has, during the last ten years, made the tour of nearly all the universities and medical centres of Europe, and has been examined with greater or lesser minuteness by hundreds of physicians and naturalists, whose opinions have differed more or less on the validity of his claims; inasmuch, further, as the long familiarity with the scientific interest of his malformation has given the individual a certain routine and a readiness in apparently adapting his answers to the questions asked him, which savors strongly of “clap-trap” (although, in my opinion, not of that character); lastly, and principally, because the case, if really at some future day proved to be what it claims, is the only instance of the kind thus far met with in the living human being, and deserves careful recording, which has never been done in the English, and only up to a certain period in the German language—for all these reasons, therefore, I have thought it worth while and not devoid of value to science and interest to the profession, to collect the occasionally conflicting items of the person's history, partly from my own previous acquaintance with the case in

Germany, but chiefly from his own statements and the numerous certificates in his possession, and to place on record a connected, lucid, and impartial account of the whole case. In doing so I have compared the present statements of the individual with the account of the case given in the publication above referred to by Dr. O. von Franqué, in *Scanzoni's Beiträge*, Vol. V., 1867, since which time the case has entered into an entirely new phase.

Catherine Hohmann was born in Mellrichstadt, Province of Lower Franconia, Bavaria, in 1824, and spent the first 46 years of her life as a female, dressed in the clothing, and occupied in the vocations of her supposed sex. After her twelfth year the genital organs began to develop and the pubes to be covered with hair, and, as is usual with girls at puberty, the breasts commenced to enlarge and develop. Soon after, sexual propensities, directed entirely towards the male sex, gradually manifested themselves, which, in her seventeenth year, she began to gratify, after the manner of girls of her class in Germany, by taking unto herself a lover, with whom she cohabited for over twenty years. There being no vagina present, the attempts at copulation were, of course, very unsatisfactory; still they were accompanied by the discharge of a thin viscid fluid from the urethra, the emission of which always coincided with the orgasm, and was not attended by erection of the clitoris. Lascivious thoughts, however, would be followed by this same emission. She expressly states, that sexual excitement always brought on a peculiar thrill or glow on the *left* side of the pelvis, the significance of which statement will appear during the anatomical description.

In her nineteenth year a discharge of blood took place from the urethra, which returned at irregular intervals for some time, and finally reappeared every three to four weeks, lasting freely from three to six days. This periodical sanguineous discharge was considered by H. and her companions to be menstruation, and was preceded by the usual menstrual molimina, such as tumefaction of the breasts, easy erectibility of the nipples, and between the twentieth and thirtieth years the secretion of a colostrum-like fluid from the latter.

After the advent of menstruation the relations of H. with the male sex in general, and her lover in particular, remained

about the same as before. As she approached her fortieth year, however, the menstrual flow gradually decreased in duration to two days, the discharge of colostrum from the breasts at the monthly periods ceased entirely, and Hohmann, while still enjoying her intercourse with her lover, began to feel an increased pleasure in female society. At the same time, often during lascivious dreams, a copious emission of fluid would take place from the urethra, preceded by a turgescence and elongation of the clitoris (or penis), which fluid emitted a peculiar odor, and differed entirely from that discharged before the advent of menstruation, or during copulation with her lover in former years.

The first distinct appreciation of sexual propensities for the female sex came to her in 1860, when sleeping with a girl in her employ during a winter night after a dance, and she then and there made her first exceedingly imperfect attempt at sexual intercourse in her capacity as a man. The erection of the penis was very slight, and its introduction, even between the labia, as yet out of the question. Gradually, however, the erectibility of the organ increased, until it measured during erection nearly 3" in a downward curve, and H. for several years enjoyed the enviable faculty of being both lover and beloved (*amant et aimée*).<sup>1</sup>

The dubious condition of her sexual status was not known to competent authorities until 1863, when a supposed crural hernia on the left side induced her to consult Dr. Reder, a physician in her native town, who first discovered the anomaly, and

<sup>1</sup> The account given by Dr. von Franqué (l. c.) of the early sexual proclivities and propensities of H., differs materially from the story told by her now. Franqué says, that her early tendencies were all towards the female sex, that seminal emissions commenced between fifteen and sixteen, and that she performed coition with women as early as in her seventeenth year. After the occurrence of menstruation, her affections became more inclined towards the male, but still remained predominant in favor of the female sex. H. explained this discrepancy to me, by stating that when Dr. von F. was taking down her history he offered her so small a sum (2 florins) for allowing herself to be sketched, that she became disgusted and refused to let him interview her again. Thereupon he sent a student to gather the facts from her, who must have misunderstood or misrepresented them. At all events, when in Würzburg, in 1873, she heard F.'s article read to the students as descriptive of her case, and at once interrupted the reading, and corrected the report as given by me above.

endeavored to induce her to submit to a thorough physical examination. This motives of modesty (long since lost) frustrated for several years; all Dr. Reder could do was to prevail upon her to supply him with a specimen of the viscid fluid, which she alleged (under oath) passed from her urethra during sexual excitement. This fluid was sent to Dr. Vogt, city physician of Würzburg, for examination, who found it to contain numerous spermatozoa. Finally, in the fall of 1866, Hohmann concluded to present herself to the medical authorities at Würzburg for a thorough examination.

She was first seen by Prof. von Recklinghausen, who, on examining her, found blood oozing from her urethra, for, as she states, she "happened to be menstruating when she came to Würzburg." Having been informed by Dr. Reder of the undoubted ejaculation of seminal fluid from the same urethra, Recklinghausen was naturally much interested, and referred her to Scanzoni and Kölliker for further investigation. After keeping her under close observation in the Lying-in Hospital for two months (the personal supervision of the case being exercised by Dr. von Franqué, who kept her in almost solitary confinement to prevent all chance of deception), these three professors made the following report of her sexual condition:<sup>1</sup>

"The person baptized by the name of Catherine Hohmann, of Mellrichstadt, is an exceedingly interesting case of hermaphroditism. The external genital organs have generally a masculine type: on the right side a distinct testicle and scrotum, a penis with moderate hypospadiasis, a urethra  $3\frac{1}{2}$ " in length; in the left inguinal region a soft, apparently somewhat lobulated body, which it is very questionable whether to consider as a second testicle; a body situated to the left of and behind the penis is likewise too soft and indistinct to be taken for a normal testicle. From above the root of the penis descend two sinuous folds of integument, which pass behind the corona glandis to the hypospadiac frenulum, which has two gaps at its point of insertion. The breasts are of female type, fully developed, as well as the nipples and areolæ, the latter studded

<sup>1</sup> All the reports and opinions of European authorities on the anatomical peculiarities of H., unless expressly stated as communicated by herself, are taken from a book in her possession, containing the autograph certificates of the gentlemen giving the opinions.

with hairs. A distinct beard is present. An examination per rectum did not reveal a rudimentary uterus. Should it be proved that neither of the two tumors perceptible on the left side are male sexual glands, this case might possibly be one of the rare cases of so-called lateral hermaphrodisism. At all events, the observation that male and female functions are both present is of the highest interest. A fluid taken from her in 1863 and examined by Dr. Vogt, showed the presence of spermatozoa; we, the undersigned, repeatedly observed the discharge of blood from the urethra, lasting two days, and presenting, by means of the perfectly fresh character of the corpuscles and the admixture of mucus, a menstrual appearance."

That this discharge of blood from the urethra was real, and not accidental, or the result of intentional deception, is vouched for by Dr. von Franqué, from personal observation. He states (l. c., p. 64) that on Jan. 23d, 1867, while H. was in the Würzburg Lying-in Hospital, epistaxis set in, which generally preceded the menstrual flow with her; the breasts tumefied, although no colostrum could be expressed from the nipples, as formerly; headache and general *malaise* were present; these molimina lasted three days, until the morning of the 26th, when the blood began to flow in drops from the urethra, coagulating in the adjacent hair (formerly the discharge of blood was more profuse). This urethral sanguineous discharge was witnessed twice during her stay in Würzburg. An occasion for proving the emission of normal sperma was also afforded Dr. v. F., for, as usual, a discharge of semen followed about a week after the cessation of menstruation (the fluid being caught in a cup by H.), which emitted the specific smell, and showed the microscopical characters of that fluid. I was at that time attending Scanzoni's clinic, and remember perfectly, not only the general appearance of the person, in the coarse dress of a Bavarian peasant woman, with the language and manners of her class, but also her presentation in the clinic on two separate occasions, at intervals of about four weeks, with the blood oozing from her urethra, from the mucous membrane of which passage it doubtless was exuded, there being no uterus of appreciable size present for the purpose.

Von Franqué answers the question, whether this hemorrhage is really to be considered menstrual, decidedly in the affirma-



tive; the symptoms preceding it, the manner of its appearance, its regular typical appearance during 22 years, would stamp it as unquestionably menstrual; whether it is to be considered as the external symptom of ovulation, however, can only be determined by the post-mortem discovery of an undoubted ovary.

The ejaculation of seminal fluid, and the more or less regular discharge of what appeared to be menstrual blood, were thus placed beyond doubt, for both these functions were demonstrated by ocular inspection. Where the semen came from was evident enough; the unquestionable presence of a testicle in the right scrotal sac answered that query. But where the organ was situated from which proceeded the stimulus resulting in menstruation—the ovary—that point was by no means decided.

From Würzburg Hohmann went to Heidelberg, and spent some time on the clinic of Prof. Friedreich, whose description of her case is so clear and minute that I cannot refrain from translating it literally:

“The person from Mellrichstadt, baptized Catherine Hohmann, was observed by me during four weeks in my clinic, and I had the opportunity to convince myself not only of the occasional emission of normal semen, but also of the periodical discharge of blood from the urethra. The body in the right integumental sac (which presents evidences of cremasteric action) is doubtless a testicle, as is also indicated by the peculiar sensation of pain experienced by H. even on light pressure. Probably the body in the left inguinal region is also to be looked upon as an atrophic testicle, since the existence of lateral hermaphroditism has by no means been indisputably proved in the human being, and also because, if ovulation really induced the periodical hemorrhage, a tumefaction or tenderness of that body would be perceptible. The body situated in the lower and posterior portion of the left fold of integument is too indistinct to be looked upon as a sexual gland; it is probably nothing more than an accumulation of adipose and cellular tissue (hyperplastic gubernaculum?).

“With the exception of the hair of the head and the mammæ, nearly the whole formation of the body bears a masculine type. Thus the larynx, muscular development, thorax, pelvis,

etc., are decidedly masculine; the type of inspiration is diaphragmatic. The existence of a short urethra also tends to place it in the male category, since the hypospadias is not total.

"With a sound I was able repeatedly to discover a pocket-like cavity at a distance of 1" from the urinary meatus, in which cavity the finger in the rectum could easily distinguish the point of the sound. The introduction of the sound was easiest when the penis was held upwards, stretched, and the sound passed along the posterior surface of the urethra. There seems to me no doubt, therefore, of the presence of a thin-walled uterus masculinus.

"I am of the opinion, that C. II. is an instance of male *hermaphrodisia transversalis*, to which the occurrence of periodical hemorrhages, which are to be called pseudo-menstrual, imparts unusual interest. The possibility of its being a case of *hermaphrodisia lateralis* cannot, however, be entirely excluded."

Prof. Dohrn of Marburg, which university II. next visited, positively declares her to be a man "because the characteristic criteria of the male sex are present." Whether a functioning ovary is also present can only be ascertained post mortem; Dohrn thinks it improbable, however, because the existence of lateral hermaphroditism has not been proved in man, and the condition and observation of II. do not offer sufficient evidence for such an assumption.

Hohmann having now reached her 43d year and approaching the climacteric, only three more menstrual epochs appeared at irregular intervals. After visiting Leipzig, Halle and various other German universities without meeting with any more positive information as to her supposed sexual duality, she proceeded to Jena, where Prof. B. S. Schultze made the most thorough examination to which she had yet been subjected, and arrived at the first approximatively satisfactory conclusion as to the exact condition of her internal genital organs and the location of a body which might, without too great a stretch of imagination, be considered to be the long and eagerly sought-for ovary, the cause of the hitherto unexplained periodical sanguineous discharges. Owing to the menopause, however, he was unfortunately not able from personal observation to add this important symptom to the valuable results which he ob-

tained with the finger in the rectum. I translate his report verbatim from his autograph certificate in possession of H., and also reproduce the diagrams made by Schultze (Figs. 5 and 6):

"Hohmann has a urethra of female dimensions, 3 centimetres (Fig. 5, *a b*). At the neck of the bladder *b*, there is not a trace of a prostate to be discovered. Somewhat towards the left side, at *v*, in Fig. 5, is the opening of the female genital canal which is 1.5 centimetres in length, *v u*; the urethra terminates by a fissure in the penis, which is of a hypospadiac character and is surrounded by a closed corpus cavernosum (bulbus urethræ). The bulbo-cavernosus muscle seems to be shaped somewhat like the constrictor cunni. The uro-genital canal appears wider, certainly more dilatable than in a man. The female genital canal will admit a good-sized male catheter, but the operation is not always successful; this canal terminates towards the left of the urethra in a button-shaped expansion (Fig. 5, *u*), the fundus uteri, from which towards the left wall of the pelvis a cord, tuba, is given off. This terminates to the left of the rectum in a somewhat movable cylindrical body several cubic centimetres large, which is sensitive to the touch, and is probably the left germinal gland (*Keimdrüse*.) The above diagram (Fig. 6) from the entrance of the pelvis, shows the relative positions: *p*, promontory, *h*, urethra, *u*, uterus, *k*, germinal gland. The pendulous glandular body on the right side is, as all authorities agree, a normal testicle, secreting normal spermatozoa. The empty sac of an obliterated crural hernia, can be observed on the left side. Whether the body I have described above as occupying the site of the ovary, and have called the left germinal gland, is a testicle or an ovary, can only be determined by a post-mortem examination.

"If truly a regular *spontaneous* loss of blood has occurred, ovulation may logically be inferred; but as I have neither from personal observation, nor from inquiry, been able to establish the fact of these periodical bleedings, I think there is considerable doubt as to their spontaneous character, because if the said left germinal gland is an active ovary, Hohmann would be the first and only case of real lateral hermaphroditism in a human being."<sup>1</sup>

<sup>1</sup> This description appeared in *Virchow's Archiv*, vol. 45, 1867.

A few days later Prof. Olshausen in Halle confirmed the above description of Schultze, and adds: "I could twice introduce a No. 7 catheter into the female genital canal (Fig. 5, v), and the catheter could then be felt per rectum much more plainly than when situated in the urethra. The button-shaped expansion (uterns?) was likewise plainly discernible."

Armed with these two formidable documents Hohmann again came to Würzburg during the winter of 1867-8, when I was junior assistant to Prof. Scanzoni, and was repeatedly examined by Scanzoni, Franqué, and various gentlemen, myself included. We could recognize all the conditions described by Schultze with great ease,—the vagina, and uterus, and the tube—all, except the supposed ovary, which persistently escaped detection, perhaps because our forefingers did not possess the requisite length. The escape of real seminal fluid was again verified on this occasion, spermatozoa being found clinging to the sound with which the uro-genital tract was explored.

In November, 1868, Hohmann presented herself to Professor Rokitansky, in Vienna, who, in the book of autograph certificates mentioned, designates the case as one of hermaphroditism, the exact nature of which can only be determined by a post-mortem examination. In a publication in the *Wiener Allgemeine Med. Zeitung*, No. 27, 1868, Rokitansky, however, expresses himself more decidedly, for he calls the case "the first case of real, true bisexuality in man." He agrees with the description of the internal sexual organs given by Schultze, and says that, while in most cases of almost total absence of the uterus the ovaries are rudimentary, in this instance the ovaries appear to possess a more perfect development and to contain Graafian follicles, as evidenced by regular menstrual discharges. Therefore, both kinds of sexual glands are united in this person. He appears to infer the presence of two ovaries, although he does not state the whereabouts of the right ovary.

With the cessation of menstruation the case entered into the new phase, to which I referred above when enumerating my reasons for adding this paper to von Franqué's previous report. Since the menopause H. claims gradually to have experienced an increase of her sexual desires for the female sex, which the concomitant growth of her penis enabled her to gratify to a

greater or lesser degree. While in 1863 the penis during erection measured but 3'', in 1868 it had increased to over 5'', and in a certain position imperfect coition was possible, as far as the peculiar attachment of the glans to the scrotum permitted; at all events, H. claims to have impregnated a girl in Vienna, who, unfortunately for science, aborted at three months. This latter statement was received with incredulity by most of the medical gentlemen to whom she presented herself, and in Pesth, Dresden and Leipzig she was induced to substantiate her claims to virility by performing the sexual act in the presence of the doubters, who furnished the necessary female for the purpose. During the next six years H. travelled about Europe, and her book contains the more or less conflicting certificates of many of the coryphees of medical science. Of these I will only refer to a few:

Ziemssen, Zenker and Rosshirt, at that time in Erlangen, all felt the supposed ovary. Kehrer and Leuckardt in Giessen, Dohrn in Marburg, Schwartz in Gottingen, Breisky and Lücke in Berne, and Hegar in Freiburg, pronounce the left inguinal body, mentioned by Schultze as an atrophic crural hernia, to be the left testicle. Breisky states that the urethra admits a thin metal probe to the depth of eight centimetres. The corpora cavernosa of the penis are not contiguous to the urethra. Whether the body to be felt per rectum in the left pelvic segment is an ovary or not, cannot be determined during life. Spiegelberg, in Breslau (1872), considers the previous existence of regular menstruation, in view of the exceedingly rudimentary condition of the uterus, highly problematical; as also the significance of the small body in the left inguinal region and at the end of the tube. Rindfleisch, in Bonn, alone of all observers, has unconditionally pronounced H., over his own signature, to be a case of true lateral hermaphroditism.

Virchow (*Berliner Klin. Wochens.*, 49, 1872), although he did not succeed in touching the ovary, speaks of the case as "the only instance, to his knowledge, in which the characteristics of true hermaphroditism have been traced so closely, as to require only the anatomical confirmation of the conclusions already arrived at."

Hitherto Hohnmann had still passed publicly for a female, and had worn female attire, changing it for the male costume



on her travels, or alternating between the two during her professional exhibitions. In 1872, however, there being no female function left, her name was legally changed to Carl, and she definitely assumed the male station.

On the morning of October 5th last, a sleek, gentlemanly-looking person, in a clerical suit of black, presented himself at my office with a card of introduction from my friend and former colleague in Würzburg, Dr. Peter Reuss, of Bremen. The face of the person appeared familiar, and, glancing at the writing on the card, I at once recognized the much improved features of the coarse peasant woman of eight years before. He informed me that, having exhausted Europe, he had now come to try his professional luck in this country, and had brought with him his wife, (!) to whom he had been married some three years. During the six weeks of H.'s stay in this city I had ample opportunity to study the case, and to arrive at what seems to me a correct estimate of the anatomical peculiarities and the value to be attached to the claims preferred by H.

The general appearance of CARL Hohlmann differs very materially from that of *Catharine H.*, as the gentlemen to whom he may exhibit himself during the coming summer will readily perceive on comparing his present appearance with that given in Figs. 3 and 4, in both of which *she* appears as I saw *her* in 1867, and as she is described by von Franqué. On removing the male clothing, however, to which, and to the barber's art, this change of appearance is mainly due, the resemblance to the form depicted in Fig. 4 becomes very marked. The features are rather more masculine in type, certainly so when in male attire; still, with the waving black hair which he formerly wore, and in female dress, the feminine cast of features is equally apparent. This is especially the case, if the view of the left profile alone is taken, for, as will readily be noticed, the whole left side of the body, on which the supposed ovary is situated and the defective formation of the genital organs is most marked, is less strongly developed than the right side, and the right half of the face is more masculine than the left. This peculiar feature has been noticed by Virchow (l. c.) and by a writer in the *Lancet*, Aug. 22, 1874 (the only short report of this case in an English journal). The beard is slight, the hairs "few and far between," but still distinct. The neck

and thorax approach the masculine type, more so than formerly, when the figure still possessed a great deal of the roundness of outline peculiar to woman. The mammae present the most prominent external feature; they are still full and well developed (notwithstanding the 51 years of II.'s age), although they have lost much since the cessation of menstruation; the nipples are large, prominent, and rose-colored; the areolæ large and somewhat darker in color; indeed, as Virchow said very truly in 1872 (l. c.), "many women at her age would be proud to boast of such mammae."

The pelvis likewise approaches the male type, although the pubic arch and the pelvic cavity certainly appeared to me more capacious than in the normal male pelvis. The relation of the extremities to the trunk, and the trunk itself, especially the back, is more feminine, the knees converging as they do in the female. The right leg will be noticed in Fig. 4 to be larger, and the muscles more prominent than in the left leg. The voice is quite deep and full; II., however, says that he sings soprano.

The whole appearance of II. still corresponds very well with the description given by Franqué (l. c.), considering the changes wrought by 8 years, and the alteration in dress and habits. The presence of a peculiar diffidence of manner, rarely witnessed in *men*, even of the lower classes, may possibly aid in explaining the statement made by Virchow as late as Nov., 1872 (l. c., in which paper, by the bye, he speaks of II. only in the feminine gender), that in opposition to former observers, who decided in favor of the male type, he finds the female character by far the more harmonious one of the two.

The *external sexual organs* undoubtedly present a masculine appearance, indeed none other was ever claimed for them. In the centre is a small curved penis, about  $1\frac{1}{2}$ " in length, closely attached as far as the corona glandis to the subjacent integument; it resembles an enlarged clitoris, partly by its small size and the absence of a well-formed prepuce, and partly by two tortuous folds of integument, which run from either side of the apex of the penis upwards towards the mons veneris to the length of about 2". The penis also resembles a clitoris in that it is not perforated, the urethra terminating on the posterior aspect of the glans near the corona in a diamond-shaped groove with a transverse slit; the end of the penis

merely presents a longitudinal furrow. The urethra is wide and dilatable, and easily admits a No. 16 steel sound. Below the urethral aperture is a shallow fossa lined with integument, below this merely integument, but no trace whatever of a vaginal pouch; on the right side, as Figs. 1 and 2 distinctly show, is a distinct scrotum with an undeniable testicle; there is but a very faint trace of a raphé, and none whatever of a left scrotum or testicle; the body felt in the supposed left scrotal sac by Scanzoni, Friedreich, and others, doubtless consisted only of adipose tissue, and disappeared with the atrophy of increasing years.

The tumor in the left inguinal region, which has been taken either for a left testicle or the remnant of a crural hernia, appears to me very like an atrophic testicle; its shape, consistence, and position certainly remind me very much of several instances of left cryptorchis which have come under my notice.

If stretched upwards by the finger, the large integumental folds on either side, and the two tortuous fringes, may very readily be taken for labia majora and nymphæ respectively (Fig. 2).<sup>1</sup> The perineal integument is very lax, and permits the finger to be carried up for some distance under the symphysis pubis, where the two diverging rami of the penis are palpable.

Of the *internal* sexual organs, in which the chief interest of the case centres, I was able, by repeated careful digital examination per rectum, and with a steel sound in the urethra, to obtain a tolerably clear impression, coinciding with that first given by Schultze, as shown in Figs. 5 and 6. I could easily recognize the canal or cord running off from the urethra to the left at *v*, also the cord or tube running still further to the left (Fig. 6, *u h*), and at the end of this last cord posteriorly and to the left of the median line an irregular, flattened, very sensitive body, about the size of an almond, as it seemed to me, for I could not clearly define its borders. This body I could

<sup>1</sup> Although I have called the general appearance of the external sexual organs of H. "masculine," still they might with equal readiness be classed with those rare anomalies described by Klob (Weibl. Sexual Org., p. 452) as excessive grades of *hemaphroditismus transversalis muliebris* or *gynandria*, in which clitoris, urethra, vagina, and labia majora and minora assume conditions almost precisely similar to those here described, the ovaries also descending into the coalesced labia majora to simulate the scrotum and testicles.

reach only, be it observed, by depressing the elbow of the examining arm as deep as possible, H. at the same time bending over almost in a sitting posture, so as to bring the pelvic viscera nearer the pelvic outlet, and by using the other hand as in bimanual examination. But I could no longer pass a probe into the rudimentary vagina and uterus, *v u*, as I had done in 1868, which Hohmann accounted for by saying that in Kiel, a few years previously, the urethra had been probed with so much violence as to cause profuse hemorrhage, and that since then no one had been able to enter the vagina. Probably it had been closed by adhesive inflammation. Neither could I detect, therefore, the slight enlargement formerly recognized as a rudimentary uterus. Agreeing with all previous observers, I found no trace of a prostate gland or vesiculæ seminales. Of a right ovary, or anything that might be taken for it, I could discover no trace whatever.

In answer to my question, as to the possibility of performing coition with an organ of the dimensions of the one in his possession, bound down by firm adhesions, as it is, H. informed me that he lived very happily with his wife, and that in a certain peculiar position it was easy for him to perform his marital duties in a manner satisfactory to both parties.

As his statement, that his penis increased during erection from the diminutive size, at which it presented itself to us, to over 6" in a curve, appeared rather incredible to the members of the Obstetrical Society and myself, I took occasion to measure the organ when in the state of turgescence, and found it to measure from root to tip  $5\frac{1}{2}$ ". The close attachment of its whole under surface to the scrotal integument, however, prevents the normal elevation of the organ during erection, and it is thus compelled closely to hug the convex surface of the testicle, like the crest on the top of a helmet. Of course an *immissio penis* is out of the question, the sexual act consisting merely in the introduction of the dorsal surface of the penis between the labia, the ejaculation taking place directly downwards, and the semen probably not entering even the vulvar orifice. The occurrence of an impregnation, therefore, seems to me exceedingly improbable.

To what conclusion now does a careful and impartial review of the historical and anatomical relations of Hohmann lead us?

Is *he* a man, is *she* a woman, or is *it* really a true lateral hermaphrodite? I am afraid this is a question which cannot be answered conclusively in all its points at present. A man, and only a man, he certainly is now, and has been ever since the alleged menopause; this he admits, and there would be no use in his attempting to deny the fact, for it is obvious to every observer. Therefore, being a man, he evidently cannot be a woman at the same time, unless the sexual qualities of both are united in one body, and this the early history of H. would appear to render probable. Von Franqué (l. c., p. 58) says that the case, which he is about to report, speaks much more for the occurrence of lateral hermaphroditism in man than against it, inasmuch as the symptoms of the functions of both sexes were observed in it, and that, as far as he was able to ascertain, a regular menstrual discharge—the sign of a functioning ovary—and seminal emissions have never before been witnessed together in one and the same individual.

Now, if an impartial observer like von Franqué, who was able to find neither the rudimentary vagina and uterus, nor the tube and supposed ovary at its extremity, could arrive at such a conclusion, merely judging from the functional expressions (which, of course, says F., require to be confirmed by the post-mortem discovery of an ovary), how much easier must it be for us, with the undeniable knowledge of the presence of the above-mentioned internal organs, to form a definite opinion favorable to the claims of H.!

The observance by competent and reliable authorities of a regular periodical discharge of blood from the genital organs, preceded by the ordinary menstrual molimina, is certainly the strongest point in favor of the existence of a female germinal gland. Menstrual *molimina*, at least, have been observed only where there was or had been an ovary (as after some double ovariectomies): so-called pseudo-menstrual sanguineous discharges have nothing to do with ovulation, and do not recur for years (24), as in this case. Granting the significance of this symptom as an evidence of the presence of an ovary, the undisputed contemporaneous emission of seminal fluid at once decides the question in favor of true hermaphroditism.

Of special interest also is the predominance of the female sexual and social propensities during the continuance of men-



strual life, and their gradual supplanting by masculine characteristics and tendencies after the menopause. To be sure, the psychological history of the case rests entirely on the statements of the individual himself, and is therefore open to a considerable degree of doubt; still, the harmony and correspondence existing in all his assertions, and the fact that in the course of repeated conversations I have never been able to discover a discrepancy or contradiction in his statements, disposes me to accept them more or less fully. My former acquaintance with the case in a female character certainly assists the impression I now derive from the history.

It will be noticed that of all the gentlemen whose certificates II. possesses, two only positively declare her to be a man, one only—Rindfleisch—with equal positiveness pronounces her to be a true case of lateral hermaphroditism; the remainder give no definite opinion as to her sex, some perhaps inclining towards the male, and not unnaturally so, judging merely from the external genital organs. Of course those who did not detect anything unusual about the internal sexual organs, such as Dohrn, Schwartz, and others, were not in a position to give a decided opinion as to the sexual status, certainly not a favorable one. In face of those numerous gentlemen who *did* find all or part of the internal conditions mentioned by Schultze, the negative result obtained by the observers just mentioned can scarcely carry very great weight. Of all the authorities I have quoted, Virchow seems to me to have expressed most clearly and fairly the opinion to which I also subscribe, viz., that this is the only case on record, in which the characteristic features of real hermaphroditism have been traced to such a degree, as to require only the anatomical macro- and microscopical confirmation of the presence of an ovary, which a careful and impartial consideration of the history and anatomy warrants us in assuming.

The interest of the case lies chiefly in the past, and presumably also in the future, *i.e.*, post mortem. The present shows us only a very ordinary instance of male hermaphroditism. In order to permit a proper reference to the past history, and a correct appreciation of the possible future, when an autopsy will confirm or dispel the conclusion to which we now arrive, a detailed report, like the one here presented, is necessary. If the peculiar nature of the subject and the exigencies of the case

have unavoidably led me on delicate and slippery ground, I trust that my readers will look upon it as done in the interests of science, and remember that "*Honi soit qui mal y pense.*"<sup>1</sup>

<sup>1</sup> After this paper had gone to press, I accidentally came across a popular work entitled "The Physical Life of Woman," &c., by George H. Naphys, A.M., M.D., Member of the Phila. County Med. Soc., Corresp. Member of the Boston Gynæcol. Society, etc., Phila., 1875, in which, on page 20, I find the following reference to the subject of my paper: "Many a physician disbelieves that there has ever been a person of both sexes—a true hermaphrodite. They are very scarce, but they do exist. There is one now living in Germany. It bears a female name, Catherine Hohmann. She was baptized and brought up a female; but Catherine is as much a man as a woman. The learned professor of anatomy, Rokitansky, of Vienna, asserts most positively that this is a real hermaphrodite. Her history is sad. Born in humble circumstances, when of marriageable age, she loved a man who wished her to emigrate with him to America. But when she disclosed to him her deformity, he broke off the engagement and deserted her. Then her affection became fixed on a young girl, but how could she make her suit to one apparently of her own sex? With passions that prompt her to seek both sexes, she belongs to neither. 'What shall I do here on earth?' she exclaimed, in tears, to a man of science who recently visited her. 'What am I? In my life an object of scientific experiment, and after my death an anatomical curiosity?' In a note at the end of the book he says, as authority for the above statement: "Rokitansky decides Hohmann to be a case of *hermaphroditu vera lateralis*, and all who examine her say the same. See *Wiener Med. Wochenschrift*, Oct., 1868, and the *Med. and Surg. Reporter*, vol. xix., p. 487." My observations, and the careful scrutiny of all the certificates and papers relating to the case do not justify me, as I have stated above, in making such a decided statement as to the sex of H. as that made by Rokitansky, which I have quoted above. The extract in the *Med. and Surg. Reporter* (quoted by Dr. Naphys) seems to be taken merely from an advertisement inserted by H. in the *Wiener Med. Wochenschrift*, and the romantic effusion towards the close of the extract has emanated from the imaginative brain of a reporter of the *Wiener Freie Presse*, a secular paper. This is the authority given by the *Reporter*. My past and present experience of H. by no means clothes her in the melancholy, misanthropic garb depicted by Dr. Naphys. To me she always appeared to look upon her physical deformity as a godsend, as a means of earning an easy livelihood.

In a letter just received in answer to my inquiry, H. says that 22 years ago her lover, knowing her peculiar deformity, proposed to her to emigrate to America, where she could have an operation performed (it being their idea that a vagina could be constructed by dividing the perineal integument); H. objected for fear of the danger, and they continued to live together for 10 years longer. This old lover was spoken to by H.'s present wife during the past year.

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## CLINICAL CASES.

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### CASE OF DYSTOCIA CAUSED BY A HYDROCEPHALIC FŒTUS.

CEPHALIC PUNCTURE OF FŒTUS AT THE SUPERIOR STRAIT—  
DELIVERY BY UTERINE ACTION—SUBSEQUENT OPENING  
OF THE PUNCTURE ON THE SECOND DAY—DEATH  
OF CHILD FOUR WEEKS AFTERWARD  
FROM DIARRHŒA.

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By WALTER R. GILLETTE, M.D.,  
New York.

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APRIL 3d, 1873, at 4 A.M., I was called to attend Mrs. L., in labor with her fourth child. Labor had commenced two hours previously, and pains were rapid and strong. Examining her, I found the os uteri high up the superior strait, widely dilated, with protruding bag of waters, through which I could feel the foetal head.

I ruptured the membranes, and drained off considerable amniotic fluid. Pains now came on furiously, but made no impression upon the descent of the head, which still remained at the superior strait. At the end of an hour I examined more carefully, and from the extreme mobility of the parietal and occipital bones, and from the widely divergent sutures and fluctuation, thought I had to deal with a hydrocephalic fœtus. I then anæsthetized the patient, introduced my *whole* hand in the pelvis, and with combined manipulation of the left hand externally distinctly made out my case as one of hydrocephalic fœtus, and of great degree. I told the husband and nurse what I had discovered, and the patient accidentally overheard me, and said, "I thought so; this is the fourth time I have had a hydrocephalic child." This was corroborated by the husband, who said she had suffered this way in all her confinements, and begged me to relieve her at once, as previously she had been allowed to linger in labor for forty-eight, and in one instance

seventy-two, hours, and finally had to be delivered by craniotomy.

I sent the husband to my office for my instruments. He was gone about two hours ; in the meantime the pains were furious, but unavailing ; the head would not move nor engage in the superior strait.

The fœtal heart was normal.

I then anæsthetized the patient, drew the urine, and with my whole hand in the pelvis as a guide, with my left hand plunged an ordinary trocar and canula in the suture, just opposite the left posterior superior angle of the parietal bone. I passed it just within the cranium ; then withdrawing partially the trocar, passed the cannula close along under the parietal bone, taking care not to push toward the brain substance. Withdrawing now entirely the trocar, I was rewarded with a stream of serum, which, unfortunately, I did not measure, but which I imagined was quite a quart.

Labor kept on actively, and with each pain came a spurt of serum through the canula, which I had left in after the serum ceased running.

In forty minutes the child was born, with the cannula still sticking in its head, and commenced to cry. I now removed the cannula from the collapsed mass of scalp and projecting sutures, and performed the necessary duties to the mother.

Upon examining the head I found all the sutures separated above the base of the skull, as is usual in these cases. I bandaged the head, which still continued to ooze serum. Nothing unusual happened to the child on the 3d. On the 4th the head had ceased discharging.

On the 5th the head had partially filled again, and the child was in convulsions. I now measured the head and found the

Occipito bregmatic circumference..... 19 inches.

“ vertical	“	..... 17½	“
“ frontal	“	..... 20	“
“ mental	“	..... 22	“

I then, with a director, opened the original point of puncture, and evacuated considerable fluid (not measured), with the effect of immediately relieving the convulsions.

The child was comfortable after this—sleeping and nursing until the 7th, when convulsions recurred, but as there was no

marked increase of fluid accumulation, I did not reopen the puncture; but gave it chloral hydrat. gr.  $\frac{1}{4}$  every hour, quieting it with the second dose. Convulsions did not recur until the 10th, when I tried chloral again, and failed, but succeeded with pot. bromid. gr. 1, every hour for three hours. All its functions seemed active, and it thrived. It had convulsions off and on until the 19th, which I controlled apparently with the bromide. I could not discover that the head had filled very much, and did not care to open it again on my own responsibility, when it was evidently improving, and the convulsions apparently easily controlled. I called Prof. Budd in consultation, who advised me to continue as I was doing. April 19th, I measured the head again, and found all its circumferences diminished about two inches.

On the 23d, it started for Louisville, Ky., with its parents, all convulsions having ceased on the 19th.

It died in Louisville, May 10th, of diarrhœa, in the meantime having evinced no bad symptom.

The mother was a native of the U. S., æt. 30, and had been married eight years. She was a pale, delicate, blonde woman, with no traceable hereditary taint of tubercle or syphilis. Her husband was a healthy man, and denied any history of disease or taint of any sort.

As a sequel to this case in October, 1874, Mrs. L. came to me again from Louisville, stating that she was again pregnant and about three months advanced. She requested my advice as to whether she should go through another term of pregnancy with the bare hope of having a living child, or whether it was proper and right that abortion should be induced at once, in order that she and her offspring might escape the horrors that labor at term entailed upon them both. I told her that in the light of her past experiences I did not believe she would be any more successful in the future, but the responsibility of the decision was too great for one physician and I must have a consultation. Prof. Budd met me in consultation, and coincided with my views.

I induced abortion, Oct. 6th, by means of the sponge tent. Introducing the tent on the evening of the 6th, the ovum came away upon its removal on the morning of the 7th, and her recovery was complete.



The points of interest in these cases are several.

*First.*—The fact that this woman had given birth to *four* hydrocephalic children.

I can find no record of a similar frequency of this accident or habit.

*Second.*—The method of diagnosis, which was absolutely and indisputably gained by the introduction of the *whole hand* in the pelvis. The ordinary method of diagnosis by the touch of one or two fingers seems to me very inadequate. There can be no objection to using the *whole hand* with the patient anaesthetized. In three cases of hydrocephalic fœtus which have fallen to my lot I have used this generous method of exploration, if I may so express it, with entirely satisfactory results; being able to map out positively the character and degree of the monstrosity, and I recommend it in all cases where it is applicable with the vertex presenting, and not moulded in the pelvis.

*Third.*—The careful introduction of the trocar and canula, avoiding deep puncture and wounding of the brain.

*Fourth.*—Although Simpson, Congnert, and others mention the possibility of the child living after this procedure, I do not know of a case recorded where the child lived so long, especially with a repetition of the puncture as related. Indeed, in the literature of the subject which I have been able to examine, and upon inquiries among those largely engaged in obstetrical practice, I cannot find a case where the child lived more than a few minutes after birth.

*Fifth.*—The question of propriety involved in the introduction of abortion to avoid the future possibility of a similar procedure (cephalic puncture), is certainly a wide one for discussion, however contented I was with my opinion and action.

## GASTRO-ELYTROTOMY SUCCESSFULLY PERFORMED.

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By ALEX. J. C. SKENE, M.D.,Professor of Gynæcology in the Long Island College Hospital, Brooklyn, New York.

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Mrs. F., aged 31 years, born in England. She is rachitic, and, in giving her history, stated that she was unable to walk without support until eleven years of age. At twenty-five years of age her physician delivered her by performing craniotomy, after which she made a slow recovery, and was confined to her bed for six weeks. Some time after that she was delivered at the seventh month. The child lived only a few minutes. When she came to this country her physician gave her a letter stating the nature of her former confinements, and also that she had a deformed pelvis. This letter she brought to me in 1872, when I found that she was again pregnant. She expressed a strong desire to have a living child, and, upon examining the pelvis, I resolved to let her go until the beginning of the ninth month, when I proposed to induce labor, in the hope of obtaining a living child. Accordingly I brought on labor in the first week of the ninth month, and found an arm presentation. Version was performed with great difficulty, owing to the fact that the antero-posterior diameter of the superior strait was not more than two and three-quarter ( $2\frac{3}{4}$ ) inches.

By using strong traction, and extreme pressure over the uterus, depression of one of the parietal bones was produced, and thereby a small child was delivered, which lived for several months. Metritis followed, and she suffered great pain in her back and limbs. There was also partial paralysis of the limbs, which slowly disappeared. Altogether it was five weeks before she recovered. At that time she was strongly advised to give up child-bearing. Some time ago she called upon me and stated that she was pregnant, and urged me to save her child if possible. I told her if she would take the risk, I would give her what I believed to be the only certain chance of having a child, and also the possibility of saving her own life. She

cheerfully accepted the proposition, and expressed her determination to go to full time.

I gave the history of her case to Prof. T. G. Thomas and asked his advice and assistance. His kind reply was that he would be at my service at any time. Late in the afternoon of Oct. 28th, 1875, she called at my office, and, on examination, I found her at full time, the os uteri dilatable, and that she had slight uterine contractions.

She was ordered to go home and to let me hear from her late in the evening. At ten o'clock her husband reported that she had slight pains, but was about the house and felt comfortable. I gave directions to call me during the night if her pains increased, and not to wait longer than four o'clock in the morning. Labor progressed, but, being unwilling to disturb me, they did not call me until six A.M. on the 29th. I then found that she had good labor pains, and that the os uteri was almost fully dilated; the membranes were unruptured, and an arm and the cord were presenting. I could feel the cord pulsating through the membranes, and the foetal heart was heard distinctly. I sent in all haste for Prof. Thomas, but most unfortunately he was engaged and could not come. I also sent for Drs. Corry, Cushing, Stuart, and Bunker. While waiting for my medical friends, I was in constant dread that the membranes would rupture and the cord become compressed so as to destroy the child.

I felt sure that I could not restore the cord and keep it in place when there was an arm presentation, and I dreaded the death of the child, which would have decided in favor of delivery by embryotomy, an operation which, under the circumstances, would have been difficult and very dangerous to the mother. Fortunately the membranes remained intact until my friends came. At nine o'clock, I performed gastro-elytrotomy, according to the method of Prof. Thomas, the best, indeed the only authority on this operation. I opened the abdominal wall and also the vagina, before I ruptured the membranes. I then delivered by performing version.

Having reason to believe, from the character of the foetal heart action, that the circulation of the child was slowly becoming interrupted, I made great haste to deliver as soon as I ruptured the membranes.

The child, which weighed ten pounds, was slightly asphyxiated, but was easily restored.

The time occupied from the beginning of the operation until the child and placenta were delivered, was fifteen minutes, five minutes longer than my former operation. The extra time required was owing to slight hemorrhage, caused by making the incision in the abdominal wall lower down than was necessary, and also by having to restore the prolapsed arm, and deliver by version.

There was no shock or vomiting after the operation, and no hemorrhage, primary or secondary, worthy of notice.

Several hours after the operation, Dr. Stuart, in passing the catheter, discovered that the bladder had been opened. The opening in the vagina had been extended so as to enter the junction of the urethra and bladder on the right side.

I am satisfied that the bladder was not wounded at the time when I opened the vagina, but that it occurred during delivery.

If I had had more time, and could have permitted the parts to distend gradually, the wound in the bladder would not have been made. It was unfortunate, if not bad management on my part, that I did not detect the wound in the bladder at the time of the operation, for then a few stitches could have been easily introduced and the catheter worn until the opening closed.

When I became aware of the accident, I was unwilling to anaesthetize my patient again and submit her to the operation of closing the wound, because I feared that I might cause hemorrhage.

The second day the temperature went up, and she became quite tympanitic, but there was no tenderness to indicate peritonitis.

The symptoms were due mostly, I believe, to a slight metritis, such as she had after her other confinements. These conditions continued for about four days, but the temperature did not at any time exceed  $102^{\circ}$ , and the pulse did not go beyond 120. On the sixth day, the tympanitis was relieved by an injection containing mint water.

Morphine was given at night to secure sleep, and quinine was used in the day in the hope of preventing malarial fever, which had troubled her, off and on, for years. From the sixth to the tenth day her condition was remarkably good. Her

pulse and temperature were normal; her appetite good, and she had a scanty secretion of milk.

On the tenth day she sat up in bed, contrary to our expressed wishes, but was apparently no worse for it.

In the afternoon of the eleventh day she had a chill, followed by fever and free perspiration. She was quite well on the morning of the twelfth day, but the chill and fever were repeated in the afternoon.

Quinine was freely given on the following day, and there has been no return of chills or fever. She believes that this part of her trouble was a return of her familiar ague, and I believe that her diagnosis was correct.

The external wound healed by first intention, except at the two central stitches, where there was suppuration. Both the abdominal and vaginal wounds were completely healed on the fourteenth day. On the fifteenth day she sat up in a chair while her bed was being made, and, with the exception of the opening in the bladder, she was as well as one of her constitution could be at that date after confinement. At the end of three weeks she went out riding, and continued in good health from that time. On November 28th, I closed the small vesico-vaginal fistula, and two weeks afterward dismissed her well and sound. January 15th, 1876, she is well, and nurses her strong, healthy baby.

Finally, I wish to state positively that I believe the injury done to the bladder was the fault of the *operator*, not of the *operation*, and that I could avoid that accident in future. I am also satisfied that I could not have under the circumstances delivered that patient in any other way, with more *safety* to *herself*. I trust that the history of this case (the first successful one on record) will aid in placing the operation among the chief triumphs of obstetrical surgery, to the honor of Professor Thomas, whom I believe to be its author.

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## CENTRAL RUPTURE OF THE PERINEUM, AND ESCAPE OF THE CHILD THROUGH THE RUPTURE.

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BY F. LYLE MARTYN, M.D.,  
Oxford, Iowa.

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AUG. 23, 1873, I was called to attend a German woman, aged 29, in her first confinement. Her pains had commenced about 10 P.M. on the evening previous, gradually increasing in severity until 8 A.M., when they were described as being extremely severe, and continued so up to the time of my arrival—about 9 A.M.

I found the patient much exhausted, and threatened with symptoms of convulsions. I immediately made an examination per vaginam, and found the occiput and upper portion of the neck just within the vulva, the head impinging against the pubis. Further examination discovered the perineum greatly distended, and that a laceration had taken place in its central portion, through which the breech of the fœtus was being expelled. Fearing lest another pain might cause a complete rupture of the perineum, and realizing that at this stage of the labor, the greater portion of the breech protruding, the exit of the fœtus must be through the rupture, and to expedite delivery, I supported the perineum with one hand, while with the other I succeeded in pushing the head from its position, thus in part relieving the anterior perineum, when another pain expelled the fœtus, followed immediately by the placenta. On examining the parts, the fourchette and sphincter ani were found entire. One of the women who had been in attendance on the patient during most of the labor, told me that soon after the membranes ruptured she made an examination, and found the head presenting. This person having attended many cases of labor, I considered her statement worthy of credence.

I regret that I am unable to give the exact figures in regard to the size of the child. It was a male child, perfectly developed, and could not have weighed less than seven pounds; in my

judgment, its weight was more ; and although dead when born, yet there was nothing in its appearance to denote that life had been extinct for a longer period than, at most, a few hours.

*After-treatment.*—Instead of proceeding at once to operate for closure of the wound, I concluded to wait until the lochial discharge should in a great measure cease. In the meantime, I confined the patient to her bed with the thighs kept in apposition by means of a bandage, using the catheter to evacuate the bladder. The wound was treated with oakum and carbolic oil. By the eleventh day the wound had healed about one-half its extent. I then proceeded, with the assistance of Dr. J. E. Jones, of Williamsburg, to pare the edges of the portion remaining, brought the parts in apposition by the wire suture, and in twelve days after the operation, and twenty-third from time of confinement, the wound was entirely healed.

About sixteen months from time of first delivery, I was again called to attend the same woman in her second confinement.

I arrived at the residence of the patient about 3 P.M., about three hours after labor had commenced. On examination per vaginam, I found the os uteri dilated, with a vertex presentation. The membranes rupturing within three-quarters of an hour after my arrival, I made out the position as being the first, occiput in left acetabulum. From this time the labor progressed rapidly, the perineum yielding readily, and everything promised a speedy delivery, until the head commenced to press upon the vulva, which I found to be rigid, and resisting all efforts to expel the head. Supporting the perineum, and, at the same time, with the aid of an assistant, making persistent pressure against the edges of the labia for nearly three-quarters of an hour, the head was at last permitted to escape.

From all the information which I could obtain from those in attendance on patient in her first confinement, together with my own investigations, and the character of the second labor, I was led to conclude :

1. That in the first labor there were no complications in the presentation, nor was there any abnormal condition of the pelvic cavity.

2. The resistance which the head encountered from a rigid vulva in the first labor, as in the second, except to a far greater

degree, accounted for the manner of delivery—labor progressing naturally until resistance to the escape of the head was encountered at the vulva, which not being overcome, the body was forced into the pelvic cavity, escaping as described.

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#### POLYPOID TUMOR OF THE LEFT LABIUM MAJUS.

BY FRANK H. RANKIN, M.D.,  
New York.

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IN the AMERICAN JOURNAL OF OBSTETRICS for August, 1875, Dr. Chadwick, of Boston, reports a case of polypoid elephantiasis of the right labium majus, and having had a case analogous to it in appearance recently under my care, I am induced to give it publication. A. M., æt. 40, maiden woman, domestic, noticed seven years ago a little pimple or wart-like growth on the left labium majus. This remained stationary, without any perceptible growth for six years, when it commenced slowly but steadily to elongate. Four months ago, eight months after it commenced to grow, it was about three-quarters of an inch in diameter, and about as large around as one's little finger. At this time it took on quite a rapid growth and became bulbous at the end, which end, in the course of two months, was the size of a pigeon's egg. During the subsequent six weeks it changed but little. The patient states that during the past four months the tumor would vary in size from day to day, the bulbous end would shrink up, and then without any apparent cause would grow larger again. Ten days before the patient came to me the tumor was about the size of a black walnut; it then enlarged quite rapidly, and at the same time the patient began to experience some pain at the point of attachment of the pedicle to the tumor. At times she found it very difficult to go about, as the tumor, which hung between her legs, interfered with walking, by becoming twisted upon itself, and causing a great deal of pain.

On making an examination, I found a tumor suspended between the legs by a pedicle, which was two and a half inches long, attached to the left labium majus, on a line with the exit

of the meatus urinarius, about as large round as one's little finger, and covered with a delicate skin. At the point of attachment to the tumor it was about three-quarters ulcerated across, so that the tumor hung by a mere shred. This ulceration was doubtless caused by the twisting of the tumor, occasioned during walking. The ulceration also was the cause of the pain. Just behind this point of ulceration I tied a ligature, and detached the tumor with a pair of scissors, close to its attachment to the pedicle. The cutting caused no pain, and there only escaped a few drops of blood. The tumor weighed five and three-quarter ounces, measured nine inches in circumference, three inches across in its horizontal diameter, and one and three-quarter inches in its vertical. It had an appearance similar to an œdematous scrotum, was covered with a delicate skin, and was translucent, except in the centre.

The patient refusing to allow me to remove the pedicle on the same day I amputated the tumor, I deferred doing so till the following day, July 20th, 1875, on which day she returned to my office, feeling very well. The end of the pedicle from which I removed the tumor had commenced to swell and was as large as a small hickory nut, and appeared distended with serum. With the assistance of Dr. H. T. Hanks and Mr. Frost Johnson, I tied an elastic ligature around the pedicle, close to its attachment to the labium, the ligature causing considerable pain, which lasted for about three hours. I saw the patient four days subsequently; she had experienced no pain since the day the ligature was applied. The pedicle had not yet sloughed off, but had shrivelled up, and was not more than three-quarters of an inch in length. She then left for her home in the country, and I have not seen her since.

The patient was a healthy-looking woman, and there was no indication of her ever having had syphilis; she had always been perfectly regular with her courses till a year ago, the time at which the tumor commenced so rapidly to enlarge. During the past six months her courses had ceased entirely.

I made a slight incision into the tumor, and it appeared to consist of cellular tissue, infiltrated with serum. There was no cyst, and so soon as the tissue was cut into the serum flowed out, and the tumor in a measure grew smaller. No microscopic examination was made. The jar in which the tumor was

placed being defective, the alcohol evaporated, and on looking for the tumor, I found in its place a dried mass not more than a line in thickness, and an inch and a half in diameter, showing conclusively that the tumor was composed chiefly of serum.

The cut representing Dr. Chadwick's case will also very well represent my case, with this exception, that, in the case I now report, the tumor was not so pear-shaped, but had more the shape of a tomato or apple, and was puckered up at its attachment to the pedicle.

117 EAST 26TH ST.

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## TRANSACTIONS OF THE NEW YORK OBSTETRICAL SOCIETY.

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*Reported by* PAUL F. MUNDÉ, M.D., *Secretary.*

STATED MEETING, MAY 4, 1875. THE PRESIDENT, DR. BYRNE, IN THE CHAIR.

DR. C. C. LEE presented the specimen and history of a

CASE OF PLACENTA PRÆVIA, COMPLICATED WITH UTERINE FIBROIDS, communicated by Dr. Joseph Anway, House Physician at Charity Hospital.

Rebecca Williams, aged 31, colored, nativity, U.S. Admitted March 14th, 1875. Married, and the mother of three children.

Patient states that she began menstruating when 14 years old, and that she was "regular" up to her twenty-seventh year. She was married when 22 years old, and has given birth to three children. The youngest one is now two years old. These labors were normal in every respect. During the last three or four years the menstrual flow has occurred more frequently than before; it has lasted longer, and the quantity has been twice as great. She also states that she is now in the seventh month of pregnancy. After an examination this last statement was believed to be true.

During the last three weeks she has lost blood at different intervals, but not in sufficient quantities to prevent her attendance to her duties. On Saturday, March 13th, she began to complain of very severe pains in the lower part of the abdomen, coming at irregular intervals, and followed by a profuse flow



of blood. She was seen by two physicians, who controlled the hemorrhage by means of the tampon, and ordered her removal to the hospital on the following day. She was brought to Bellevue Hospital on the morning of the 14th, where she was seen by Prof. I. E. Taylor. He removed the tampon; discovered that the hemorrhage had ceased; and ordered her to be taken immediately to the obstetrical wards, at Charity Hospital.

*March 14, A.M.*—Pulse 120, and very weak; temperature,  $99\frac{1}{2}^{\circ}$ . Patient was not flowing, but appeared very much exhausted by what she had already lost. She was put to bed, and given sol. morph. sulph. (Mag.),  $\mathfrak{m}\mathfrak{x}$ , and in a short time was sleeping quietly.

*Monday, March 14, P.M.*—Patient has not lost any blood during the day, and has had no pain. Has been given iced milk, beef tea, and an ounce of whiskey in tablespoon doses. Temperature, normal; pulse, 112, and stronger than in the morning.

*Tuesday, March 15, A.M.*—Patient doing well. Slept during the entire night. On examination the os was found to be soft, readily admitting two fingers. Placenta could be distinctly felt implanted over internal os. To the left side of cervix the fingers could be carried further up, and at this point the placenta seemed to be detached for a considerable distance. This examination was not prolonged, as uterine contractions were excited and a slight discharge of blood followed, which ceased in a short time, the patient being kept quiet by the use of sol. morph. sulph. (Mag.)  $\mathfrak{m}\mathfrak{x}$ .

Nothing worthy of notice occurred during the next two days. The patient was allowed four ounces of whiskey per day, and all the milk she could drink.

*Friday, March 18, P.M.*—Patient has been well all day. Has not been allowed to leave her bed since admission. Pulse 99; temperature normal. One hour after the above note was made, 7.30 P.M., the physician was called to the patient, and found that blood was literally pouring away from her. She then said that she had had slight pains for an hour or two, and that during the last thirty minutes they had increased markedly. Dr. Robert Watts, then in charge of the obstetrical service, had left directions with the House-Physician to proceed at once to deliver by version in case of another violent hemorrhage. In the presence of the Chief of Staff, and the House Staff, Dr. Anway began delivery. The hand was introduced within the vagina, and the os found to admit three fingers. The patient's pulse was very rapid, and could not be counted. The cervix was gradually dilated, and the placenta entirely detached from

the left side. The head of the child presented. Position, R. O. A. A large submucous fibroid could be distinctly felt, which was attached to the posterior wall. The feet were easily reached and brought down. Two drachms of Squibb's fl. ext. ergot were now given. The patient was not losing blood, and the uterus contracted firmly on the body of the child. The body and head were readily delivered. Uterus still firmly contracted. Another fibroid, subperitoneal, could be felt through the abdominal walls attached just below the fundus, posteriorly. Pulse 160. After the placenta had been removed, the vaginal nozzle of Davidson's syringe was passed up the cervix, and iced water thrown in. One half ounce of brandy was given by the mouth. After the use of the ice water there was still found to be a slight flow of blood, and as the patient's condition would not admit of much more being lost, liq. ferri persulph.  $\frac{5}{8}$  iss., to which had been added an equal quantity of water, was injected just within the cervix. This stopped the hemorrhage entirely. Pulse 140, and much stronger than a few moments ago. Suddenly the patient began to cry for more air; the windows and doors were opened. The pulse, which a moment before was thought to be better than at any time during the delivery, was hardly felt at the wrist. Preparations which were being made for transfusing were stopped, as the instrument of the hospital was not in condition to be used for that purpose. The patient was dying, and was dead thirty minutes after delivery.

*Post-mortem Examination.*—Head: not examined. Thorax: pleura normal. The lungs were anæmic, with moderate cedema in lower lobes.

Heart and pericardium: The latter contained two ounces of serum, slightly stained with blood. The right heart contained gas from commencing decomposition. Little bubbles of gas seen underneath the endocardium. The walls were pale and flabby, and the cavities, on both sides, were entirely free from blood. On the left side they were dry and contracted. The valves were normal. Small atheromatous patches were found in the wall of the ventricle. Very little blood flowed from the cut vessel, and what did escape was of the consistency of thin currant juice.

Liver, spleen, kidneys, stomach, and intestines were anæmic, but otherwise healthy.

Uterus and appendages: The uterus measured  $9\frac{1}{2}$  inches in length, 6 inches deep; walls,  $\frac{1}{2}$  inch thick; weight, 3 lbs. and 10 oz. Upper portion of vagina and cervix contained a dark granular mass of coagula, and firm coagula were traced into the large uterine sinuses; and at the seat of the placental attachment

coagula from liq. ferri persulph. were found. A laceration two inches long was found at the cervix, along the right posterior lateral wall. Placental insertion was on the left and anterior side of cervix. Submucous fibroid, two inches in diameter, covered by a veil of decidua, was found attached to posterior wall of body. The right posterior lateral wall of the body was thin. An intramural fibroid, one inch in diameter, was found situated at junction of the right Fallopian tube with the fundus; another in the central portion of the fundus; and another in the posterior wall of the body. Nine subperitoneal fibroids, varying in size from No. 4 shot to one inch in diameter, the latter in posterior wall of body; others irregularly scattered over the surface. Evidence of old peritonitis by bands of adhesion were observed in the pelvis.

Ovaries: No corpus luteum could be found. A cyst was found in ovary.

Pelvis: Ant. post. diameter of inlet, four inches. Transverse, four and three-quarter inches. Oblique, five inches.

Bladder: Normal.

The peculiarly interesting feature of the case was the singularly defective contractility of the uterus in its whole upper part, particularly at the junction of the body and cervix, a defect doubtless owing to the presence of the fibroid tumors, one of which projected deep into the uterine cavity, and was as large as a hen's egg.

DR. PEASLEE said that *à priori*, simply considering the presence of the subperitoneal and intramural fibroids, we would expect an early miscarriage, much more so, however, if a large fibroid projected into the uterine cavity, as is the case here. Probably the submucous fibroid was intramural in this instance when pregnancy commenced, which accounts for the non-interruption of the gestation. He should hardly think that a uterus, the muscular tissue of which is so thickly interspersed with fibroid tumors, would contract down to the normal post-partum state.

DR. JACOB asked whether there exist any statistics on the connection of uterine fibroids, or any condition preventing contraction of the unimpregnated uterus, with the occurrence of placenta prævia. It might happen that the ovule is obliged in some cases to glide down to the lower portion of the uterine cavity to find normal mucous tissue to develop in; in this case, indeed, the lower portion of the uterus is the only normal part. Theoretically he thinks there is a direct connection between uterine fibroids and the occurrence of placenta prævia.

DR. T. G. THOMAS said that he knew of no statistics on the

subject. All the cases of placenta prævia which had happened in his practice were in multiparæ, who had no uterine fibroids. The placenta in placenta prævia is never attached near the external os, but always above the internal orifice, and if it is found near the external os it has always slipped down.

Dr. THOMAS also related the following

CASE OF PLACENTA PRÆVIA, WITH REMARKS ON THE TREATMENT  
OF THE ACCIDENT.

Two months ago he was called to a lady 35 years of age, who was in the eighth month of her fifth pregnancy; all of the preceding pregnancies had been normal. When she was seven and a half months gone she had suddenly been awakened at night by finding the bed deluged in blood; by the time her physician had arrived the hemorrhage had ceased spontaneously. One week later it returned, and recurred still three times, in all five times, at intervals of five or six days. At the last hemorrhage, when Dr. Thomas first saw her, she was almost pulseless, and the attendants were endeavoring to revive her with frequent doses of brandy and water, and by depressing her head. The two physicians in attendance proposed version and immediate delivery, but to this Dr. Thomas objected, for two reasons: 1. Because version, although in a lax uterus and with intact membranes one of the easiest and least severe operations, in the opposite conditions may be exceedingly difficult and dangerous; 2. And principally because it is his conviction and experience, that in a patient exsanguinated by repeated hemorrhages, the shock of the operation almost invariably proves fatal.

In support of this view he related a case in which he predicted the death of the patient from shock twenty-four hours before it occurred. In the present case he suggested waiting until the patient had recovered her strength to some degree, and then inducing labor. Her physicians thought the patient would have another hemorrhage and die; but day after day passed until on the fifth day her condition appeared sufficiently favorable to permit the induction of premature labor. The child had apparently died at the time of the last flooding. A Barnes' dilator was passed into the cervical canal by means of a silver catheter pushed into the small pouch at the side, and pure blood at once flowed out of and beside the catheter. The dilator was injected with cold water and the hemorrhage ceased, but recommenced as soon as the water was allowed to flow out, ceasing again as soon as the dilator was filled, thus showing that the pressure arrested the hemorrhage, which had been

produced by displacing a portion of the placenta when the dilator was introduced.

After consultation the patient was put under ether and he attempted to pass his hand into the uterus, but found it impossible to do so, as the placenta was central and entirely closed the cervical canal. He endeavored to thrust his hand through it, but was afraid to use much force for fear of tearing it from its attachments (an objection also made by Dewees to the above plan, first recommended by Rigby); he therefore passed his hand through the canal formed by the dilator, seized one foot of the child, turned and rapidly extracted it. As had been expected, the child was dead. No further hemorrhage ensued, the placenta being at once removed. The lady made a good recovery.

The question which he would now like to propose to the Society is this: Is it better to allow a pregnancy, during which the woman has become exsanguinated and dangerously reduced by repeated hemorrhage from placenta prævia, to go on to term, or should premature labor be induced? He chooses the latter alternative, and has lost but one case of placenta prævia in which he brought on labor prematurely; this case died of post-partum hemorrhage. The children, of course, usually succumb. In the case just mentioned he detached the placenta (which was centrally inserted), cut the cord and removed it, leaving the child in the uterus; no hemorrhage occurred; twenty-four hours later the child was safely expelled. The uterus contracted well apparently, but three hours afterwards the family physician was hurriedly called and found the lady dying of hemorrhage. In his opinion the induction of premature labor offers greater safety, both to the mother and the child, than the plan of allowing the pregnancy to go on to term. The hemorrhage from this malposition of the placenta generally occurs suddenly, often at night, and before the physician can reach the patient she is beyond medical aid, or at least at the point of death. These repeated depletions also debilitate the child, and the question arises whether a child born prematurely at the eighth month is not fully as likely to live or more so, than one weakened by repeated hemorrhages. If the labor is induced by rubber bags, the hemorrhage will be slight and the danger to the mother not great, for these rubber dilators compress so thoroughly as to arrest the bleeding from the placenta during the dilatation of the os; of course the diagnosis should be correct, and a granular endocervicitis producing occasional discharge of blood should not be mistaken for placenta prævia. This method of treatment is not mentioned in the obstetrical text-books.



DR. JACOBI agreed with Dr. Thomas, that it is better to defer operating when the patient has become debilitated from repeated hemorrhages in placenta prævia, than to run the risk of the shock which an operation might produce. As to the mode of death, does the patient die from shock or from anæmia of the already exsanguinated brain and medulla oblongata unavoidably following the rapid evacuation of the uterine cavity by version and extraction? He is confident that the latter is the true cause. (To this Dr. Thomas assented.) Dr. Jacobi further said that post-partum hemorrhage in placenta prævia differs somewhat from that after ordinary confinements, in that unusual care and watchfulness is required to prevent its occurrence. When we consider how flaccid the cervix is to the touch after a normal labor, no matter how firmly the bulk of the uterus is contracted, we can readily understand the danger of hemorrhage in placenta prævia, where the cervix and lower portion of the uterus is composed chiefly of a large vascular network, with numerous gaping orifices.

DR. LEE said, that to avoid the danger resulting from the rapid evacuation of the uterus by version in placenta prævia, proposals had been made by various authors to empty the uterus gradually, but had never been practically acted upon.

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STATED MEETING, MAY 18, 1875. THE PRESIDENT, DR. BYRNE, IN THE CHAIR.

#### RETENTION OF THE DEAD OVUM.

DR. PAUL F. MUNDÉ presented an entire uninjured ovum of about four and a half months, which was expelled from the uterus four months after the death of the fœtus, and read the following history of the case with remarks on the subject generally:

Mrs. R. S., 39 years of age, came to my class at the West-Side German Dispensary, Jan. 29, 1875. She gave her history as follows: She first menstruated at 15 years, and had always been regular. Immediately after the first menstruation she married, and in the 24 years of her married life has been delivered at term eight times, and miscarried three times, the last 15 months ago. After this miscarriage she was irregular for several months, and menstruated the last time, May 10, 1874. During the following summer she suffered very much from nausea, and noticed an increasing abdominal enlargement, for which reasons, being well versed in these matters, she considered herself pregnant. She continued to enlarge very rapidly, unusually so, she thought, but was in good health, when, towards the end of September, unexpected

family troubles brought on excessive and protracted mental excitement, in the course of which she noticed that the motions of the child, which she had begun to feel indistinctly some two weeks previously, after being unusually violent for a short time, gradually diminished, and then ceased entirely. A few days later, that is about Oct. 1st, a quite profuse discharge of watery fluid from the vagina took place, which gradually ceased, the distention of the abdomen decreased somewhat, and it has since remained perfectly stationary at its present size. The patient regained her perfect health and spirits, the supposed pregnancy did not manifest itself by any sign, except the abdominal enlargement, and she began to suspect that she had been mistaken in thinking herself pregnant. Still as month after month passed by, and the abdominal tumor remained unchanged, she became partly curious, partly anxious to know what it was, and consulted Dr. S. Teller, of the above Dispensary, who, after hearing her history and examining her, told her that he thought she had either a tumor or a fœtus in the uterus, but that he was not certain as to which it was, and advised her to consult me, as physician in charge of the Gynecological Department.

Having only a few days before corrected the proof of an article on "Retention of the Ovary after the Death of the Fœtus," by Mackenzie Johnston, M.D., of Galveston, Texas, this anomaly was fresh in my mind, and it at once occurred to me that this might be a case of the kind. The subsequent events proved the truth of the statement made by Duncan in regard to these cases, that "the diagnosis will be easily made if the practitioner only suspects the true nature of the case, and is thus induced to investigate it." On examination, the abdomen was found to be enlarged, corresponding to the fifth month of pregnancy, and a tumor with an irregular nodulated outline at its upper and right portion was easily palpable through the abdominal walls; it reached to within two inches of the umbilicus, was tolerably firm and hard, less so than a fibroid tumor, but apparently more so than a normal non-contracted pregnant uterus of that period, the natural elasticity of which was also wanting. Per vaginam I found the cervical canal sufficiently dilated to admit the passage of two fingers through the internal os, immediately above which I could just reach with the tip of my middle-finger a soft body of apparently considerable size. By bimanual manipulation, a connection of the cervix with the irregular nodular tumor on the right side was easily discernible. Judging from the physical signs, especially in connection with the early history of the case, as given me by an intelligent woman, well acquainted with the

symptoms of pregnancy, I came to the conclusion that the uterus contained an ovum, that that ovum was dead, and that its death dated from the time of the cessation of the motions of the child some four and a half months previously. My reasons for considering the foetus to be dead were based partly on the absence of any of the signs of foetal life, as ascertained by auscultation (the foetal heart, uterine murmur) and palpation (the foetal motions), and partly on a peculiar firm non-elastic sensation, imparted by the abdominal tumor, now recognized as the uterus, to the palpating fingers; the symptoms described by the patient supported this view. Since the diagnosis, however, could be merely probable, and would be positive only when it became possible to examine the uterine contents more closely, also admitting the possibility of the intranterine tumor being a submucous fibroid, although the absence of hemorrhage, or any discharge whatever, rendered this supposition extremely improbable, I determined to bring the uterine contents somewhat more within reach by exciting uterine contractions. I was first inclined to accomplish this object by dilating the cervix with Molesworth's dilator, or with sponge-tents; but not possessing the former instrument, I determined to temporize, and to try the effects of ergot for a few days. I therefore gave the patient ten pills, each containing one grain of Squibb's semi-solid aqueous extract of ergot, one to be taken three times a day, and told her that it was necessary to open the womb still more in order to give a positive opinion as to the nature of her trouble; that the womb contained a tumor which, as far as I could then tell, could be only one of two things, either an intranterine tumor, or a dead child retained since last Fall, in all probability the latter. Three days later she returned, having taken all the ergot, and told me that on the previous afternoon she had experienced quite severe abdominal pains, similar to those of labor, but that they had subsided spontaneously. I was pleased to find the ergot acting so well, seeing in the ready response of the uterus to the drug a confirmation of my presumptive diagnosis, and, finding the cervix slightly more dilated, softer and more moist, I gave her six suppositories of the aqueous extract of ergot, containing five grains each, equal to thirty grains of the powdered ergot, one to be taken night and morning. She took only two of these, the second already producing such violent contractions of the uterus, that on the next day at 5 P.M. I was called in haste, and found the patient in labor, the os already about half dilated, and the ovum protruding from it. The pains were very violent, and succeeded in the course of an hour in expelling the ovum, which, by dint of careful manipulation, I

contrived to remove intact as here exhibited. It represents an ovum of about the beginning of the fifth month, with uninjured membranes and the placenta attached. The latter has a white, homogeneous appearance, and would seem to have been very loosely attached to the uterus; on section, and under the microscope, it presents an exquisite picture of fatty degeneration. But a few drops of blood were lost after the expulsion of the ovum; the uterus contracted well, and the patient made a good recovery. At no time, during labor, at the moment of the expulsion of the ovum, or during the puerperal state, did the discharge from the vagina possess a putrid or offensive character; the lochia were very scanty, lasting less than a week, and not at all offensive. The ovum was apparently perfectly fresh and inoffensive, although the peculiar appearance of the placenta, the ease with which the chorion could be separated into its different layers, and from the amnion, and the flaccidity of the foetus, proved, without doubt, that the devitalization of the ovum (a term used by Davis) was not of recent date.

The whole ovum, as here presented, is about the size of a cocoa-nut; the placenta measures 5" by  $3\frac{1}{2}$ ". The ovum corresponds perfectly in size and appearance with the age it must have attained at the time the mental excitement reported by the mother occurred, to which doubtless the death of the foetus at that period is to be attributed. In all probability the placenta still continued to grow for some time after the death of the child.

On opening the ovisac, you see a well-preserved, but somewhat shrivelled foetus, the length of which from vertex to heel is  $9\frac{1}{4}$ ", from vertex to umbilicus  $5\frac{1}{4}$ ", thus corresponding with the measurements given by Scanzoni (*Lehrb. der Geburtsh.*, I., 1867) for the period of four or five months of intruterine life. The umbilical cord is quite long, its spirals are very short and sharply twisted, and it is wound tightly around the abdomen and back of the foetus, forming a deep groove, a circumstance perhaps material in causing the death of the foetus. The latter, as you perceive, is perfectly fresh and inoffensive.

Regarding my treatment of the case, there are two points to which I wish to refer more particularly. First, in producing uterine contractions, I was perfectly aware that I was running the risk of bringing on the premature delivery of a living foetus, for the occasional uncertainty of our means of diagnosis of the life or death of the foetus in utero are well known to me. Denham, at a late meeting of the Dublin Obstetrical Society (Obst. J. Gr. Br. and Irel., Feb., 1875), during the discussion of a case of retained ovum, reported by McClintock,

says: "If we can come to the conclusion that the foetus has perished, there is no difficulty as to our line of practice, but that is a most difficult point to determine." My reasons for believing the foetus dead, I have already mentioned; suffice it to add that, taking the history, physical signs and general impression of the case together, a certain *je ne sais quoi* made me positive that my presumptive diagnosis of a retained devitalized ovum would prove correct. The second point is, that I now regret not having temporized and deferred active interference for several weeks, which I could easily have done, as the patient was in perfect health, until the normal period of gestation had arrived or passed, in order to test the truth of the universal belief that a foetus dying during the earlier half of pregnancy is never retained beyond the normal limit of that pregnancy, but is always expelled at the end of the ninth month. There being still twelve days wanting from the day when the patient consulted me until the day when her menstruation would have returned the tenth time (that is, from May 10, 1874, to Feb. 10, 1875), it is more than probable that she would have gone on to that date without disturbance; whether still farther cannot, of course, be decided; but judging from the dilatation of the cervix and the ease with which uterine contractions were excited, I am under the impression that labor would have come on spontaneously at or about term.

There have been a number of cases of this kind reported. Indeed, in the transactions of this Society for 1871, I find cases reported by Drs. Jacobi, Peaslee and Noeggerath. Still they are of sufficient rarity and interest to warrant my reading to the Society a few notes on the subject which I have been able to collect, principally from the report of a case by Dr. McClintock in the Dublin Obstetrical Society (*loc. cit.*) and the discussion which followed, and from that personification of *multum in parvo*, Schroeder's Compendium of Midwifery.

McClintock (*loc. cit.*) says that there are many exceptions to the rule that an ovum is expelled soon after its death within a few hours or days, and that these exceptions "form a group of very troublesome, perplexing cases, severely testing the patience and diagnostic skill of the practitioner."

It is not very uncommon to find an ovum retained for several weeks after its death. I refer, of course, only to the whole ovum in this paper; the retention of portions of the ovum (placenta, membranes) is of much more frequent occurrence, and belongs in a different category. Some authors, Lecieux and others, assert that the uterus will not tolerate the presence of a dead ovum for a longer period than from five to twenty days, but there are numerous cases on record to disprove this



statement. Dr. J. A. Byrne remarked at the meeting of the Dublin Obst. Soc., already referred to, that in his experience "as a general rule the ova were not expelled for a considerable time after they had perished." I am surprised to find Dr. McClintock stating at the same meeting that he agrees with the opinion of Ramsbotham, that "the ninth month of pregnancy is the outside limit of retention of the blighted fruit of conception." At the same time he says that he does not "see any reason, *à priori*, why an early ovum should not be retained beyond the 9 months, when a foetus dying near the full time may be carried much beyond the ordinary period of gestation, as shown by the cases recorded by himself and Dr. Oldham, under the designation of 'missed labor.'" I confess I do not see the necessity of applying a different term to those cases in which the nearly full-grown foetus dies, and is retained long beyond term, than is used for those in which the ovum perishes at an early age and is likewise retained for an indefinite period.

Cases in which the ovum was retained for several months after the death of the foetus, but was expelled at or before term, have been reported by Young (Trans. Edinb. Obst. Soc., 1870, p. 29), retention for two months and ten days; Pridie (e. l., p. 34), and Newman and Harley (London Obst. Tr., II., p. 251), Montgomery, Atthill (Tr. Dublin Obst. Soc., Obst. J. Gr. Br. and Irel., Feb., 1875), three months; Sir Charles M. Clarke, Jacobi, Peaslee (Amer. J. Obst., Nov., 1871), Mackenzie Johnston (Ibid., Feb., 1875), four months; Graily Hewitt (Tr. Lond. Obst. Soc., III.), Markoe (N. Y. Med. Record, March 2, 1868), Chailly (Midwifery), Meigs (Females and their Diseases), McClintock (Dublin Obst. Soc. Tr., loc. cit.), five months; Ingleby, McCall (Tr. Phila. Obst. Soc., Am. J. Obst., May, 1873), six months; Montgomery, six and a half months; McClintock (loc. cit.), seven months.

Cases of retention of the ovum beyond the normal end of pregnancy, so called "missed labor," are less common. The foetus may shrivel or mummify and be retained for an indefinite period. Manget (Bibl. med. pract., B. III., Geneva, 1696, p. 814) reports an observation by Langelott of a case in which the foetus perished in the fifth month, and was not expelled until during the twelfth month in a mummified condition. Johns (Dubl. Quart. J., Aug., 1855, p. 63) observed two cases in which the foetuses died at the sixth month, and were not expelled until five and six months respectively after their death. Olshausen (Berl. Klin. W., 1871, No. 1) reports a case of the retention of a mummified three months' foetus for eight and a half months. McMahon (Med. Chir. Review, No. 89, Jan., 1870, p. 278) relates a case in which a foetus of four months

was retained in the uterine cavity for eighteen months, and was then expelled enclosed in a compressed placenta, which had evidently continued growing for some time after the death of the foetus. A number of similar cases in which the placenta (not only the maternal but also the foetal portion, the villi of the chorion) retained its vitality and increased in size after the death of the foetus are on record. A case, in which the ovum corresponded in size to the sixth month, but contained a foetus only three-quarter centimetre in length, and was not expelled until thirteen months after conception, was reported by Schroeder in Scanzoni's *Beiträge*, Vol. VII., which contains also cases of the same kind by Dohrn and Hegar. Churchill (Ranking's Abstract, No. XIV.) describes an instance of the expulsion at term of a full-sized and well-formed placenta without a foetus or umbilical cord, the foetal surface of the placenta being level and the vessels terminating in a round knot. No doubt the minute foetus here disappeared entirely in the liquor amnii, or escaped unobserved during several hemorrhages which occurred in the course of the pregnancy. As a rule, if an ovum nearer term dies and is retained in utero indefinitely, at some subsequent period putrefaction and decomposition of the foetus are prone to occur, which may either cause the death of the mother from septicæmia or marasmus, or result in the gradual elimination of the foetal bones piecemeal per vaginam. In younger ova putrefaction of the dead product of conception is decidedly the exception, the foetus being usually expelled unchanged in an entirely fresh and inoffensive condition, as in our case, or shrivelled, mummified and reduced in size. Schacher (Universitätsprogramm, Leipzig, 1717) describes the case of the expulsion of a fresh foetus of three months with its placenta in the thirty-second week of gestation; Stephen (*Am. J. Med. Sc.*, April, 1870) observed the expulsion of an ovum of six weeks five months after its death; Warner (*Boston Gyn. J.*, Vol. V.) removed a perfectly fresh ovum of two to three months, on account of severe leucorrhœal discharge, seven months after its death. McClintock (*Dub. Quart. J.*, Aug., 1870) likewise delivered a woman of a well-preserved fresh embryo five and a half months after its death. Holst (*Beiträge*, I., p. 192) reports the expulsion of a perfectly fresh ovum of four months, twenty-four weeks later, at the normal end of the pregnancy. Cases of this kind are not very rare, and many more could doubtless easily be discovered in the journals. Instances of the discharge of foetal bones from the uterus years after conception, or of the retention of the calcified or mummified foetus in the uterus, until its presence was accidentally discovered after the death of the mother many years later, are, however, of very uncommon occurrence,

and are looked upon as very unusual freaks of nature. Voigtel (*Handb. d. path. Anatom.*, III., p. 519) cites a case observed by Schulz, in which, after a pregnancy of nine years' duration, 128 foetal bones were gradually discharged from the uterus. McClin-tock (*Dublin Quart. J.*, Feb., 1864), over a year after the normal end of the gestation, removed from the uterus the bones of a foetus which perished in the seventh month of its intrauterine existence. Simpson (*Edinb. Med. J.*, Dec., 1865) reports a pregnancy of twelve months, during which bones and purulent fluid were discharged from the os uteri; at the autopsy the foetus was found covered with a substance similar to adipocere, and forming a solid mass adherent throughout to the internal surface of the uterus, and the latter communicating with the transverse colon. A. Halley and H. Davis (*Obstetr. Jr.*, IX., p. 90) report the case of a woman who in the second half of her pregnancy had a brownish discharge from the vagina, and occasionally lost putrid fleshy masses, at times accompanied by bones. Four years later the os was artificially dilated, and eighty-six bones removed in two sittings.

Cases of the last-named class are not uncommon in cows and sheep, the lithopedia of which animals are found in many pathological collections. In the human species there are but few cases reported. Menzies (*Glasgow Med. Jour.*, Vol. I., No. 2, 1853) observed a case complicated with cancer of the cervix, in which the mother died of subacute peritonitis as early as the seventeenth month of the pregnancy, and the foetus was found still well preserved, not having had time to undergo the calcareous or desiccative changes. Voigtel (l. c.) reports the following cases: Albosius found a lithopedium in the body of a woman who had been pregnant twenty-eight years before, and had never been delivered. Camerarius found a calcified ovum, weighing eight pounds, with a mummified foetus enclosed in the uterus of a woman 94 years of age. Prael (*De foetu duodeviginti annos in utero detento*, Gott., 1821) relates a case in which the patient died, after a retention of twenty-eight years, in consequence of the putrefaction of the child still contained in the uterus. Hecker (*M. f. Geb.*, B. 13, p. 112) mentions two cases, one observed by Mühlbeck, in which, in a woman pregnant for fourteen and a half years, the mummified foetus was found in the shrivelled, indurated uterus; and the other, by Caldwell, in which a calcified foetus was found closely attached to the internal surface of the uterus of a woman 60 years of age.

The foregoing cases sufficiently show that there is no positive limit to the time of retention of an ovum within the uterus after its death, that period varying from a few hours, days, or weeks,

to many years (in the case of Camerarius probably more than fifty years).

To determine the length of time which an ovum has been retained from the mere size or appearance of the ovum is not possible, as is shown by the cases demonstrating the fresh condition of the ovum after long retention and the non-correspondence in size between the foetus and the placenta. All we can say with certainty is, that the foetus (not the whole ovum) at the time of its death had arrived at such and such an age; how long it was retained must be determined by the history of the case, which, of course, in a doubtful case, or in one where there is reason to suspect wilful deception, would be of very little value. The personal knowledge of the existence of pregnancy and of the death and retention of the ovum will, in such cases, be the only absolutely certain means of correctly estimating the duration of the pregnancy. In our case there was no reason whatever to doubt the veracity of the patient, and consequently the age of the foetus and the period of retention require no further proof.

The importance, from a medico-legal point of view, of correctly diagnosing and appreciating these cases is self-evident. Cases in which the expulsion of a young ovum many months after the departure of the husband threatened the fair fame of the mother are related by Montgomery, Johns, and others.

The *diagnosis* of the retention of a dead ovum is usually beset with many difficulties; as Montgomery says, there is "no class of cases more unsatisfactory or puzzling." The history of the patient is generally similar to the one given in our case: The woman supposes herself pregnant; she has all the signs of pregnancy for several months; these signs gradually or suddenly cease, with or without watery or hemorrhagic discharges; the health of the patient becomes impaired (in some cases it remains perfectly good, as in our case), frequently sanguineous or fetid evacuations take place from the vagina, perhaps simulating menstruation, and the patient, entirely at loss what the matter is, and doubtful whether she really was pregnant, consults a physician. His first duty now is to determine whether the woman is pregnant or not, whether the uterus contains an ovum; and this having been decided, the next, not less important and frequently even more difficult step, is to ascertain whether the ovum is living or dead. It is not necessary for me to discuss here the means of recognizing gravidity; they are sufficiently known, and I will only say that the external and internal appearances will, of course, vary in accordance with the period of pregnancy, if that state be found to be present. As regards the condition of the foetus, it is to be assumed that if



there is a discharge from the vagina, and that discharge is fetid in character, the death of the ovum may be considered certain, but unfortunately such a discharge is rather the exception than the rule. In cases where there is no discharge at all, or merely a serous or sanguineous flow, the physician is obliged to depend on his judgment and whatever facts he may derive and inferences he may draw from the examination and history of the case. McClintock (*loc. cit.*) says, as regards the advisability of placing any amount of belief in the statements of the patient, that her assertion, that she is still carrying the child, will frequently prove to be correct, notwithstanding the cessation of all the signs of pregnancy, and that he would, therefore, not altogether shut his ears to the earnest representations of the patient on this point. Occasionally the physician is obliged to trust, in a certain degree, to chance, having perhaps no other positive reason for considering the foetus dead than the firm conviction that such is the case. Errors in this respect have, however, occurred to the most experienced and celebrated obstetricians. McClintock (*l. c.*) mentions being present at a consultation with six distinguished accoucheurs, none of whom could say whether the foetus was dead or living. Treatment being adopted for a dead foetus resulted in the expulsion of a living child.

The diagnosis of the retention of a dead foetus having been made, what is the *prognosis*? As a rule, if the woman has not suffered from exhausting hemorrhages or discharges, if the ovum is fresh, and there is no sign of putrid decomposition of the uterine contents and subsequent possible septicæmia, the probabilities are, the uteroplacental vessels having become more or less obliterated, that the ovum will be expelled with but slight hemorrhage, and the woman make a good recovery. If, on the other hand, the conditions mentioned are present, a fatal termination is to be feared. Still, according to Charles Johnson and McClintock (*l. c.*), when the putridity of the discharge has once become decidedly developed, there is seldom any considerable amount of hemorrhage, and this circumstance may be considered to be, at least, a slight consolation under the worse affliction of possible septicæmic infection. "I believe it may be laid down as an aphorism, that the more advanced the development of the ovum, the more likely is the retention to be productive of ill consequences to the health of the patient" (McClintock, *l. c.*).

Why in some cases putrefaction of the ovum takes place, and in others it remains fresh and inoffensive, is a question not fully decided in all its points. That an unbroken condition of the membranes and the consequent exclusion of air from the interior of the ovum is the chief cause why decompo-



sition of the ovum does not take place, is probably well settled, but still it appears strange that an entirely foreign body, as the dead ovum, certainly at least the dead foetus is, should be allowed to remain in a cavity more or less open to the access of air, like that of the uterus, without undergoing decomposition. Again, entirely detached portions of the chorion and placenta have been retained in the uterus for some time without becoming putrid. Scanzoni attributes the preservation of the foetus within the membranes to the influence of the salts contained in the liquor amnii, which in course of time, as it were, pickle the foetus in the same manner as herrings are preserved in brine. I suppose this explanation may be considered sufficient for some cases; for others, it is certainly inadequate, and another reason is required, which I confess myself unable to supply.

In conclusion let me say a few words about the *treatment*. The diagnosis once made of the existence of pregnancy, and the life or death of the foetus, the therapeutic indications are generally clear. If hemorrhagic or fetid discharges, or exhaustion, call for speedy evacuation of the uterus, the dilatation of the cervix by artificial means and the administration of ergot are the measures to be at once adopted. General and local antiseptic treatment should also be employed in appropriate cases, or the vagina may be plugged until the os dilates sufficiently to permit the expulsion of the ovum, or its extraction by instruments. Occasionally the condition of the patient will allow us to follow the expectant treatment, which plan will usually result in the spontaneous expulsion of the ovum at term. In the case, which forms the basis of this paper, this would have been the proper treatment, and would have been adopted, had the patient not desired urgently to be informed of the nature of her disease, and, if possible, to be relieved at once. Thus ergot was given partly for the purpose of aiding the diagnosis, and partly to expel the contents of the uterus. To dilate the uterus, however, without feeling tolerably certain of the death of the foetus, would be unjustifiable, unless, of course, as might happen in ordinary abortion, the violence of the hemorrhage peremptorily demands such interference. The great difficulty of deciding this vital point has already been referred to, and forms, without doubt, the most difficult feature of this interesting class of cases.

Dr. POOLEY asked whether in making the diagnosis of pregnancy, the sign of ballotement had been sought for and found.

Dr. MUNDE answered that he never examined a case of suspected pregnancy without making use of this diagnostic sign;

that he had not mentioned it specially, because he had not been able to detect it, the uterus feeling doughy and inelastic, both to the external and internal touch.

Dr. WYLIE, who was present as a guest, asked whether the sound had not been thought of as a means of diagnosis between pregnancy and an intra-uterine fibroid.

Dr. MUNDÉ said that he had not thought of using the sound in this case, because it seemed to him more practical and simple to induce uterine contractions and dilate the cervix at once, which treatment could be productive of no worse results than might follow the passage of the sound in case there were pregnancy. To be sure, the puncture of the tumor (the membranes) with the sound would in this case by the discharge of liquor amnii have revealed the true nature of the case.

Dr. MANN said that Dr. Mundé had neglected to employ a means of ascertaining the life or death of the foetus, viz.: the measurement of the intra-uterine temperature, which, according to recent experiments by Cohnstein, Fehling and Schroeder, measures  $0.9^{\circ}$  more than the ordinary vaginal or uterine temperature, when a living foetus is present, than when the foetus is dead or there is no pregnancy.

Dr. MUNDÉ said that he was acquainted with the observations quoted by Dr. Mann, and acknowledged their utility in the case reported by him, but that he felt so certain of the diagnosis that it never occurred to him to use intra-uterine thermometry as a confirmatory test.

Dr. PEASLEE presented a

#### SPECIMEN OF FLUID FROM AN OVARIAN CYST,

which he had removed by tapping with a small-sized canula in preference to the aspirator. He had expected to find one large cyst, with several smaller ones near the pedicle, but in tapping he found that it was a large polycystic tumor, from the two prominent cysts of which he drew about three quarts of bloody, turbid, highly albuminous fluid. These two large cysts were found to be inflamed, as already shown by the fever and high temperature of the patient, for which reason the paracentesis was performed. In case the inflammation should increase after the tapping, the removal of the tumor is the only chance for the patient; indeed, a polycyst should never be tapped unless the operation of ovariectomy can be performed within twenty-four hours, which will generally be found to be necessary. He thinks that the aspirator in these cases is no safer than the ordinary trocar and canula, and that it takes longer to evacuate the fluid with the former, while it makes but little

difference whether the puncture is slightly larger or smaller. In a case on which he is going to operate to-morrow, inflammation of the cyst followed tapping with the aspirator within forty-eight hours.

Dr. WALKER showed a specimen of clear, limpid

FLUID FROM A CYST OF THE BROAD LIGAMENT,

which was a portion of twenty-seven pounds of fluid removed from a cyst of this kind, which had been growing some four or five years. The case is now doing well. It is interesting on account of the unusual size of the tumor.

Dr. PEASLEE said that cysts of the broad ligament grow very slowly, and the patients remain blooming and fresh in color, and do not present the emaciation of the countenance and upper part of the body characteristic of ovarian disease. He has tapped six cysts of the broad ligament, none of which returned, except one which returned partly, and then disappeared permanently. This was eight years ago. Cysts of the broad ligament should therefore never be operated on, although errors in diagnosis are not always to be avoided, and might happen to him or anybody else. One of these cysts contained thirty-three to thirty-four pounds of fluid.

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STATED MEETING, OCTOBER 5, 1875. THE PRESIDENT, DR. BYRNE, IN THE CHAIR.

Dr. LUSK presented the specimen of an

ACARDIACUS

from the practice of Dr. R. C. F. COMBS. The following anatomical description of the specimen is by Dr. John A. Wyeth: Male fœtus; lower extremities well developed, the upper partly so. Umbilical cord at the junction of the first and second thirds of the broad fleshy trunk. The bulk of the trunk consists of flabby, œdematous connective tissue.

The collection of bony particles at the superior extremity of the vertebral column is, doubtless, the rudimentary cranium. The rudimentary membranes of the spinal cord extend from this to the canal below.

Three cervical vertebræ can be distinguished, the superior one of which bears strong resemblance to the adult atlas in shape.

Twelve dorsal vertebræ, with as many ribs.

The lumbar vertebræ and sacrum appear normal.

On the right side the clavicle, scapula, and humerus were well marked. The radius and ulna were represented by a small

block or mass of cartilage, with three metocarpal bones extending from its lower extremity.

Three phalanges, with the last two bones of each digit not developed.

Diaphragm, though rudimentary, was well marked.

The biceps and triceps muscles of the right arm were distinct.

The psoas magnus was large, and the lumbar nerves were found.

The intestinal canal was traced from the rectum to the diaphragm, where it terminated in an open extremity.

There were no viscera found except the bladder, which was connected with the umbilicus : no testicles.

DR. L. A. RODENSTEIN exhibited the photograph of a

CASE OF ARREST OF DEVELOPMENT OF BOTH UPPER EXTREMITIES, from the practice of Dr. G. W. SNOW, of Newburyport, Mass. The history of the case and description of the deformity are as follows :

September 4th, 1873, delivered Mrs. M., primipara, of female



child in a vertex presentation. The child appeared to be strong, and well developed in every respect except the arms. The hands project from the shoulders like the fins of a dolphin.

The scapulæ and clavicles are well formed, and there was evidently an attempt to produce the bones of the arms. I thought I could distinguish the humeri, ulnæ, and radii. Carpal and metacarpal bones natural. Hands well developed and perfect. Rotation at the shoulder joint is impaired. The motion is more like the hinge-joint, similar to motion at wrist-joint. Could not detect any joint at the elbow, or distinguish the dividing line between the humerus and ulna and radius. Distance from glenoid cavity of scapula to carpal bones about  $1\frac{1}{2}$  inches.

The mother made a good convalescence, though without having any flow of milk, and it was necessary to feed the baby from a bottle.

The child died in July last from cholera infantum, aged ten months. No post-mortem.

DR. PAUL F. MUNDÉ presented a

CASE OF SUPPOSED TRUE LATERAL HERMAPHRODISM.<sup>1</sup>

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STATED MEETING, NOVEMBER 2, 1875, THE PRESIDENT, DR. BYRNE, IN THE CHAIR.

ADENO-SARCOMA OF BOTH OVARIES; DOUBLE OVARIOTOMY; TRANSFUSION OF MILK; RECOVERY.

DR. T. G. THOMAS presented two solid tumors of the ovary, both removed from the same patient, whose history he related as follows:

Three weeks ago he was consulted by a lady, thirty-two years of age, the mother of three children, the youngest of which is seventeen months. The patient had always enjoyed good health until the birth of her last child, after which she gradually lost strength, suffered from night sweats, and became very much emaciated. She consulted Dr. Clark, of Oswego, who made an examination and discovered a solid tumor of the right ovary of the size of a hen's egg, which he thought malignant in character. The tumor grew with moderate rapidity until it reached the size of the larger one exhibited, plus about one-third lost by shrinkage since its immersion in absolute alcohol, *i.e.*, about the size of an adult head. The patient in the meanwhile consulted Dr. Chauncey L. Mitchell, of Brooklyn, where she resided, and Dr. Atlee, of Philadelphia, the latter of whom said that the tumor was malignant, and that an operation for its removal would be extremely hazardous. When Dr. Thomas first saw the patient

<sup>1</sup> See this number of the JOURNAL.



she was exceedingly feeble and emaciated, appearing like a person suffering from diabetes. The abdomen was tender to the touch and distended by a tumor, which reached above the umbilicus. The general and local features of the case reminded him of two cases of adenoma of the ovary which had previously occurred to him; he therefore made that diagnosis, and dissented from Dr. Atlee as to the chance of recovery, telling the friends of the patient that there were ninety chances out of one hundred against her, but probably ten in her favor. Wishing to make a further examination in the presence of several gentlemen of this city, Dr. Thomas requested the patient to call at his office again, but she was so exhausted by her first visit, that she was unable to comply with his request. The friends were very anxious for the removal of the tumor, and the operation was therefore fixed for Thursday, October 14th, and performed at 3 P.M. on that day. On opening the abdomen, which contained no ascitic fluid, a large solid tumor of the right ovary was found, the pedicle of which was first secured by a clamp, which was subsequently removed, however, and its place supplied by a ligature, the pedicle being then dropped. The left ovary was found in Douglas's cul-de-sac, pushing the uterus forward. Previous to the operation Dr. Thomas had thought this tumor behind the uterus to be a portion of the large tumor, and probably adherent, which fact would have rendered the prognosis still less favorable. The left ovary was removed, the pedicle ligated and dropped. The duration of the operation was only thirty-six minutes, which was fortunate, as it is important not to keep the abdominal cavity open too long, or the patient for a long while under ether. After the operation the patient was seized with vomiting, which continued until the following Saturday, and obliged nutrition to be performed entirely by the rectum. On Saturday she had a severe metrorrhagia (after having been amenorrhoeic for the last three months), and became very much prostrated, pulse 140, temperature  $101^{\circ}$ , no febrile reaction. On Sunday Dr. Thomas left for Rhinebeck, where he had an operation to perform, leaving Dr. S. B. Jones in charge of the patient. In the afternoon he received a telegram that she was sinking, and apparently near death. During the night, however, she rallied somewhat, and appeared slightly better when he saw her on Monday morning. In the evening, between 6 and 7 o'clock, however, he received a despatch, saying that she was sinking rapidly. He hurried to Brooklyn, arriving there at 8 o'clock, and found the patient bathed in a cold, clammy perspiration, and exceedingly collapsed, the pulse 142-145, sometimes entirely lost at the wrist; and in his opinion, and that of the at-

tendants, the end approaching. Preparations had already been made to perform transfusion with milk in case of necessity, for which Dr. Thomas had left directions in the morning; an Alderney cow was driven into the yard and milked into a pitcher covered with gauze, the pitcher standing in a pail of warm water. All impurities were thus prevented from entering the milk, which was kept at a proper temperature by the warm water in which it stood. The canula was introduced into the median basilic vein, and nine ounces of milk were gradually injected. Dr. Thomas used the transfusion apparatus devised by Robert & Colin, of Paris, which consists of a large funnel, into which the fluid to be transfused is poured, at the bottom of which funnel is an opening connecting with a syringe, to which is attached the tube leading to the canula in the vein of the patient; closing this opening in the bottom of the funnel is a movable ball of aluminium, which, being lighter than any fluid, but heavier than air, when the mouth of the funnel is turned upward, by its own gravity effectually closes the opening against the entrance of air with the fluid when the piston of the syringe is drawn back in filling, or pushed forward in emptying the syringe. This ingenious contrivance was used with great satisfaction in this case. After the injection of a few ounces the patient experienced no sensation whatever, the pulse being feeble and beating 160-170; but when six ounces had been introduced, she at once complained that her head felt like bursting, a rigor came on, followed by high temperature, the pulse beating 152-155. These symptoms continued for some little time after the completion of the transfusion of the nine ounces of milk. One hour afterwards she fell into a sound sleep, which continued all night, and from which she was not aroused for the purpose of giving her nourishment, because she had latterly been unable to retain anything administered either by the stomach or rectum. The next morning, Tuesday, she was slightly delirious, but much stronger, the pulse 116; she said that she felt "as though she were going to get well." From that time she improved steadily, and is now out of bed and out of danger, and doing well.

Dr. Thomas said he would not positively assert that the transfusion of milk saved the life of the patient, but his firm conviction is that it did.

The tumors were both of the same structure, with a dense, fibrous feel, but still not like a uterine fibroid. The larger shows a deep sulcus in which runs the Fallopian tube; the smaller is about the size and shape of a kidney, its surface like that of a cirrhotic liver in appearance. Specimens of the larger tumor were sent to four gentlemen for microscopic

examination, two of whom reported it to be adeno-sarcoma, a mixture of embryological ovarian elements and sarcomatous cells, and the other two simple adenoma.

DR. JAMES R. CHADWICK, of Boston, who was present as a guest, read a paper on

THE INJECTION OF FLUIDS THROUGH THE ABDOMINAL WALLS INTO THE INTESTINE.<sup>1</sup>

DR. MANN asked whether the injection could not be made as well into the small intestine.

DR. CHADWICK said that the colon was generally the most distended portion during peritonitis, and therefore most easily discernible.

DR. JANVRIN asked whether there would be any objection to leaving the canula in the intestine for some time for the purpose of prolonged nourishment or medication.

DR. CHADWICK answered that the danger of causing a permanent gastro-intestinal fistula, or of the canula slipping out of the intestine in consequence of peristaltic action, should be borne in mind as contra-indications of the prolonged retention of the tube.

DR. LUSK mentioned a case in which a patient of his was nourished for seventeen days by Leube's pancreatic fluid, introduced into the rectum, and even increased in weight during that period.

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The annual election of officers was held and the following gentlemen were elected:

*President*—Dr. T. Gaillard Thomas.

*Vice-Presidents*—1st, Dr. A. Jacobi; 2d, Dr. C. C. Lee.

*Corresponding Secretary*—Dr. E. Noeggerath.

*Recording Secretary*—Dr. Paul F. Mundé.

*Treasurer*—Dr. G. S. Winston.

*Committee on Admission*—Drs. William T. Lusk, J. B. Reynolds, Charles S. Ward.

*Committee on Publication*—Drs. E. R. Peaslee, H. F. Walker, Paul F. Mundé.

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STATED MEETING, NOVEMBER 13. THE PRESIDENT, DR. T. G. THOMAS, IN THE CHAIR.

DR. NOEGGERATH exhibited to the Society a

<sup>1</sup> See November, 1875, number of this JOURNAL.

UTERINE DOUCHE FOR THE INJECTION OF ALKALINE CARBONIC  
ACID OR PLAIN WATER,

devised and constructed by Mr. Thomas Warker, of this city, and explained its use and indications as follows :

This instrument is intended to represent what is known as the "*Bubenquelle*" or Babyspray of Ems, Germany. It is used extensively in diseases of women, with and without proper indications. It has been used and abused considerably in the past, and is so to the present time. The *Bubenquelle* has had the reputation of being one of the principal factors in Ems to remove sterility in women. Over the spring at Ems, some twenty years ago, was written a rhyme which said, roughly translated :

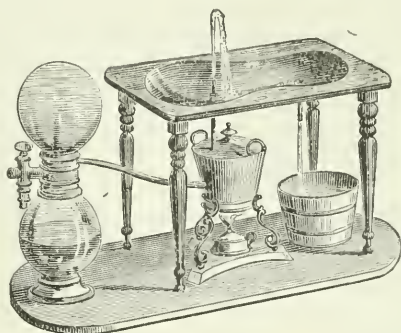
" This spring is the best,  
But it does not act except through a guest."

Now, whatever agency has brought about the reputation of this spring more than another I do not know, but in some instances the spring has accomplished what it was intended to perform—the removal of sterility in women. Still, scientifically speaking, the application and indications for the use of this spring are very limited.

The douche in Ems is a stream of Ems water which, when allowed to develop its strength, produces a jet two feet and a half in height, and about half an inch in diameter. The spring comes out of the ground, and is applied in about the manner you see by this apparatus. There is a bidet used, and the patient sits over the spring and allows the water to get to the genital organs. The indications are restricted to two conditions. First, the spring is applied in cases where there is an absence of nervous energy in the uterus and neighboring organs. There is a set of patients whom every one of you has seen, where there is an absence of sexual desire, and in these cases the use of the warm douche saturated with carbonic acid has had decided effect, and some have said that the first time they ever had sexual excitement was when they sat over it.

Another class of cases treated by this water is vaginismus, and the douche acts in pretty much the same manner as the carbonic acid acts—by soothing. It acts also as a stimulant, and it was thought that the alkalies contained in the water act as alteratives, removing induration present even in the uterus itself, and in the neighboring organs. Now, if you consider the manner in which the douche is used, it is not likely that the water ever touches the uterus, only in cases of an exceptional character where, for instance, the muscles are in a very lax condition. In most instances the water only touches the

outer organs. Still I think there are always (not numerous) cases which call for treatment, for which the remedies at our disposal are insufficient. In cases of absence of nervous excitability and the opposite, we need a remedy that acts just in the same manner as this douche acts; and so for these two groups of cases, the douche is a very efficient remedy.



Mr. Warker has been happy in imitating the douche just as perfectly as it could possibly be performed. You see here he has an apparatus filled with Ems water, and overcharged with carbonic acid, so that the pressure of the gas will expel the water through the aperture, just in the same manner as in Ems itself. And, in order to make it more like the spring, he has constructed an apparatus by which the Ems water is heated to any desired degree. The degree of heat used at Ems is  $80^{\circ}$  Fahrenheit. By pouring hot water into this apparatus, and keeping the water hot by the spirit lamp, the imitation of the Ems pool is about as perfect as it can be made.

Each patient requiring the douche provides herself with a proper bidet, and Mr. Warker proposes to deliver the jars filled with the carbonized water in the same manner as syphons, and call for the empty jars when notified, thus rendering the use of the apparatus exceedingly simple and convenient.

I have a case now of enormous hyperaesthesia, and I shall try the douche and report the effects of it.

DR. CHAMBERLAIN exhibited a very simple, inexpensive, and useful

#### SUBSTITUTE FOR THE ASPIRATOR,

in the shape of an ordinary hard rubber syringe, to which a hard rubber nozzle with two floating valves can be attached; to the central nozzle can be affixed a rubber tube and aspirator needle, from the lateral nozzle another tube to carry away the



aspirated fluid. The expense of the nozzle is \$1.50; it can be obtained of Ford, in this city. He would not hesitate to use the instrument for transfusion, or in any case where one of the usual complicated and inexpensive aspirators is generally used.

Dr. ALEX. J. C. SKENE reported a

SUCCESSFUL CASE OF GASTRO-ELYTROTONY.<sup>1</sup>

Dr. F. D. LENTE, who was present as a guest, said that the large size of the child would readily account for the extension of the laceration into the bladder; it would, therefore, always be well to make as near an estimate of the size of the child as possible before operating.

Dr. THOMAS said that he had experienced no trouble from the bladder in the two cases in which he had operated.

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STATED MEETING, DECEMBER 7TH, 1875. THE PRESIDENT, DR. T. G. THOMAS, IN THE CHAIR.

Dr. HENRY D. NICOLL reported the following case of

COMPLETE INVERSION OF THE UTERUS OF FOUR AND A HALF YEARS' STANDING. REDUCTION ACCOMPLISHED BY TAXIS.

Hannah Goggin, thirty years of age, a widow, a native of Ireland, a laundress, came under my care in the month of July, 1873, with the following history. She began to menstruate at fifteen years of age; was regular until the time of her marriage, at twenty-four years of age (six years previously). Thirteen months subsequent to her marriage, after violent straining in lifting a heavy weight, she had a miscarriage of a three months' foetus, losing a large quantity of blood. Ten months later, she was delivered of a well-developed child at term; the labor being, as she believed, natural, though the physician used considerable force in delivering the placenta. This birth took place four and a half years ago (*i.e.*, previous to July, 1873). She nursed the infant until it was twenty-one months old. Six weeks after its birth, she had a most profuse hemorrhage from the vagina, which continued for several days. Menstruation recommenced when the child was fifteen months old, has occurred at regular intervals, the flow lasting for eight or nine days, and the quantity of blood lost at each period has been very great. The patient is quite free from pain, is very feeble, emaciated, and presents the usual appearance of a person who has been subjected to the repeated loss of large quantities of blood.

Upon physical examination, a solid, ovoid tumor, with a smooth, velvet-like covering, of the size of a hen's egg, is found, filling the upper portion of the vagina, its attachment being at the superior extremity of the vagina; a thin, ring-like

<sup>1</sup> See this number of the JOURNAL.

membrane surrounds and embraces the upper portion of the tumor. It is impossible to introduce a sound beyond this ring. The abdominal walls are very lax, and by conjoined manipulation a very thorough examination of the pelvic viscera can be made. The presence of the uterus above this tumor cannot be detected. The diagnosis of complete inversion of the uterus was made, and it was decided, after consultation, to attempt the replacement of the organ by taxis.

On *July 24th* (her last menstrual period having ended on 21st instant), the patient was anesthetized (sulphuric ether being employed), and the reduction of the uterus attempted, Drs. Sims, Byrne, and Hunter being present. After fifty minutes of careful and well-directed manipulation, each of the gentlemen relieving the others in turn, the organ was returned as far as the os internum, when it was deemed advisable to discontinue further efforts. The vagina was tightly packed with cotton-wool, and the patient put to bed.

*25th.*—Patient passed the night well, but complained of great pain in vagina, from pressure of the cotton; the tampon was removed, when the uterus became completely inverted again. The vagina was syringed with warm water, and a large air pessary was introduced and fully inflated.

*26th.*—Patient has had no unpleasant symptoms; pessary taken out, vagina syringed with warm water, and pessary replaced.

*27th.*—The pessary seemed to be of no advantage in retaining the uterus in its (partially reduced) position, and it was left out; warm-water injections ordered night and morning.

*September 6th.*—Patient was again anesthetized, Drs. Sims and Hunter present. The body of the uterus was soft and pliant, and was readily returned as far as os internum. After thirty-five minutes of constant manipulation (the hand over the abdomen always controlling the action of the hand within the vagina, as on the former occasion), the organ was so far reduced that the fundus was well within the canal; when, by directing the pressure more particularly against the right corner this portion of the organ was forced through the constricting ring, and was followed quickly by the remainder of the body, and the uterus resumed its normal position. The patient was placed in bed.

*7th.*—Patient slept well; no pain; considerable thin bloody discharge from vagina, which disappeared after a few days. Vagina was very gently syringed daily with warm water. She continued to do well. Menstruation occurred on the 16th instant, and was quite normal in every particular; free from pain; flow not too great, lasting for four and a half days.

This inversion occurred four and a half years previous to its reduction, its *probable* cause being undue traction in delivering the placenta. The tampon and the air pessary were of no avail in retaining the partially reduced uterus in its place. The time occupied in reducing the displacement at the second trial was much less (thirty-five minutes) than that spent upon the first occasion (fifty-five minutes), though the greater portion of the organ was replaced at the second effort. It seemed reasonable to ascribe this difference to the use of the warm-water injections.

DR. B. F. DAWSON exhibited a

NEW GALVANO-CAUSTIC BATTERY

devised by him, and demonstrated its power in heating platinum knives and coil.

The advantages claimed over other batteries lie in the principle of construction, and the means for preventing and overcoming what is known as "polarization" of the battery, the occurrence of which in all small batteries has heretofore rendered them unreliable and useless. The battery is composed of but two cells, in each of which are two positive (zincs) and one negative (platinum) plate, all measuring but  $4\frac{1}{2}$  by 6 inches. The zincs are perforated, and adjusted but half an inch apart, and between them a platinum plate is placed and held in position by uprights. On each side of the platinum plates are hard rubber or celluloid pumps or agitators, worked by means of a small knob. The entire battery requires but a quart of fluid, with which amount it will keep up a most powerful action, long enough for the most prolonged operation, by the moving up and down of the pumps, which, according to the intensity of the heat desired, are moved more or less quickly. By this action, the old and exhausted fluid between the plates is thrown out through the perforations, and fresh fluid is made to take its place, thus keeping up a uniform power equal in intensity and constancy to that obtained from the large imported batteries. In consequence of its power it has been found necessary to make special platinum cautery instruments of heavier metal to accompany this battery. The entire battery, with polished wood case, measures  $8\frac{1}{2}$  inches in height, 6 inches in width, and 4 inches in depth from front to back.

Dr. Dawson also showed a new handle and conductor, with isolators to prevent the current from burning the vagina.

DR. NOEGGERATH said that he had used Dr. Dawson's battery, and was astonished at its power; he had used it for thirty minutes, and found it to work better than any other battery of its size. The great objection to it, however, which he was afraid would be difficult to overcome, was the intense heat generated

in the fluid, which boiled over on the table after twenty-five minutes.

DR. DAWSON said that he hoped to obviate this difficulty by putting lead on the top and bottom of the platinum plates, that now on the sides of the plates apparently not being sufficient to abstract the heat from the platinum.

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## TRANSACTIONS OF THE PHILADELPHIA OBSTETRICAL SOCIETY.

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*Reported by* JAMES V. INGHAM, M.D., *Secretary.*

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STATED MEETING, MAY 6, 1875.

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DR. ELLWOOD WILSON read the following paper on

VERSION IN CONTRACTED Pelves,

in reply to the paper on the same subject read by Dr. Wm. Goodell at the meeting of the Society, Feb. 4th, 1875:<sup>1</sup>

MR. PRESIDENT AND GENTLEMEN:—The great interest I feel in the subject now before us for discussion leads me to again appear before you, to urge with what force I may some points in opposition to the treatment of labors with contracted pelves by version, as recommended by Dr. Goodell in the paper read before this Society in February last. At a previous meeting I stated the mortality to the child in pelvic natural labors to be 1 in 3 (Churchill and others); but as this estimate includes all children born dead in pelvic labors, by excluding such cases as were born dead from causes other than those dependent merely upon pelvic labor, we find the mortality in pelvic natural labors to be estimated

By Hodge, 1 in 7.

By Meigs, 1 in 11.

By P. Dubois, 1 in 11.

By Churchill, 1 in a little more than 4.

I stated, further, that the mortality in these cases arises from causes incident to such labors, and cannot be obviated by the adroitness of any expert, namely, interruption to or cessation of the placental functions.

In artificial pelvic labors, without defect in the natural pelvis, the mortality is increased to 28 or 33 per cent., or to 1 in 4, or 1 in 3 (Hodge's table), thus showing that version is a very serious operation to the child.

<sup>1</sup> See the AUGUST, 1875, number of this JOURNAL.

Dr. W. Goodell, in a paper recently read before the "Philadelphia County Medical Society" (*Philada. Med. Times*, March 20, 1875), in alluding to the mortality to the child in ordinary pelvic cases, says: "For this mortality fifty per cent. is, I think, a very low estimate;" but his opinion is offered without statistics, and I regard these figures as much overrated.

While version in contracted pelves gives a fatal result to the child as high as from 66 per cent. to 75 per cent., or from  $\frac{2}{3}$  to  $\frac{3}{4}$ , or from 2 in 3 to 3 in 4 (Capuron, Cazeaux); and as these statistics must necessarily include a large number of cases with but a slight defect in the pelves, as was the case with many of those reported by Dr. Goodell in the paper read before this Society, viz., Case No. 1, pelvis,  $3\frac{1}{2}$ ; Case No. 3, pelvis, +3; Case No. 4, pelvis,  $3\frac{1}{2}$ ; Case No. 5, pelvis,  $3\frac{3}{10}$ ; Case No. 8, pelvis, +3; Case No. 10, pelvis,  $3\frac{1}{2}$ , we can readily understand and fully agree with Velpeau that when the contraction is as great as to  $\frac{5}{8}$  inches, or approaching three inches in its antero-posterior diameter, the mortality may reach the enormous amount of 97 per cent., or nearly all. And many authors emphatically state that when version is performed in pelves of 3 inches or — 3 in the conjugate diameter, the result of the labor is almost always fatal to the child (Cazeaux, Leishman, Chailly-Honoré).

Leishman very positively declares that, with a pelvis of 3 or — 3 inches, version should never be attempted. Cazeaux and others confirm this statement.

Thus, whilst in pelvic natural labors one child in from 7 to 11 dies; in artificial pelvic labors, without defect in pelves, 1 in 3, or thereabouts; in artificial pelvic labors with contracted pelves, 2 in 3 or 3 in 4; and when this contraction amounts to 3 or — 3, nearly or quite all of the children die; on the other hand, in cephalic labors the death-rate to the child is 1 to 30 or 1 to 50. Version is also a painful and dangerous operation to the mother, proving fatal to her in the ratio of 1 to  $10\frac{1}{2}$  (Cazeaux) or 1 in 14 (Churchill). With such results as these, version, in my opinion, should never be made without good and sufficient reason, such as prolapse of the cord, avoidable or unavoidable hemorrhage, etc., when we have a pelvis of such capacity as may permit a woman to give birth to her child at full-term of pregnancy unaided. Now, what is the minimum capacity of a pelvis through which a child, at full-term, may be born by the unaided efforts of the mother? We find it to be, according

To Meigs,  $2\frac{1}{2}$ , —  $2\frac{3}{4}$  inches.

To Cazeaux,  $2\frac{1}{2}$ , —  $2\frac{3}{4}$ , — 3 inches.

To Hodge,  $2\frac{3}{4}$ , — 3 inches.



To Gardien,  $2\frac{1}{2}$ ,  $-2\frac{3}{4}$ ,  $-3$  inches.

To Churchill,  $2\frac{3}{4}$  inches.

To Madame Boivin,  $2\frac{1}{2}$ ,  $-3$  inches.

And in a table of 38 cases of labors with deformed pelves, the last writer informs us that 8 cases occurred naturally. Chailly-Honoré asserts that labor may occur naturally in pelves of  $-3$  inches. And to this array of statistics I add a case of my own of 3 or  $-3$  (see page 678).

If, then, women having pelves of 3 or  $-3$  inches can be delivered of living children by their unaided efforts at full term of pregnancy, and if it be a fact that in contracted pelves, in the great majority of cases, the narrowing is at the superior strait,—as stated by Hodge, Meigs, Simpson, Cazeaux, Leishman, and many others,—and if this narrowing is further merely marginal, since it is caused most frequently by the sharp angle of the promontory of the sacrum dipping too saliently into the brim, we can readily understand how a woman may in this special form of contraction overcome this obstruction by a well-directed and timely effort. Hodge, referring to the compressing power of the forceps to overcome the difficulty in this form of labors, says the compression will be of but short duration. That women do thus accomplish their delivery through pelves of 3,  $+3$  and  $3\frac{1}{2}$  I can testify, the head passing over the point of constriction with an audible sound and a jerk-like sensation (Goodell, Case No. VIII., and others), which I have repeatedly felt while “touching” when conducting such labors, and also while using the forceps. So distinct is this sensation, that I have thought the forceps had slipped. Below this narrowing the pelvis is usually ample, and offers no obstruction to the delivery. Further, pelves narrowed at the superior strait are not symmetrically deformed, one side being more involved than the other; Leishman asserting that “in a majority of deformed pelves there is a want of symmetry.” Authors almost universally admit, that in pelves narrowed at the brim the vertex is found presenting towards one of the lateral sides of the pelvis; this we find to be the fact in the table of cases now under discussion, and it is my own constant experience. This being established, and the pelvic brim being somewhat irregular in its opening, it is evident that the forces operating upon the child’s head, inclining or forcing the vertex to the sides of the pelvis, will, so long as the child is floating in its waters and free to move in its spiral axis, have a tendency to drive the vertex towards the larger or more open side of the pelvis. Every obstetrician has experienced while “touching,” when the membranes were intact and the head not engaged, the great facility and rapidity with which the vertex moves

from one side of the pelvis to the other and back again during a pain. This is an important consideration for us to bear in mind during the management of labors with narrowed pelves, -- whenever the woman is not able to accomplish her task without assistance, -- as the head can be so grasped and brought down in the greatest capacity of the opening.

We have thus, upon abundant testimony, shown that it is possible for women to be delivered unaided at "full term" with pelves of 3 or  $-3$  inches; therefore no woman should be subjected to one of the most serious operations in obstetric surgery (fatal to the mother in one of every  $10\frac{1}{2}$  cases), and one which it is, at times, almost impossible to accomplish, even with the aid of anæsthesia, and which also increases to so great an extent the mortality to the child; when the pelvis is no more contracted than those given in the paper of Dr. Goodell,  $-3\frac{3}{16}$ ,  $3\frac{1}{2}$ , and "somewhat contracted." But if the woman fails to accomplish her delivery through a contracted pelvis, can she be assisted by the forceps without injury to her tissues and with safety to the child? So far as the mother's tissues are involved, the forceps, if skilfully used, should never injure them (Hodge); as for the child, in pelves of  $+3$  and  $3\frac{1}{2}$  the result should generally be favorable; the forceps should never injure the child or mother (Meigs and Hodge).

Does the head flex and the vertex dip into the excavation in narrow pelves? The first process in the mechanism of labor is the increased flexion of the head, caused by the resistance offered by the neck and *os*; and according to Meigs this is equally the case with contracted pelves when the contraction is such as to prevent the head passing into the excavation.

Hodge says, most emphatically, in pelves of three inches the head will flex and the vertex project into the pelvis and become fixed, -- this is in accordance with my own observation. In my opinion, the proper time for using the forceps is when the *os* is dilated or is dilatable (in some cases, where the contraction is great, the head rests upon the brim of the pelvis, and fails to press upon the *os*, which may become soft and yielding, but does not dilate); and as soon as the waters pass off it is found that the vertex is pressed into the excavation, and has become moderately fixed but not jammed. The child at full term weighs  $6\frac{1}{2}$  to  $6\frac{3}{4}$  pounds; its conjugate diameter is  $3\frac{1}{2}$  inches. Now, to what extent may this diameter be lessened or diminished by the natural efforts of the mother, and by the careful interrupted compression of the forceps, with safety to the child?

Siebold says 6 lines.

Osiander, nearly 1 inch.

Velpeau, 6 lines.

Baudelocque, 4 to 6 lines.

Leishman, half an inch.

Meigs, 4 to 6 lines.

Denman, nearly 1 inch.

Hodge, 10 lines.

And I am positive I have seen it reduced 10 lines without injury to the child. The child's head diminished to this extent is brought within the control of the woman's natural efforts, or is reduced to such dimensions as may be readily brought through a pelvis of 3 inches by the forceps.

That the forceps are the proper and safest means for assisting women in difficult labor—where the pelvis is too small, or where the head is too large—has been the almost universally accepted rule of modern practice; a rule apparently unquestioned until the appearance of Sir James Y. Simpson's paper upon "Turning as an Alternative for Craniotomy and the Long Forceps," in 1847.<sup>1</sup>

Dr. Hodge declares that the use of the forceps as a means of delivery in difficult labors is a settled practice.

He taught and practised this method of delivering women, and gives a case in which he delivered a living child through a pelvis of 3 inches, the conjugate diameter of whose head measured  $3\frac{1}{2}$  inches shortly after birth. This is said to be a "typical case"; nevertheless it is a record, and by one whose ability and truthfulness I cannot doubt. In immediate connection with this history he says, with the forceps to the child's head, by compression, and the resistance the head meets with by the sides of the pelvis, the head is moulded, it is elongated in its occipito-mental diameter, the parietal bones overriding each other at the sagittal suture, thus materially shortening the bi-parietal diameter; and we have cause to know many children have thus been born safely who would otherwise have perished. He further states that, with a pelvis of 3 inches, in the antero-posterior diameter, the forceps should be applied to the sides of the child's head, which can be done without accident to the child and with but little or no discomfort to the mother; and that such compression and traction as may, in the judgment of the operator, be requisite to overcome the resistance, must be made until it becomes evident that the child cannot be born, or that it is dead, when perforation should be resorted to. He cites cases where children have been delivered with safety through pelves of  $2\frac{3}{4}$  inches.

This great teacher nowhere in his elaborate work recommends version as a means of delivery in labors with contracted pelves,

<sup>1</sup> *Obstetric Works*, Vol. I., p. 393.

or where the child's head is too large to pass, but especially advises against it, and always recommends the forceps, declaring that delivery can be accomplished with safety to both mother and child, in the great majority of cases, where the pelvis measures 3 inches and +3, and further states that if they fail to accomplish the delivery, the only remaining resource is craniotomy.

Dr. Dewees recommended the forceps for delivery in narrow pelves, and cites cases of successful delivery with pelves of  $2\frac{3}{4}$  inches.

Those of us who have listened to the teachings of another able American obstetrician, Dr. Meigs, know that he taught and practised the feasibility of applying the forceps to the sides of the child's head at the brim of the pelvis, and in his writings he cites cases of successful delivery through pelves of  $2\frac{1}{2}$  and 3 inches. On several occasions I have been present and assisted him in this operation. Dr. Jos. Warrington, another eminently practical teacher, recommended the application of the forceps to the sides of the child's head at the superior strait, and it is on record that he successfully made it. Mr. President, I think I have been present when you adjusted the forceps in this way. Denman, Dubois, Chailly-Honoré, Baudeloeque, Velpeau, and many others have taught and asserted the practicability and success of this mode of using the forceps. During ten years that I was the assistant in the Philadelphia Lying-in Charity, I applied the forceps to the sides of the child's head in all cases where the head was arrested at the brim, either from narrowing of the pelvis or too large a head; and for the eight years that I held the lectureship in that institution I taught, demonstrated, and practised this method. From the time of the organization of this charity until I retired from it, including the eighteen years I had the active management of it, the mortality to the mothers was less than half of one per cent. in a table of nearly seven thousand cases; and there was during that time no case of vesico-vaginal fistula, and but one instance in which the rectum was lacerated, and that occurred in a labor terminated without instruments. I have pursued this course in private practice for thirty years, during which time I have had charge of many cases of labor with contracted pelves, and have had very satisfactory results. I am confident there are many physicians now present who have used the forceps in this manner, and with like results. I will here mention, as confirmatory of the power of the natural efforts of the woman, and also of the efficiency of the forceps, the case of Mrs. McN——, whose pelvis was 3 or —3. The dimensions were carefully taken by myself, with the assistance of two competent physicians, and our joint

opinion confirmed the above measurement. I on two occasions delivered her of living children with Davis's forceps. She had previously been delivered by craniotomy, while under the charge of skilful and conscientious physicians; and it would be extremely unfair to suppose they would have resorted to this mode of delivery except from a conviction of duty. Yet she subsequently did deliver herself at full term of a living child. In still further support of the efficiency and safety of the forceps as a means of delivery in narrow pelves, and their great superiority over version, I refer to the following authorities:

Churchill cites cases in which women have been safely delivered of living children by the forceps with pelves of  $2\frac{3}{4}$  inches, and declares that unless the child can be delivered by forceps in pelves of this capacity there is no resource but embryotomy.

Bandeloeque recommends the forceps in pelves of  $2\frac{1}{2}$  inches and upwards, for the safety of both mother and child.

Cazeaux recommends the forceps for delivery in pelves of  $2\frac{3}{4}$  and 3 inches, saying that version may be resorted to in pelves of  $3\frac{3}{4}$  inches, differing with Simpson, Madame Lachapelle, and Radford, who say that version should be made in narrowed pelves when the child presents favorably and the narrowing is less than  $3\frac{1}{2}$  or  $3\frac{1}{3}$ ; and he further instructs us that it is an especial condition for version, that there be no disproportion between the size of the child's head and the walls of the woman's pelvis.

Smellie recommends the forceps in contracted pelves in preference to version.

Madame Boivin recommends the forceps in contracted pelves.

Chailly-Honoré says that when the vertex presents with a pelvis of 3 inches, the case should be left to nature; and if the woman fails in her efforts, we should use the forceps. He advises their use in pelves of  $2\frac{1}{2}$  inches, and cites successful cases of delivery with pelves of that measurement.

He also informs us that all obstetricians, up to 1866, are agreed to consider malformation of the pelvis as a motive for the exclusion of pelvic version, and continues, "How often have we seen women brought to the hospital with the child's head still held in the pelvis, the trunk having been long before extracted, and the head having resisted energetic traction. I saw a case of this kind in which the trunk of the child was extracted thirty-six hours before the head was delivered."

The author further says that version in contracted pelves almost always endangers the life of the child, no matter how skilful the operator may be. Facts observed every day in our hospitals prove this, and will convince any impartial observer, who will make a careful examination of the laws which regu-



late the passage of the child's head through a contracted pelvis. If the head presents first the expulsion may occur spontaneously, even if the diameter of the pelvis is less than 3 inches; and if it fails, recourse should be had to the forceps.

Version should not be made where there is any disproportion existing between the head of the child and the mother's pelvis. And he further informs us that the very few cases of children extracted alive by means of pelvic version, have *all* occurred in cases where the pelvis has not been deformed in all of its diameters, and where the children have had small and compressible heads. In Case No. VIII. of Dr. Goodell's paper the child weighed five pounds and six ounces, and was at 256 days of gestation, or but little over eight months.

Ramsbotham says that, as a substitute for the use of the forceps, he cannot sufficiently condemn version, and that he should not have mentioned it except that it is advocated by Simpson, who, however, merely recommends it in place of craniotomy.

Gardien gives cases of successful delivery unaided, in pelves of 3, —  $2\frac{3}{4}$  inches, and in rare instances  $2\frac{1}{2}$ .

Denman recommends version in deformed pelves with malpresentations.

Robert Collins recommends version when there is no disproportion between the child's head and the pelvis.

Edward Rigby makes no allusion to version for delivery in contracted pelves, but recommends the forceps.

Capuron says version is always dangerous to both mother and child, and especially alludes to the increased dangers to both in contracted pelves; to the child by dragging and traction on its spinal column, with the danger of luxation and detruncation; to the mother, from laceration and contusion of her tissues and the great difficulty of performing the operation; and concludes that it would be useless to "turn" the child unless the disproportion should be very slight.

Churchill says, version, in slightly narrowed pelves, is more than questionable.

Blundell recommends forceps when the head is detained at the superior strait.

Robert Lee says, in cases of slight deformity of the pelvis, the case should be let alone so long as the head advances and no unfavorable symptoms are present, and when it becomes necessary, aid with the forceps.

Here, gentlemen, upon the evidence produced, I rest the assertion that women can be delivered with safety to both mother and child at full time unaided, with pelves of 3 inches and upwards; and if the mother fails in her efforts, with a pelvis of +3, the case is easily and safely within the control

of a master with forceps; and that Hodge is fully sustained in his assertion that delivery with the forceps is the settled practice. Now, let us examine some of the arguments brought forward by the "versionists," by which they seek to turn aside and ignore all the natural powers and forces provided by the Creator for the relief of woman in her travail, and aim (in vain) to bring to naught the promise that "in sorrow thou shalt bring forth children."

1. It is claimed that delivery may be more rapidly accomplished.

With so great a man as Sir James Y. Simpson as authority, this is the first time in the history of obstetrics that rapid delivery, forcibly dragging the child through a narrow pelvis, through an over-stretched neck and os, through the vagina and over the perineum, is claimed to be a means of safety to the mother. In Case V. of Dr. Goodell's paper, "the os did not dilate," "I turned the child and delivered it alive in less than three minutes," etc.

With such rapid and forcible delivery as here described, the mother's tissues must necessarily be liable to laceration and contusion. The neck of the uterus may be injured, for time is not always allowed for dilatation. In Case III. of Dr. Goodell's paper, "two fingers of the left hand were then squeezed into the os, and ultimately a third."

The vagina cannot be thus rapidly forced open without the danger of laceration. The perineum would in all probability be torn. But this is said to be of little importance. True, it may frequently be relieved by a few stitches, but sometimes the rent extends into the rectum, and, after repeated and skillful operations, may fail to unite,—the poor woman remaining for the rest of her life alike disgusting to herself and her friends. This, in my opinion, should not be regarded as a trivial accident, but should always be guarded against with the most scrupulous care. I always feel disgraced when it occurs in my practice. Or the parts may become contused, and inflammation, sloughing, etc., follow, resulting in vesico- or recto-vaginal fistula, or septicæmia, or even the death of the patient; besides, rapid deliveries are sometimes productive of profound fainting fits, followed by hemorrhage, difficult to control and even fatal in result. The most alarming cases of post-partum hemorrhage that have occurred in my practice have followed rapid delivery. This has, in my experience, been so frequent a sequence to such labors that, for many years, I have adopted the plan of remaining with the patient a much longer time than I find it necessary to do in ordinary cases.

2. Another argument is, that when the head enters the pelvis

by its base, the parietal bones are more easily compressed and override each other better than they do when the head presents by the vertex.

Simpson avers that when the head presents in narrow pelves, it presents by its vault, and as the labor progresses this presenting part becomes widened out to a greater extent, thus making it impossible for the head to engage. I have previously referred to Meigs and Hodge in proof that the head in vertex-presentations, with pelves of 3 or +3 inches, does dip, and the vertex does enter the brim. Leishman, reviewing Simpson's paper, declares that, in attempting to prove that the head in narrow pelves presents by its vault, etc., he has attempted too much;<sup>1</sup> and Dr. Hodge, after a careful and elaborate review of the same paper, dissents, and does not admit that the head is more compressible when it presents by the base than it is when it presents by the vertex; and says the head is not flattened out (in narrow pelves) as represented by Simpson, but demonstrates that one pole or extremity of the bi-parietal diameter rests on the pubes, and the other glides over against the promontory, making the head oblique at the brim. The head thus compressed between the sacrum and the abdominal muscles, and subjected to the pressure of the uterine contractions, is diminished by the overlapping of the parietal bones at the sagittal sutures, and is therefore not flattened at its vault, but, on the other hand, increased as to its convexity. These observations of Meigs, Hodge, and Leishman are precisely what I have observed in my own practice. I therefore cannot accept the first of the series of arguments brought forward by Dr. Goodell in favor of version.

3. Simpson prefers version to craniotomy or the long forceps, because we may thus sometimes save the life of the child. In his paper upon Version he repeatedly makes this pointed preference, "but it is principally as a substitute to craniotomy," etc.<sup>2</sup>

There is a great want of distinctive accuracy in the table of cases cited by Dr. Simpson, for we find no evidence that the pelves were measured except in two of the forty cases given by this great author. No. XXV., in which the antero-posterior diameter is given at  $3\frac{1}{4}$  inches; and in this case the head resisted "even very strong traction on the inferior jaw and shoulders," but delivery was subsequently effected without assistance, the head being "expelled by the action of the abdominal muscles;" the child gasped but did not live. And No. XXXV.

<sup>1</sup> System of Midwifery, p. 493.

<sup>2</sup> Obstetric Works. First Series, Vol. I., p. 475.

In this case the antero-posterior diameter was given at "about 3 inches"; the child was still-born.

Dr. Goodell refers especially to No. I. of Simpson's cases. We find by reference to it the pelvis is designated as distorted, and no measurement given; the child was still-born. In the balance of his cases cited, the pelvis are spoken of as "distorted or contracted."

But as I do not admit the justice of the comparison between craniotomy and the use of the forceps, for either of which version is thus offered as a substitute, nor accept the conditions laid down by Simpson for the use of the forceps, nor his mode of applying them, I will not further allude to his statistics.

4. The trunk of the child serves as a better and safer means of traction than the crotchet (Simpson).

This may be admitted. As the crotchet is generally fatal to the child,—and I regard it as a very dangerous instrument for the mother,—nevertheless, if the child's body is to be dragged upon with a force of 130 pounds, and pulled out with the arms either over its head or brought down,—that is, "neck or nothing,"—we are compelled to regard this mode of delivery as attended with imminent danger to the child's life.

5. The child's neck can sustain with impunity a very great strain,—120 lbs. and more; "and this great weight (130 lbs.) I certainly have, *on several occasions*, thrown on infants' necks, etc. I have broken the neck but once." Goodell, *Philadelphia Medical Times*, p. 387.

Meigs informs us that an unborn infant will not safely bear a greater traction than it will after its birth; and, further, "he that makes rude traction upon the child's neck does a pernicious thing."

Cazeaux says that traction upon the child's neck, when there is a great obstacle to its birth, is fatal.

Dubois and Desormeaux say that traction upon the child's neck is more hurtful than beneficial.

Hodge writes: "It would be very easy, from the records of the profession, to detail the horrible results of forcible traction upon the limbs and body of the child, when such traction was made not at the proper time or in the proper direction,—when brute force has been substituted for art and science. Perhaps there are but few practitioners who have not seen or heard of cases where the strength of two individuals has been applied to the body of the child, while that of three or four, or even more, attendants to the body of the mother, to accomplish the delivery of the head after the body has been born. That the child should ever escape with its life under such circumstances is surprising, but it is not to be wondered at that the spinal

marrow has been fatally injured ; that the spine itself has been dislocated, or even fractured ; that the limbs have been torn off, or that, in some unfortunate cases, the body of the child has indeed been delivered, but the head remained impacted in the strait of the pelvis. Surely, in this present state of art and science, such practice is exceedingly reprehensible, not to say positively criminal."

Radford says that a force sufficient to greatly compress the sides of the cranium is a procedure at variance with all scientific views, and incompatible with the safety of both mother and child.

Nor can I be persuaded to believe that an unborn child can with safety bear a tractive force upon its neck, "with a pump-handle motion," of 120 lbs. or more, because a dead infant's neck is found to sustain a weight of 105 lbs. before the spinal column yields, or 120 lbs. before the body parts from the head ; yet Dr. Goodell informs us that he has twice delivered living children, after using so much force as to cause their necks to "snap," but not to break, and that he has (*AM. JOURN. OF OBSTETRICS*, Aug., 1875, p. 208) on *several* occasions delivered living children after throwing on their necks a weight of 130 lbs.

6. The child may continue to live where great delays have existed with the body delivered and the head detained at the brim (Simpson).

This is the consolation offered to us for the frightful dilemma in which we may find ourselves, after making version and finding it is impossible to effect the delivery of the child. But earnest, conscientious writers, Denman, Hodge, Meigs, and others, to whose testimony I add my own, declare that they are not able to confirm this statement. Dr. Goodell (*Philadelphia Medical Times*, March 20, 1875) says, "five minutes may be too long and prove fatal to the child."

7. That the umbilical cord may seek shelter or be protected behind the promontory of the sacrum, and thus avoid compression.

It is not possible to suppose this could be the case, or that the cord could be moved by any manipulation from the contracting grasp of the uterus through whose neck the fingers had just been squeezed to make version, and where it was impossible to bring down both hips, and where powerful traction was required to bring down one limb, and where the neck was firmly pressed by the head of the child, and added to this the pressure of the hand of the operator in bringing down the arms, together with the counter pressure caused by the bony canal of the pelvis. One moment's thoughtful reflection, and one case to test this suggestion, will suffice to answer it.



I ask your indulgence a little longer while I refer to the statistics brought forward by Dr. Goodell in the paper now under discussion, in which he attempts to establish version as a means of delivery for women in labor with contracted pelves, and which form the basis on which he seeks to justify a traction of 130 pounds upon the neck of an unborn infant.

CASE I.—Here the pelvis was  $3\frac{1}{2}$  inches in its conjugate diameter, evidently a marginal narrowing, because the head passed the brim with a jerk, which was followed by no further difficulty in the delivery.

The indication for version was clearly established by the prolapse of the cord. The capacity of the pelvis was such as to admit of delivery by the natural efforts of the woman, or with her effort aided by a master with forceps, and, therefore, so far as the bony pelvis was concerned, version was not demanded.

CASE II.—In this case the pelvis was ample in its capacity; the head presented normally, and version could not have been justified except for a great emergency; but as there were three competent physicians engaged in the case, it would be very unfair to question their united judgment or their deliberate action.

CASE III.—This is an interesting case. The woman fell in labor with her first child at seven months of gestation; the labor continued forty-eight hours, six of them being occupied with expulsive efforts. When the child was born it weighed five pounds. Her doctor (this is an important consideration with a patient, and I think justly so, because, as in this case, she placed her life and the life of her unborn child in his keeping) told her she could not give birth to a child at full term. In her second pregnancy he urged her to have the induction of premature labor. Naturally dreading so formidable an operation, she declined to submit to it, and she was placed under the care of Dr. Goodell. The capacity of her pelvis was estimated at +3, say  $3\frac{1}{8}$  inches. After a few hours of labor she complained of severe twitching and impaired vision, with headache. She was immediately etherized, the os forced open, version made, and the child quickly delivered. It weighed seven pounds twelve ounces. For this hurried delivery Dr. Goodell gave as a reason his "fears of an attack of eclampsia." He does not, however, inform us of the presence, previously to the onset of labor, of any of those symptoms which usually precede and accompany an attack of puerperal convulsions. It would be but reasonable to expect a woman to have some nervous symptoms during the first stage of her labor, when she had been informed by her doctor that she

could not live through her labor, now that she was at "full term."

We are not told that this patient had, previous to falling into labor, headache, with local pains in the head, vertigo, impaired vision, nausea, loss of or irregular appetite, constipation, frequent and scanty urination, swollen feet or hands, tingling of the fingers, or albumen in the urine; yet, as a very general rule, patients exempt from these symptoms up to the onset of labor do not have convulsions; but, supposing the case as estimated to be one of impending convulsions, was version demanded? Evidently there was no great uterine irritability, because, "with two fingers of the left hand squeezed into the os, and ultimately a third," and the hand externally, the child was easily turned more than half a circle and had to be reversed.

Many excellent physicians do not recognize the necessity of hurried deliveries in puerperal convulsions, and in confirmation of this view of the subject, it is well known that convulsions do not always cease with the delivery, and that, in many instances, they do not occur until after the delivery, and moreover they frequently subside for hours or even days before the labor terminates—in my experience as many as five days.

But the patient was etherized. I know from abundant experience the controlling effect of ether in the treatment of puerperal convulsions. I gave it in puerperal convulsions in July, 1854, probably the first time it was administered for this purpose; and I then gave it under the earnest protest of two of my most esteemed professional friends, Dr. C. D. Meigs and Dr. William Pepper. The result was so satisfactory that I have given it in every case admitting of treatment that has since come under my care, with unabated confidence; but it does not always control the spasms. It is a well-established fact that in puerperal eclampsia, during a convulsion, the spasm or contraction of the uterus, at times, is so great that the child is driven with such force as to dilate a rigid os, and through the vagina—and through the perineum with one pain—so that "touching" before a convulsion and finding the os rigid and not dilated, and after the convulsion repeating the "touch," the child may be found extruded from its mother's body. The capacity of the pelvis in this case was such as would place the delivery within the natural efforts of the woman, or within such natural efforts conjoined with the assistance of a master with forceps, and therefore unless the emergency represented to complicate the labor was sufficient to justify version (which I cannot admit upon the evidence submitted), version was altogether uncalled for.

CASE IV.—This case is the first in which version was decided upon from choice; the pelvis was “somewhere about  $3\frac{1}{2}$  inches.” I like the expression, because I know of no means by which the measurement of the pelvis can be ascertained with mathematical accuracy. Hodge, Meigs, and Leishman, referring to this subject, say that the measurement of the pelvis can only be taken with an approach to accuracy. The child was undersize, weighing 5 pounds 14 ounces, with a pelvis but slightly deformed; and, certainly, twenty-nine hours is no unusual delay for a first labor. The case, therefore, comes under the classification of cases given by Chailly-Honoré. (Above, p. 679.)

CASE V.—This woman had been delivered in two labors of living children. And this must be accepted as evidence that she might have been safely delivered subsequently. Her pelvis was estimated to be exactly  $3\frac{3}{10}$  inches, and, therefore, the delivery was within the control of the natural efforts of the woman, or such efforts conjoined with the assistance of a master with forceps. I find no justification for lashing the handles of the forceps, or for dragging them against the neck of the bladder; and it is scarcely a good argument to offer such manipulations as a justification for version.

CASE VI.—This woman had been delivered in four labors of living children by the aid of the forceps. In her fifth confinement Dr. Goodell saw her. He informs us that the forceps slipped. It was evident, therefore, they were not applied as taught by Hodge; and as the pelvis “was somewhat narrowed only,” and the woman had repeatedly given birth to living children, we cannot regard the size of the pelvis in this instance as demanding version, but are compelled to attribute the performance of the operation to some other cause.

CASE VII.—This unfortunate woman had been delivered in her first labor by “craniotomy;” her second was premature; her third was by “craniotomy;” her fourth by version, of a dismembered child. Her pelvis was ascertained to be exactly  $2\frac{8}{10}$  inches. The child was found to be under the usual size. The pelvis was, however, upon more careful investigation, thus found to be smaller than first noted, but version was proceeded with by Dr. Goodell. He says: “I turned through the bag of waters, then broke it and seized both feet, but found there was not room enough to bring them down. I therefore let go the right foot and coaxed down the left one, by hooking two fingers over the instep. Very powerful traction was needed to pull the half breech and chest through the brim. Finding the child alive, I at once brought down the arms, and proceeded to extract the head. By a propelling power of about 50 pounds on the vault of the head, and a traction weight of

not less than 120 pounds on the child's neck, aided finally by a pump-handle movement of traction on its legs, a dead child was born *in exactly twenty minutes* after the rupture of the membranes. Its neck was unbroken."

Let me ask you to notice especially the following points in this case: 1st. The great uncertainty of measuring the pelvis "with exactness," particularly when the patient is in labor. 2d. The difficulty and unavoidable delay in bringing down the arms, with or without regard to their integrity, and in dragging the head over the narrowed brim when that is 3 inches or less, "twenty minutes having been occupied in the operation," and we are recommended by Dr. Goodell (page 386, *Medical Times*, March 20, 1875) to pull at the risk of breaking the child's neck rather than allow the head to be detained at the brim five minutes. 3d. It is here plainly demonstrated that the child's life cannot be sustained for "twenty minutes" with its head at the brim of the pelvis and its body extracted, and with such powerful traction as was here made. 4th. That it is equally impossible to suppose the umbilical cord could slip, or could by any manipulation be moved into the space upon either side of the promontory when hugged by the neck of the uterus forcibly stretched open, as must have been the case in this instance. 5th. The result in this case corroborates the statements of Leishman, Churchill, Cazeaux, Chailly-Honoré, and others, that version in pelves contracted as this one is represented to have been, almost always proves "fatal to the child," and therefore should never be resorted to on this account, as well as because of the great and uncalled-for dangers to the mother.

CASE VIII.—This woman had, in her first confinement, a tedious labor, was delivered by "craniotomy," and recovered with a vesico-vaginal fistula. In her second pregnancy, labor was established 258 days following the cessation of her last menses, which ended on the 28th of November, 1873, and she was delivered on the 13th day of August, 1874. Deduct two days (I usually allow five in estimating the term of pregnancy), this would give her a duration of gestation of 256 days, twenty-four days less than normal pregnancy; her pelvis was +3 inches. Dr. Goodell (*AM. JOURN. OF OBSTETRICS*, Aug., 1875, p. 205) says: "I decided to turn as soon as the arms were brought down. Dr. Roberts made supra-pubic pressure with *all his might*, while I threw on the child's neck *all of my weight possible in a bent posture*. A distinct snap of some cervical structure was felt and heard; in less time than it has taken me to describe the process, the head *bounced* out of the grip of the brim, and the child was born. It weighed 5 lbs. 6 oz."

And thus was *pushed* and *pulled* into the world a baby weighing 5 lbs. 6 oz. at a little over *eight months* of gestation.

Permit me here to give a case from my own practice of somewhat similar character. Mrs. L. was confined with her first child under the care of a distinguished professor of obstetrics in a Western city, assisted by an eminent physician of the place. Her labor continued for three days and nights, and was finally terminated by the aid of the forceps. The child was still-born, its head was badly mutilated, the perineum was torn, the rent extending one and a half inches into the rectum. After a painful and tedious recovery, in due time she became pregnant again, and was told that she could not give birth to a child at "full term," that her life would be sacrificed in the attempt, and that therefore she must have her labor induced prematurely. Accordingly she came to Philadelphia for that purpose, and was placed under my care. After a very careful examination, I could detect no defect in her pelvis, and it was only by great persuasion I could prevail upon her to go to full term. The first pain she recognized as a labor pain occurred whilst she was at dinner, about three o'clock. She immediately sent for me and for the nurse. I arrived at her house a few minutes before five p.m.; the nurse was not yet there. Upon entering the house I heard the baby screaming lustily. It weighed 9 *pounds* 11 *ounces*. I had no opportunity of using the forceps. I mention this case in detail, because I consider it of importance as teaching us the necessity of carefully investigating every case of reputed defect of the pelvis that may come under our care, and the necessity of being on our guard about managing such cases in accordance with statements thus made as to previous labors.

CASE IX.—This woman had, with her first child, a tedious and difficult labor, and she was delivered by "craniotomy." In her second labor she was delivered of a living child by version, through a pelvis said to be  $2\frac{8}{16}$  inches. How far the result in these two labors may be attributed to the established difference of mortality in first and subsequent labors, I cannot say; it is, however, true that the death-rate to the child is far higher in first than it is in subsequent labors.

CASE X.—This case gives us the history of another difficult first labor. The child was still-born, and the delivery was effected by the forceps. Her pelvis was  $+3\frac{1}{2}$  inches. In her second labor version was made, and it was found impossible to effect delivery even after an exhaustive effort with a traction force of 130 *pounds*, and after the child's neck gave way, the head was perforated.

Thus, by examining the ten cases reported by Dr. Goodell, it



appears that his table includes but one primipara. The character of the individual cases may be summarized as follows :

CASE I.—Pelvis  $3\frac{6}{12}$  inches. Still-born. Prolapse of cord.

CASE II.—No defect in the pelvis. Convulsions.

CASE III.—Pelvis + 3 inches, say  $3\frac{4}{12}$ . Child living.

CASE IV.—Primipara. Pelvis  $3\frac{6}{12}$ . Child living. Weight, 5 pounds 10 ounces.

CASE V.—Pelvis  $3\frac{3}{10}$  inches. Child living. Weight, 7 pounds 10 ounces.

CASE VI.—“Pelvis somewhat narrowed.” Child living. So were all her children in previous labors.

CASE VII.—Pelvis  $2\frac{8}{10}$ . Child dead.

CASE VIII.—Pelvis + 3 inches, or  $3\frac{4}{12}$ . Child alive. Weight, 5 pounds 6 ounces.

CASE IX.—Pelvis  $2\frac{8}{10}$  inches. Child dead.<sup>1</sup>

CASE X.—Pelvis  $3\frac{6}{12}$  inches. Version followed by craniotomy.

Again, CASE VIII.—The child was  $36\frac{1}{2}$  weeks of gestation. The bi-parietal diameter at this period of gestation is estimated by Ritgen at  $2\frac{1}{2}$  inches; by Stoltz, at —3. It weighed 5 pounds 6 ounces. We, therefore, must regard the head as having been very compressible. Its diameter was certainly less than three inches (—3 inches), whilst the mother's pelvis was more than three inches (+ 3 inches). In this case, therefore, there were no disproportionate relations between the child's head and the bony walls of the pelvis.

CASE IV.—The child was much under-sized. It weighed 5 pounds 14 ounces (just 8 ounces more than Case VIII., in which, by the authority of Ritgen and Stoltz, the bi-parietal diameter was estimated at less than 3 inches), whilst the mother's pelvis was  $3\frac{6}{12}$  inches. There could, I venture to assert, have existed in this case no disproportion between the child's head and the mother's pelvis.

CASES I. and II.—There existed special causes demanding version.

CASES V. and VI.—Each of these women had repeatedly given birth to living children.

CASE VII.—The child was still-born.

CASE X.—The child was still-born. Version followed by craniotomy.

CASE IX.—We have the unfair comparison between first and subsequent labors (with the possibility of some slight mistake in the fractional accuracy of the measurement of the pelvis, such as occurred in Case VII.).

CASE III. is only another instance of comparison between first and subsequent labors.

<sup>1</sup> [Is not this a mistake? Should it not read “living”? See preceding page.—Ed.]

In this recapitulation of Dr. Goodell's cases, the evidence in favor of version, so far as the result to the child goes, is not very satisfactory. And, as all the women survived their previous treatment, it is quite possible that the results would have been equally if not more favorable if the forceps had been skilfully used instead of version.

In Dr. Simpson's forty cases, he gives the measurement of the pelvis in but two instances (Cases XXV. and XXXV.), and in each of these the child was still-born.

Schroeder informs us that "in very great pelvic contraction, with a conjugate diameter of  $2\frac{8}{12}$  inches, he has extracted live children," but does not give the diameters of their heads nor their weights.

Time fails me to discuss the modes resorted to for the delivery of women in the second, fifth, and seventh centuries, as the forceps were not then in use, or for any lengthy consideration of the fact that upon the introduction of the forceps in the seventeenth century there were some physicians indisposed to use them. Nor do I wish to enter into any description of the various kinds of instruments in use at the present time, or the manner of applying or using them, as my only object in taking part in this discussion has been to advocate the skilful and masterly employment of the forceps as the *best, most efficient, and safest known means* of delivering women whose pelvis are contracted when the conjugate diameter is not less than three inches.



STATED MEETING, JUNE 3, 1875. THE PRESIDENT, DR. A. H. SMITH, IN THE CHAIR.

DR. WM. GOODELL read the following reply to the paper on

VERSION IN CONTRACTED PELVES,

read by Dr. Ellwood Wilson at the last meeting :

MR. PRESIDENT AND GENTLEMEN :—It is the first time in the history of this Society, that a contribution, such as mine, was ordered to be printed in pamphlet form, in order to facilitate discussion. It is also the first time that a written discussion, such as Dr. Wilson's, was ever offered before us by one of its members. This shows the interest taken in the subject of my essay ; but it, at the same time, put me to disadvantage. For while my critic has had over six weeks in which to prepare his paper, I have had but two, and those very busy ones, in which

to frame a reply. And this is the more difficult because his paper is unfurnished with references to his authorities.

My reviewer begins by showing the foetal mortality of head-last labors, and of those attended with contraction of the brim. But he overlooks the fact that I have offered a new mode of delivery, by which I claim that this can be reduced; and that I give reasons why an average head is more readily dragged through a narrow pelvis, than a large head through an average pelvis. He, on the other hand, gives the mortality of normal cephalic labors by way of contrast, but carefully avoids giving that of children when delivered by the forceps through narrow pelves. In the verbal discussion that ensued on the first reading of my paper, I described the mechanism of head-first and head-last labors in narrow pelves, showing the mechanical advantages in favor of the latter.

The arguments then adduced he has not attempted to meet. He simply replies in effect, that version is not proper, because Hodge, and Meigs, and Baudelocque, and Boivin, and Velpeau, and others, think differently. Is the art and science of obstetrics a *punctum stans*, and not a *punctum fluens*? And must it rest stationary where obsolete or obsolescent authors have left it? Every student knows the opinions of the authors whom he cites, and this Society will not, therefore, be intimidated by a formidable bead-roll of names. Witnesses are not to be numbered, but weighed. The watchword of science is *Nullius addictus jurare in verba magistri*. If, however, he looks to authorities I can give him some modern ones, who far outweigh all those he offers. Simpson, *facile princeps* of obstetricians, advocated turning in narrow pelves, and so did W. Tyler Smith. So do Leishman, Meadows, and Milne, who have published works on obstetrics; the great Cazeaux became a convert to the new doctrine. Barnes, Braxton Hicks, and Schroeder, the present leaders of obstetric thought, endorse Simpson's views. While Braun, who has had the unrivalled experience of over one hundred thousand labors, is unqualifiedly pledged to this mode of delivery in narrow pelves.

In proof of its usefulness in lessening the number of cases of craniotomy, Pugh (Barnes, *Obstet. Operations*, Lond., 1871, p. 226) "never opened one head for upwards of fourteen years." Smellie, one of the authors whom Dr. Wilson quotes, says (*Ibid.*), "Midwifery is now so much improved that the necessity of destroying the child does not occur so often as formerly; indeed, it should never be done, except when it is impossible to turn or to deliver with the forceps,"—placing turning before the forceps. Milne, "out of many cases" of turning, had to perforate but once (*Midwifery*, 1871, p. 239). Dr. Geo. Jones (*British*

*Medical Journal*, 1871, vol. ii., p. 32) resorted to version in all cases of disproportion, and had no case of craniotomy in two thousand labors. Denman, who came very near the truth, without fully grasping it, remarks (*Midwifery*, chap. xiv., sec. ix.), in surprise at the rarity of craniotomy in head-last labors complicated with disproportion, "But it will be scarcely believed how seldom this operation is necessary under these circumstances, if we have not been in a hurry, but have acted with prudence."

I cheerfully admit with Dr. W. that "it is *possible* for women to be delivered unaided at full term with pelves of 3 or — 3 inches," but it is highly *improbable*. Hodge says (*Obstetrics*, p. 400), "Such cases are of very rare occurrence. Few accoucheurs have been permitted to see them." There are a few stock cases of spontaneous delivery in narrow pelves, but they are so exceptional as to be worn thread-bare by obstetric authors, and as Meigs observes of them (*Midwifery*, 1852, p. 568), "The exceptions but prove the rule" of their impracticability. As an argument against version these examples are worthless, for no practitioner is justified, on their account, to see whether nature is equal to the occasion. It is the opinion of Hodge (p. 400), that, "If the pelvis has not at least three and a half inches in its sacro-pubic diameter, a child fully developed will seldom be delivered alive by the spontaneous efforts of the mother."

But, argues my critic, if nature can deliver in very narrow pelves, *a fortiori* can nature plus the forceps. This is a fallacy, for, on the contrary, the fact of remarkable natural deliveries through very narrow pelves only strengthens my position, for it shows that the head, under these exceptional circumstances, untrammelled by the steel blades of the forceps, will so mould itself to the configuration of the brim, as to pass through pelves, through which the forceps could never drag it. By version there is added to this free moulding a tractive power fully equal to that of the forceps. But the forceps interferes with the process of moulding. So impressed are many physicians with this defect of this instrument, that they advise its use merely as a tractor, and not as a compressor, hoping thereby to "wire-draw" or "mould" the head. But they overlook the fact, that, in "wire-drawing," the pincers which seize the wire do not pass through the graduated openings through which the wire is dragged. After version the tractive power is applied, as in wire-drawing, outside of the opening through which the head is to pass and to which it is to be moulded.

For these reasons Cazeaux argues thus (*Midwifery*, ed. 1866, p. 819): "Certain practitioners, having observed that the head

became gradually moulded to the shape and dimensions of the pelvic cavity, by the efforts of the womb alone, in some cases in which the pelvis was contracted to less than three inches, have therefore imagined that the resources of art could accomplish what nature alone sometimes effects; that by the forceps a similar reduction in the diameters of the head might be obtained; and, consequently, that the instrument could be usefully applied when the contracted diameters are even less than three inches. But they have instituted a comparison between two forces that are wholly dissimilar. Indeed, there can be no doubt that the expulsive efforts of the womb have succeeded in forcing the head through the pelvis where the smallest diameter did not exceed two and three-quarter inches; but this result was only effected after a tedious labor of thirty, or forty, or even sixty hours; and where the slow and gradual compression, to which the head was then subjected, has enabled the brain to accommodate itself thereto by degrees. . . . But where the pelvic diameters exceed three inches, the forceps may prove very useful; though I am induced to believe that the character of its action has been misunderstood, by supposing that it is to serve both as an instrument of traction, and as one calculated to reduce the dimensions of the head. Let it be understood that the forceps merely act here as an instrument of traction." Chailly-Honoré (*Traité Pratique*, Paris, 1867, p. 643), another of Dr. Wilson's authorities, states, in almost identical language, that because nature can sometimes, by a slow process of moulding, successfully deliver in pelves under nine centimetres (3.54 inches), it by no means follows that the forceps can. To this opinion Gardien also inclines (*Traité d'Accouchements*, vol. ii., p. 505).

Having disposed of this argument, let me turn to another. "As for the child," contends my critic, "in pelves of +3 and 3.5, the result should generally be favorable; the forceps should never injure the child or mother."

He further quotes Hodge as saying that "delivery can be accomplished with safety to both mother and child, in the great majority of cases, where the pelvis measures 3 inches and +3."

And also contends that "If the mother fails in her efforts, with a pelvis of +3, the case is easily and safely within the control of a master with forceps." Again, that by the forceps he has seen the biparietal diameter "reduced ten lines without injury to the child." By what facts are these assertions substantiated? Simply by very rare exceptions to the general rule in so far as a three-inch pelvis is concerned. Among them he cites *one* case of Hodge's, and *one* of his own (Mrs. McN.), in



which he does not give the weight of the child, nor the dimensions of its head. Let me read what Hodge says about his case (*Obstetrics*, p. 252), "In one case of contracted pelvis, where the sacro-pubic diameter measured *but little, if any more* than three inches, the author delivered an infant alive, whose head, a few hours after delivery, measured three inches and ten lines in its transverse diameter." It was, as the language plainly shows, his *only* case. At page 402, Hodge describes, what he calls, "a very remarkable case," one in which, after "less than two hours" traction, he delivered a living child through a conjugate "not more than three and a quarter inches." Would Hodge speak of this as "a remarkable case" were a successful delivery in so narrow a pelvis as 3.25 inches, the ordinary and easy result with the forceps that Dr. W. would lead us to infer? Once more, Hodge did not apply his forceps to the sides of the child's head when at the brim of a contracted pelvis.

We all know that Dr. W. is a great advocate for the application of the forceps to the sides of the child's head, and in this he has our hearty concurrence. But when he asserts that Hodge advises, "that with a pelvis of three inches in the antero-posterior diameter, the forceps should be applied to the sides of the child's head," that Meigs "taught and practised the feasibility of applying the forceps to the sides of the child's head at the brim of the pelvis," and that "Denman, Dubois, Chailly Honoré, Baudelocque, Velpeau, and many others, have taught and asserted the practicability and success of the mode of using the forceps," I must call his accuracy in question. In cases of narrow pelves, Hodge says (p. 401), "that the blades of the forceps cannot be applied over the sides of the head in all those cases where it is situated transversely, or somewhat obliquely at the superior strait," but can only be in those "exceedingly rare" "third or sixth positions," viz., occiput toward the pubes or the sacrum. He therefore sanctions the application of the blades to the occiput and forehead; but advises as a much better one the *oblique* (fronto-mastoid) application. So far from Dr. Meigs teaching this bilateral application at the brim, he prints for greater emphasis, in leaded type (*Obstetrics*, chap. xv.): "A man shall hardly be justified who inserts his forceps within the os uteri. He must wait until the circle has risen above the parietal protuberance, and can no more be felt." With such a rule of guidance, Meigs could not have made many applications at the brim. In transverse cranial positions below the brim, he sanctions the application to the sides of the head. But in similar positions of the brim in narrow pelves, he thus condemns it: "It would be *preposterous* for him to think of adjusting the blades upon the sides of the head

in that direction. The pelvis (one of three inches) is, perhaps, fatally small. To apply the blades of the forceps, then, would be considerably to increase the necessity for the reduction of the transverse diameter of the head. *Common sense*, therefore, would teach him that if he must apply the additional force, it must be applied to the face and occiput of the child."

Again, he adds (chap. xiv.), referring to English practitioners, "I cannot but indulge a disposition to dissent from this almost invariable habit of adjusting the blades to the sides of the head, much preferring the practice of the Continental physicians, and of those of the United States, who seize the head upon the sides of the pelvis—a practice as to the safety of which I confidently speak, from multiplied opportunities in my clinical experience." Great are the changes which have taken place since Meigs wrote his book. Hamlet and Laertes have exchanged weapons, and we of this city are flourishing our cephalic application of the forceps, in the face of the English pelvic application.

So far from Chailly-Honoré applying the forceps to the sides of the child's head at the brim, he declares (*Paris*, 1867, p. 618) that in transverse cranial positions, in the majority of cases, such an application cannot be made even below the brim. Whilst, when the head is engaged in the superior strait, the bilateral application can be made only in those "very rare" positions in which the occiput or the forehead lies directly behind the pubes (p. 627). He, therefore, teaches the "German method," viz., to the sides of the pelvis, without reference to the position of the head. Hence, he observes, in oblique positions of the head at the brim, the head will be caught in the fronto-mastoid diameter; and in the transverse, over the forehead and occiput. Denman, it is true (*Aphorisms*, 1815, pp. 11, 12), taught that "the forceps should always be applied over the ears of the child; it must therefore be improper to apply them when we cannot feel an ear." He then goes on to explain that the ear must be felt "by a common examination" with the index finger. He, further, applied them when "the head should have rested for six hours, as low as the perineum, that is, in a situation which would allow of their application" (*Midwifery*, chap. xi., sect. vi.). This advice, of course, excludes Denman's application of the forceps to the sides of the child's head at the brim. Velpeau (*Traité Complet des Accoucheurs*, Paris, 1835, vol. ii., p. 873) declares that in transverse cranial positions at the brim, "which are often met with, I do not see how we can avoid following the rules of the German accoucheurs," viz., the application over the forehead and occiput. Dubois and Baudelocque did apply the forceps to the

sides of the head at the brim. So did Joseph Warrington, another author referred to. But he deemed this operation so hazardous a one as not to be undertaken except by only the skilled physician. In his "Obstetric Catechism" are to be found the following questions and answers: "Is it warrantable, in any case, to introduce the forceps before the head has cleared the os? Prof. Meigs, who is high authority, says it is not. Suppose the head becomes arrested at the superior strait . . . would you turn or apply the forceps? Turning would be safer, unless the practitioner have much experience in the use of the forceps." With two such rules of guidance, few physicians would venture even to apply the forceps at the brim, much less to the sides of the child's head. Thus, out of eight authors, cited by my reviewer as applying the forceps to the sides of the child's head in transverse cranial positions at the brim, only three of them taught this mode of application, the rest did not. Of these three, one only, DuBois, gives any statistics. To these I shall refer before long. Now, it is bad enough for Dr. Wilson to rest the progress of obstetrics upon authors who are nearly all of them obsolete or obsolescent, without misstating what their practice was.

At this stage of our discussion, it may be interesting to note the behavior of two "masters with the forceps," when they were confronted, the one with a pelvis of three inches, the other with a pelvis supposed for many hours to be of the same size. Dr. Joseph Warrington informs us (*Obstetric Catechism*, p. 297, also *Hodge's Obstetrics*, p. 272) that he had a case of labor in "a contracted pelvis measuring three inches in the antero-posterior diameter." My hearers, who have heard from Dr. Wilson, that "delivery can be accomplished with safety to both mother and child, in the great majority of cases, where the pelvis measures 3 inches and +3," will of course conclude that Dr. Warrington, unaided, delivered his patient of a living child. Not so. But he at least delivered her of a dead child. Not even that. After repeated ineffectual trials with the forceps, he very properly sent a messenger after Dr. Hodge, who reports the case as follows: "These instruments were re-applied, and again appeared to be insufficient. Dr. Warrington opened the head and applied the forceps. The author then grasped the instruments firmly, and secured them by a fillet. The instrument was not sufficiently powerful immediately to effect our purpose. Fortunately, however, by continued pressure the left parietal bone collapsed, when delivery was safely and easily accomplished."

Let us now see how several other masters with the forceps behaved, when a pelvis was supposed for thirty hours to be

"not at the most three inches in the antero-posterior diameter" (*Meigs*, p. 570). Dr. Fox, the attending physician, called in Dr. James, Professor of Midwifery. Then, "on account of the unusual interest of the case, Drs. Meigs and Lukens were invited to attend." But still the case looked so unpromising that "Dr. Physick was called on, but was confined to the house by sickness. Dr. Dewees was also called for, but was absent from the city." Dr. Hewson was therefore "added to the consultation," making five prominent physicians who met over a pelvis with the supposed diameter of three inches. It does not appear that any one of them tried to apply the forceps. At noon of the next day, they discovered the pelvis to be smaller than they had at first imagined, and then proceeded to a very difficult operation of embryocælia.

We are now told that "Siebold, Osiander, Velpeau, Baudelocque, Leishman, Meigs, Denman, and Hodge" contend, that "by the natural efforts of the mother and by the careful interrupted compression of the forceps," a reduction of the bi-parietal diameter may be gained, variously rated by these authors at from four lines to nearly an inch. Now I cannot find that either Meigs or Leishman considered the forceps as a compressing instrument. The former (p. 542) quotes Baudelocque's well known experiment, in which, after lashing the handles and spoiling three fine instruments, he barely got over four lines of reduction, and argues from them that "they ought to suffice for removing any lingering disposition we might have to regard the forceps as a compressing instrument." Leishman (*Midwifery*, Glasgow, 1873, p. 528) observes that the pressure necessary to keep the instrument from slipping, "will effect all the compression which is desirable."

With regard to Denman's "nearly one inch," of reduction *by the forceps*, I should like to have the reference to this quotation. In hurriedly glancing through Denman's book, for I had no time to do more, I did not find this "inch," but I did the following inch. He delivered (*Midwifery*, chap. xi., sect. i.) a woman with a very narrow pelvis twice with instruments, evidently meaning from the context, craniotomy. In five more labors she was delivered by other physicians of dead children. In her eighth labor Denman, being again called in, delivered her by version of a living child, although, as he relates it, "on the left parietal bone there was a depression of considerable extent, and to my apprehension of full one inch in depth, occasioned by the projection of the sacrum." Perhaps this is the "inch" referred to, and my critic has got it misplaced.

But, granting that Siebold, Velpeau, and others could reduce the bi-parietal diameter when the blades were accurately ad-

justed to the sides of the head; what advantage did this give to those of them who in a narrow pelvis applied the blades to the occipito-frontal diameter? Far from being a help, this very compressing power of the forceps, thus applied, must necessarily increase the bulk of the offending bi-parietal diameter. Recognizing this mechanical disadvantage, Meigs, Cazeaux, Chailly-Honoré, Leishman and others use this instrument simply as a tractor,—making the pelvis the compressor, and not the forceps. With regard to the reduction of the bi-parietal diameter by the forceps when applied to the sides of the head, I demonstrated, in a previous discussion, that it could not compete with that made by version. For when the forceps is thus applied, in a transverse cranial position at the brim of a narrow pelvis, the virtual reduction of the bi-parietal diameter is vitiated by that made by the pubic blade, which is wholly unnecessary, and in fact thrown away as so much lost power, because it is the sacral side of the head which alone needs moulding.

We are now informed that Hodge, Dewees, Meigs, and Churchill cite cases of “successful delivery” by the forceps in “pelves of 2.75 inches.” They cite, it is true, a few very rare and, therefore, the stock cases of obstetricians, but offer none of their own that I can find. What they themselves thought about the feasibility of applying the forceps in narrow pelves, I will show by a few extracts. Churchill (Philada., 1855, p. 351) asserts, that “in distortion of the pelvis, when the antero-posterior diameter of the brim is less than three inches, we have no chance of delivery by the natural efforts or by the forceps; so that to save the mother we must destroy the child.” Again (p. 270), “it is clear, then, that unless there be a space of full three inches, it would be useless, probably injurious, to use the forceps.” Dewees (*Midwifery*, par. 790) says, “It would seem, then, from all experience, to follow, that from a pelvis with less than three inches, *or even three inches and a half* in its small diameter, a child’s head at full time cannot be made to pass with safety by means of the forceps, unless there be an unusual degree of suppleness in the bones of the cranium, or the head itself unusually small.” Again, “but when (par. 1547 with foot-note) the small diameter of the superior strait has less than 3.25 inches, these instruments cannot be employed at the full period of utero-gestation with any chance of success.” He also observes (par. 833) that, “when necessity obliges us to deliver when the head is situated at the superior strait, it is much better to have recourse to the doubtful but safer alternative of turning, for it will rarely happen that this cannot be performed while the head remains free above the



superior strait, even when the pelvis may be a little contracted, with at least as much safety to the child, and certainly more to the mother." Meigs (Philada., 1852, p. 567) avers that "indeed, if the pelvis have three inches of antero-posterior diameter, it (the child) cannot be born alive, unless it be uncommonly small, and moreover possessed of a very incomplete ossification of the cranial bones, and great laxity of the suture lines that unite them."

Dr. Wilson next informs us that "Baudelocque recommends the forceps in pelves of 2.5 inches and upwards, both for the safety of the mother and child." Also, that "Chailly-Honoré advises their use in pelves of 2.5 inches." Again must I challenge the accuracy of my reviewer. Baudelocque (*Traité des Accouchements*, par. 1972) writes thus: "This instrument is sometimes dangerous to the child when the pelvis has only three inches in the small diameter, and much more dangerous, if not murderous (*meurtrier*), when that diameter is still farther contracted; while it destroys the child it also exposes the mother to lesions more or less grave. The forceps are in no way proper, when the small diameter of the pelvis is not 2.5 inches." Again he writes (par. 1789), while on the subject of the use of the forceps in narrow pelves, "I shall suppose in all these cases that the length of the smallest diameter of the superior strait is less than 3.50, and more than 2.75 inches." Dewees, commenting upon this great Frenchman's work (*Midwifery*, par. 1547, and *foot-note*), says that "it must be borne in mind, that the French inch is one line, or one-twelfth of an inch larger than the English inch"; consequently 2.5, 2.75 and 3.5 French inches are nearly equal to 2.75, 3 and 3.8 English inches. In paragraph 1629, Baudelocque asserts that the use of the forceps is dangerous to the child "when the conjugate is three (3.25 English) inches in length"; and (par. 1633) states that he applied them but twenty times at the superior strait, and implies that his luck in saving the children was not great.

Chailly-Honoré, so far from advising the use of the forceps "in pelves of 2.5 inches," informs us that (ed. 1867, p. 642) he once extracted a *small* girl through a conjugate under eight centimetres (3.14 inches), "but such observations, which are extremely rare, only prove the infinite resources of nature, and ought in no wise to give support to those who advise us to attempt to extract with the forceps the head of a mature child in a pelvis under eight centimetres." He further considers the application of the forceps in pelves under nine centimetres (3.54 inches) as death to the child, very dangerous to the mother's life, and, in the majority of cases, useless. Again, in an argument against version, he says (p. 649): "This instrument

will frequently extract a living child (through a conjugate of) over nine centimetres (3.54 inches), sometimes, indeed—although this is very rare—under this size.” He also mentions (p. 643) a very grave complication in the application of the forceps above the superior strait, viz., the prolapse of the cord. “Statistics show,” he adds, “that out of five applications of the forceps above the superior strait, no fewer than two are complicated by a prolapse of the cord. I have met with it in the ratio of twice in three cases.”

We are now informed that “Cazeaux recommends the forceps for delivery in pelves of 2.75 and three inches, saying that version may be resorted to in pelves of 3.75 inches, differing with Simpson, Madame Lachapelle, and Radford, who say that version should be made in narrowed pelves when the child presents favorably and the narrowing is less than 3.5 or  $3\frac{1}{2}$ ; and he further instructs us that it is an especial condition for version that there be no disproportion between the size of the child’s head and the walls of the woman’s pelvis.” Again, that “Leishman very positively declares that with a pelvis of 3 or —3 inches version should never be attempted. Cazeaux and others confirm this statement.”

Now Leishman (*Midwifery*, Glasgow, p. 569) objects to turning only “when, in round numbers, the conjugate diameter is less than three inches.” In his résumé of the subject he says: “The following may be given as showing, according to the most approved authorities, the degree of conjugate contraction at the brim, which may be supposed, under ordinary circumstances, to indicate the various operations which have been described:

Long forceps,	4	to 3.25 inches.
Turning,	3.5	to 2.75 “
Craniotomy,	3	to 1.5 “
Cæsarean section,	1.5	and under.”

You have just heard what my reviewer makes Cazeaux say. Let us see what he actually now does say. “The ultimate limit to which we restricted the use of the forceps (*Midwifery*, 1866, p. 819) was three inches; because any reduction we could hope to obtain in the diameters of the head beyond that would not, as a general thing, be great enough to permit it to pass through the contracted diameter of the pelvis.” “An attentive examination of this question (version in narrow pelves) has convinced me that Madame Lachapelle, Dr. Simpson, of Edinburgh, and Mr. Radford, of Manchester, were right in preferring pelvic version to the application of the forceps in some cases;” p. 776. Again (p. 823): “If, after several fruitless attempts

made with the forceps upon a favorably situated head, the heart is heard to beat distinctly and regularly, we should, if the pelvis has at least 2.75 inches, attempt the pelvic version before resorting to craniotomy." Further (p. 584), "If moderate tractions are found to be insufficient, the instrument should be withdrawn, and pelvic version attempted, in the hope of extracting a living child."

But it is contended that version is also a painful and dangerous operation to the mother, proving fatal to her in the ratio of 1 to  $10\frac{1}{2}$  (Cazeaux), or 1 in 14 (Churchill). In these days of anæsthetics the "painful" argument against version seems to me to be a very weak one. But let that pass. Turn now to Cazeaux. He, it is true (p. 788), quotes the version-statistics of Riecke, who makes the ratio of maternal deaths 1 in 10.4. But he qualifies them by "considering the accidents which necessitated this operation," and terms it "an easy operation and but slightly hazardous, either to the mother or the fœtus, . . . when performed under favorable circumstances." Again, he says (p. 772), "We are, at present, convinced that the dangers of pelvic version in cases of contracted pelvis have been much exaggerated." With regard to Churchill's ratio of 1 in 14, my critic again fails in accuracy. Churchill, who is a bitter opponent to version, gives (*Midwifery*, Philadelphia, 1855, p. 292) the ratio of maternal deaths from version as one in sixteen. But if we are to be deterred from turning because Churchill makes the death-rate 1 in 16, *a fortiori* ought we to give up the use of the forceps, for (p. 321) he gives one in fifteen as the maternal death-rate after forceps deliveries. Yes, gentlemen, according to a writer, quoted by my critic as an authority on this point, in spite of the performance of version for accidental and unavoidable hemorrhages, for narrow pelvis, for every conceivable grave puerperal emergency or complication, it is less fatal to the mother than is the use of the forceps. Excluding the emergencies and complications of labor, version in narrow pelvis thus becomes by far the least fatal of all obstetric operations. Excluding the emergencies and complications of labor for which the forceps is very rarely used, and limiting it to cases of disproportion and of narrow pelvis, it becomes a very fatal instrument, fully justifying Baudelocque's celebrated remark that it "has been more injurious than useful to society." On the other hand, Schroeder (p. 185) asserts that "The prognosis of extraction by the feet is decidedly favorable to the mother. All the various steps of the operation are devoid of danger to her." And so they are when the operation is one of election, as in turning in narrow

pelves; and not one of compulsion, as in the hemorrhages, transverse presentations, etc., of labor.

I come now to a very important question, one on which much hinges. Is it a fact that those of my tabulated cases with conjugates ranging from  $+3$  to  $3.5$  inches, exhibit "but a slight defect," a defect which can be readily overcome by the forceps? Is it true, that, as urged by Dr. W., "if the mother fails in her efforts with a pelvis of  $+3$ , the case is *easily* and *safely* within the control of a master with forceps"? Is it true that with the forceps, "delivery can be accomplished with safety to both mother and child in the great majority of cases, where the pelvis measures 3 inches and  $+3$ "? Out of the seven thousand cases which Dr. W. supervised at the Philadelphia Lying-in Charity, he offers but one case of successful delivery through a three-inch pelvis—that of Mrs. McN——, and in this single case he has not given the weight of the child or the diameters of its head—a negligence for which under similar circumstances he takes Schroeder to task. Now, although he twice delivered this woman of living children, and although she once delivered herself of a living child, I shall not be so discourteous as to cast the same doubt on the accuracy of his measurements in this case, as he has on mine, in one of my cases (Case IX.), which he could not otherwise dispose of. I shall, therefore, not retort his language by styling this conjugate as one "said to be" three inches, but shall accept this diameter as accurately determined. Yet, is the whole question of treating labor in narrow pelves to be settled by this single example out of seven thousand cases? Is science to hinge on it? Besides, out of these seven thousand cases, he has given us none of his failures, and no statistics of his craniotomy cases, or of the foetal mortality. Can he affirm that he has "easily and safely" delivered all his cases with pelves ranging from  $+3$  to  $3.5$  inches?

One word in this connection on another point: Dr. Wilson states that in this Charity "The mortality to the mothers was less than half of one per cent. in a table of nearly seven thousand cases." Now, I do not for one moment question his veracity or that of the other eminent gentlemen connected with this noble charity. But, as was well shown in the long debate on hospitalism, in the Dublin Obstetrical Society, the statistics of all maternities, where women are delivered at their own houses by students or by recent graduates, are, for many reasons, wholly untrustworthy. For instance, according to one annual report of the Philadelphia Lying-in Charity, 293 poor women were attended in their confinement, and it is added that "during four years 1,253 obstetric patients have been registered

on the books of the Charity, all of whom have recovered." By a strange coincidence, during that very year, I was applied to by two pregnant women to send physicians to attend them at their homes. I referred them to this maternity, but they absolutely refused to apply there, on the ground that each one during that year, had lost a near relative, who had been attended in her confinement by some student of this Charity. Post-partum hemorrhage was the cause of one of these deaths.

But to return from a digression, Meigs' typical illustration of a case of locked head (p. 558), in which the child dies, the soft parts slough, the womb and vagina are lacerated, fistulae form, and the mother perishes, is in "a pelvis only three and a half inches in its antero-posterior diameter." Hodge's "remarkable case" of forceps delivery, which has already been cited, took place in a pelvis measuring 3.25 inches. A great misconception exists in regard to contracted pelves. The difference between the circumference of the head of the boy and that of the head of the girl averages only three-eighths of an inch—a difference which may be represented, according to A. R. Simpson (*Eclinb. Med. Journal*, Dec. 1870, p. 490), by the thickness of a single fold of a table-napkin wrapped around the head. Yet so nicely adjusted is the size of the foetal head to the calibre of the pelvic canal, that this slight difference is enough to make the birth of the male child more than twice as fatal to the mother as the birth of the female child, and to cause the large average of one hundred and sixty male still-births, to one hundred female (*New York Medical Record*, June, 1873, p. 302). Hence, as Meigs shows, "If the child's head is 3.8 inches in its smallest diameter (and this he deems the average size), the dried pelvis ought to be more than four inches in its smallest diameter, in order to allow for it a free transit." Thus, a standard head in a pelvis measuring 3.75 inches, constitutes a difficult labor. According to Cazeaux and Dubois (p. 582), "a spontaneous delivery is possible under such circumstances," but the forceps will usually be needed. Barnes (*Obstetrical Operations*, London, 1871, p. 235) writes, "I claim to speak with the confidence drawn from large experience, when I say that a head of standard proportions and firmness will hardly ever pass a conjugate reduced to 3.25 inches without the forceps, and very rarely indeed with the forceps—that is, alive. I might even extend the conjugate to 3.50 and affirm the same thing." Schroeder quotes (*Obstetrics*, New York, 1873, p. 234) Michaelis approvingly, who contends that "a pelvis is contracted when its conjugate measures less than 3.7 inches." Further, he states that "This, however, by no means gives a complete definition of a contracted pelvis, for numerous very serious dis-



turbances during parturition may occur, as Michaelis himself admits, in the generally contracted pelvis with a conjugate of somewhat more than 3.7 inches."

Let us now see whether in pelves of 3 to 3.50 inches craniotomy is not only often necessary, but frequently a very difficult operation. There is probably not a gentleman in this room who has not seen one such case or more. The medical journals are full of them, but I shall cite only three, and those from the practice of J. Braxton Hicks (*London Obstet. Trans.*, vol. vi., p. 203), one of the best of British accoucheurs. Turning was not resorted to, because either the child was dead or the womb too contracted. Case II.—"The brim was probably a little over three inches in its antero-posterior diameter." "First child born dead after a most lingering labor; second delivered alive by forceps after severe traction, with some laceration to os uteri." In her third labor, Dr. Hicks was called in after she had been in labor thirty hours. He waited twelve hours longer, and finding no improvement, applied forceps, but could not deliver "by legitimate pulling." Craniotomy was therefore performed, but he had "great difficulty in drawing down the head by the craniotomy forceps and crotchet." Case IV.—"Probably antero-posterior of brim was about three and one-quarter inches." "Two attempts to deliver by forceps failed." Perforation with traction being of no use, the greater part of calvarium had to be removed. Case VI.—Had "a brim of not much over three inches in its conjugate diameter." Delivered in first labor "by perforation, after the trial of long forceps, with much difficulty;" in second labor with forceps. In third pregnancy, the induction of labor was advised by Dr. H., but rejected by his patient. He "found the forceps useless, even using them to the limits of safety." He therefore perforated, evacuated brain, fractured bones, pulled with craniotomy forceps and crotchet; and yet could not deliver until he had hooked down the face.

Turn now to W. H. Jones (*Management of Labor in Contracted Pelves*), who gives the statistics of fifty-one cases of deformed pelves observed by him in the hospital of *La Clinique d'Accouchements*, in Paris, under the service of the celebrated Paul Dubois, who applied the forceps to the sides of the child's head, and is cited as an authority by Dr. Wilson.

Of antero-posterior diameters *above* 3.5 inches there were sixteen cases, exclusive of one of "narrowing at inferior strait." Of these, six were delivered spontaneously, two of them being breech presentations; all the mothers recovered, one child was lost. The forceps were successful six times, with two maternal and one foetal death. Craniotomy or cephalotripsy was per-

formed four times, with two maternal deaths. One of the cases of cephalotripsy was after version in a transverse position; for Dubois, who deemed, as my critic avers, traction on the legs "more hurtful than beneficial," and did not know the method of propulsion, had to resort to the cephalotribe. The average length of labor was thirty-three hours; average weight of children six pounds and ten ounces. Just think of it, gentlemen; out of thirteen vertex presentations in pelves over 3.5 and under 3.75 inches, treated by "a master with forceps," such as Dubois was, and with the blades adjusted to the sides of the head, there were three cases of craniotomy or of cephalotripsy!

Of antero-posterior diameters between 3.5 and 3 inches there were fifteen cases. Two ended spontaneously; mother and children did well. One ended spontaneously after the induction of premature labor; mother and child did well. In *two* cases the forceps were successful; mothers saved; one child died. There were three transverse presentations; turning was performed in two of them. The mothers lived; one child died. In the third, spontaneous version and delivery took place, the child, of course, being dead. *Seven* times recourse was had to the cephalotribe, with the death of one mother. The average weight of the children was *five pounds and seven ounces*.

Do not these figures speak for themselves, and utterly disprove the assertion of Dr. Wilson, that "if the mother fails in her efforts with a pelvis of +3, the case is *easily* and *safely* within the control of a master with forceps"? These labors were presided over by the foremost obstetrician of his age, and "a master with forceps;" the children's average weight was only *five pounds and seven ounces*,—a weight "in which," according to Dr. W., "by the authority of Ritgen and Stoltz, the bi-parietal diameter was estimated at less than three inches,"—and yet there were *seven* cephalotribe operations out of *ten* presentations at term of the vertex, and one of the face. Since reading my paper, I have, by turning, again delivered Case V. of a fine living child; this time weighing eight pounds and four ounces. In view of the foregoing facts, I feel fully justified in asserting, that in spite of the adverse criticism passed on them by my reviewer, my tabulated cases show a far better record both to mother and child, than if version had not been resorted to.

To my description of the mechanism of labor in narrow pelves, he replies by referring once to Leishman, and twice "to Meigs and Hodge in proof that the head in vertex presentations, with 3 or +3, does dip, and the vertex does enter the

brim." Leishman makes such an assertion, but it is only an assertion, and he does not discuss it. While, since those two other excellent teachers wrote, the art and science of obstetrics has made great strides. Besides, when compared with European obstetricians, they had but a limited experience in pelvic deformities. Now, I did not contend that the head does not finally dip, when nature is equal to the task; but I showed that it "does not *tend* to dip," and that usually the fore-cone of the head dips first and becomes engaged before the hind-cone, when the narrowing is mainly limited to the conjugate diameter. On the other hand, I state in my paper, that when the pelvis is uniformly contracted, the head enters the brim strongly flexed. If my critic will study the subject of pelvic deformities under the guidance of more modern and more experienced teachers, he will find that there is more than one kind of pelvic deformity, and that these distinctions are important ones. If the head always dips in narrow pelves, what explanation can be offered for the fact that the anterior fontanelle is so commonly found to be either on the same plane or on a lower one than the posterior fontanelle? How interpret the frequency of face-presentations in narrow pelves? Or how, that peculiar rocking or pivot-motion of the head so happily described by our excellent President at a previous discussion. Again, if, as Dr. W. asserts, "The first process in the mechanism of labor is the increased flexion of the head, caused by the resistance offered by the neck and os," how is the head to flex when it cannot descend low enough to bear upon the cervix? Further, what does Dr. W. mean, when he says that "in some cases, where the contraction is great, the head rests upon the brim, and does not press upon the os, which may become soft and yielding, but does not dilate"? Does this mean dipping? Of course, if in such cases nature is equal to the delivery, the vertex must ultimately dip, but it does so usually after passing through the mechanism previously described. And if the physician should wait for the head to dip, before applying the forceps, he would often have to wait until doomsday.

The objection next made is that version means a rapid delivery, and we are warned of "profound fainting fits, followed by hemorrhage, difficult to control, and even fatal in result," that are likely to happen after "rapid deliveries." Now, Cazeaux (p. 530) and with him all obstetric authors define "a rapid labor" as one in which the woman is "delivered with only a few pains." But why is version resorted to in narrow pelves? Because the woman has been long in labor, with perhaps violent expulsive pains, and cannot deliver herself; because the forceps have been tried in vain. Does this

comport with the definition of "a rapid delivery"? On the other hand, I maintain that version empties the womb more equably and less rapidly than the forceps. For by the former, after the slow extrusion of the half-breech and body, the womb is rapidly emptied of only the relatively small head. By the forceps, the head passes the conjugate with a jerk, and is soon born, while the next pain after its birth quickly empties the womb of the more bulky body. No fears of "profound fainting fits followed by hemorrhage" need, therefore, deter us from turning.

The tensile strength of the child's neck is the next point in my paper which arouses all of my reviewer's powers of sarcasm. To disprove the wonderful strength of the child's neck, he advances his own disbelief, a quotation from Meigs, and the alleged adverse opinion of Cazeaux, Dubois and Desormeaux. Now with regard to Meigs' assertion (*Obstetrics*, p. 404), "that the infant will not safely bear a more violent pull by the neck [in pelvic presentations], than it would if dressed and lying in its mother's arms;" this picture is certainly a heartrending one to the laity, and in a case of alleged malpraxis might have its weight with the average jury. But I do not think that either Drs. Meigs or Wilson would be willing to squeeze a head with the forceps as viciously after the child is "dressed and lying in its mother's arms," as they did to effect its birth. Neither would they be willing to make as much traction on its lower jaw, as they may have been compelled to make before it was born. I prefer facts to sentiment, and the facts are that, unless the neck be twisted, it will stand a wonderful amount of traction. Meigs himself (p. 578) in describing a difficult craniotomy case remarks that "I got a firm hold of the neck, and with the exertion of the *greatest strength* gradually got the button-like remainder of the head out of the vulva." Dr. R. Stewart, in one of the discussions on my paper, stated that after breaking up the head in a narrow pelvis, the shoulders refused to pass. He, therefore, twisted around the neck of the foetus a long towel, which was pulled upon by two physicians "*with all their might*," before the body was delivered. Yet, in spite of this enormous traction, certainly over two hundred pounds, the neck was unbroken. In my paper I gave a crucial example of the strength of the child's neck, and also my reasons for believing that a living child's neck will resist greater traction than that of a dead one. But it may be retorted that such traction is not compatible with life. Granted, in so far as the traction of two stalwart men is concerned. But I have successfully put as much weight upon the legs of a child as I have on the forceps-handles—a weight, as I believe,



of fully one hundred and thirty pounds. Barnes, who has had the large experience of over three hundred cases of version, tells us (*Obstetric Operations*, chap. xvi.) as the result of many successful deliveries, that "Surely no one can doubt that the traction-power, and, therefore, the compressing power, acquired by pulling on the legs and trunk, is greater than can be exerted by the strongest forceps." The harrowing scenes at the scaffold, when the rope snaps, or the prolonged struggles, when the neck is unbroken, show conclusively that no amount of traction *per se*—traction without twisting—will destroy life unless the spinal column yields. Now M. Duncan has shown, that, after the neck of an infant has been broken, it takes from fifteen to twenty pounds more before the head parts from the body. Accepting these data, the conclusion is logical that a living child may be born if the tractive power has not reached to within fifteen or twenty pounds of the decapitating limit. But what stronger proof of the viable resistance of the child's neck to traction can be adduced, than that unconsciously offered by my critic himself in the following very apposite quotation from Hodge: "It would be very easy, from the records of the profession, to detail the horrible results of forcible traction upon the limbs and body of the child, when such traction was made not at the proper time, or in the proper direction,—when brute force has been substituted for art and science. Perhaps there are but few practitioners who have not seen or heard of cases where the *strength of two individuals* has been applied to the body of the child, while that of three or four, or even more, attendants to the body of the mother to accomplish the delivery of the head after the body has been born. *That the child should ever escape with its life* under such circumstances is surprising."

Six and even more persons pulling away at mother and child, and yet, as Hodge clearly implies, the child has escaped with its life! When compared with this enormous amount of traction, what is the weight of one hundred, or even of one hundred and thirty pounds, which I have on several occasions *successfully* put on the necks of children, but which Dr. W. "cannot be persuaded to believe." We are told in this relation, that "Cazeaux says that traction upon the child's neck, when there is a great obstacle to its birth, is fatal," and that "Dubois and Desormeaux say that traction upon the child's neck is more hurtful than beneficial." Cazeaux, as I have shown, does not deem a conjugate of 2.75 inches as an objection to version made with the object of saving the child's life. As for Dubois and Desormeaux, Cazeaux (p. 772), from whom Dr. W. gains this information, makes them, it is true, object to traction; but



from a foot-note (p. 454) it will be seen, that Desormeaux there objects simply to pulling on the legs before the arms are delivered. In his brochure (*Précis de Doctrine sur l'Accouchement par les Pieds*), I find not only that he delivers by traction, but that he scorns the idea of uterine inertia and of hemorrhage after version.

The fears entertained by my reviewer about the production by version of lacerations of the perineum into the rectum, and his "always feeling disgraced when it occurs in my (his) practice," convey the impression that he is singularly unlucky in his very creditable efforts to preserve the integrity of this body. But as this is probably a verbal carelessness of expression on his part, I shall treat him more generously than he has treated me, and shall make him say what I believe he intended to say and not what he does say. With regard to this alleged danger of lacerations after version, I have successfully operated in quite a large number of cases of torn perineum, and upon inquiry have found that, save in one instance, they were the results of instrumental delivery. I will venture to say that in nine out of ten cases of rupture into the rectum, the forceps will be found to have been the cause.

This ought not to be so, but it is so, and for a case in point let me cite "Mrs. L.," referred to by Dr. W. She was delivered by forceps "under the care of a distinguished professor of obstetrics in a Western city, assisted by an eminent physician in the place," and yet the "perineum was torn, the rent extending one and a half inches into the rectum." On the other hand, every one must admit that after the vagina and perineum have been stretched open by the half-breech, no great laceration can at the worst be produced by the head. In all my breech—and version—cases, I can recall but one in which the perineum was at all torn, and that was a case of version for a face presentation. In cephalic presentations I have, like my reviewer, seen the perineum give way, but I have so constantly cured the rent by the silver sutures, that I should have no misgivings about causing such a lesion were it necessary thereby to save the life of the child. On this very point, W. Tyler Smith has expressed the opinion that "Surely it is better in any case that the perineum should be torn by the foetal head, or by the forceps, than that a living child should be killed" (*London Obstet. Trans.*, vol. i., p. 29).

With regard to the "contusions of the parts followed by inflammation, sloughing, etc., and resulting in vesico- or recto-vaginal fistulae, or septicæmia, or even the death of the patient," which my critic lays much stress on, these are mere assumptions. In point of fact the danger of these lesions and acci-

dents is far greater when the forceps is used for one hour or more. Schroeder (p. 185), writing of version, contends that "All the various steps of the operation are devoid of danger to the mother; only when there are disproportions in size between the head and the pelvis are the maternal soft parts compressed by the head passing through them. This pressure, acting once and only for a very short time, is known not to have any bad effect, and, as a whole, extraction is at times more favorable to the mother than delivering with the head presenting." "It is," writes W. H. Jones (*loc. cit.*), "the *duration* rather than the *intensity* of pressure upon the soft parts, which produces mischief. And I base this assertion upon statistics that have been established by many writers."

But what are the circumstances in my tabulated cases which have aroused this solicitude of my critic for the mother's soft parts? What the reasons he offers for these fears? By incomplete quotations from the history of these cases and by divorcing them from the context, he makes me say what I do not say. For instance, in Case V., I am made to say that "I turned the child and delivered it alive in less than three minutes, etc."—giving the impression that the operation of turning and delivering occupied less than three minutes. What I inculcate is to deliver the breech slowly so as to dilate the parts. What I really do say in this case is that "I delivered it alive in less than three minutes after strong traction and propulsion had been begun;"—that is, after the breech and body had dilated the soft parts and had been delivered, the arms brought down and the head alone remained for delivery. To show that "the neck of the uterus may be injured, for time is not always allowed for dilatation," he makes me say in Case III., "Two fingers of the left hand were then squeezed into the os and ultimately a third;" when I afterwards state that "One foot having been dragged down, time was given for the os to dilate before the body and arms were brought down." In Case VIII., he makes me say by quotation marks that "the head *bounced* out of the grip of the brim and the child was born;" when I state that "the head bounced out of the grip of the brim" and "after a short but anxious detention at the cicatricial band, a living and lusty child was born." Finally, he makes me advise, in contracted pelves, that a child should be "pulled out with the arms either over its head or brought down." Now, all the ground for this assertion is the following remark that I made in another paper,—that on "Head-Last Labors:" "That in cases of pelves *known to be ample*, I can *conceive* of its being perfectly justifiable to follow Gifford's and Froriep's plan of dragging the head through with the

impacted arm extended above, rather than that of losing golden minutes in liberating it."

Time forbids me to expose other misapprehensions and misstatements on the part of my critic; and also to purge my Cases from partisan interpretation. Suffice it to say, that he has taken the same liberties with them that he has with most of his own authorities. Since he has played such havoc with modern authors—authors in the vernacular—we may well feel thankful that "time fails" him "to discuss the modes resorted to for the delivery of women in the second, fifth, and seventh centuries." The physicians of "the seventeenth century" may also congratulate themselves on their escape.

To sum up, the staple of his argument is, that version cannot be the proper thing, because he twice successfully delivered *one* woman, who also delivered herself through a three-inch pelvis; and because this or that physician said or did so and so—or rather, as I have shown you, did not say or do so and so. But I forget there is one argument kept in reserve, like the imperial guard, as a clincher. It is that the "versionists aim to bring to naught the promise that 'in sorrow thou shalt bring forth children.'"<sup>3</sup> This curse, and the corresponding one on man, are not new arguments. A good deal of respect should be shown to them on account of their age. From time immemorial they have been the stock arguments against all inventions and improvements in the arts and sciences. The one or the other has been advanced against steam, the loom, and all labor-saving machines; against the forceps, and the use of ether in labor, and finally, in the wane of the nineteenth century, by my reviewer against version. So the "versionists," as he contemptuously calls them, do "aim to bring to naught the promise that 'in sorrow thou shalt bring forth children.'"<sup>3</sup> All honor to the "versionists" then say I, while I refer my critic to a careful perusal of the pious Simpson's reply to this time-worn and mediæval argument.

In conclusion: Obstetrics is not one of the exact sciences, and in our penury of truth we ought to be accurate in our statements, generous in our doubts, tolerant in our convictions. Without these qualities science cannot be promoted nor truth educed. Above all, as Milton has said, "Truth needs no policies, no stratagems to make her victorious." Surely, then, arguments based upon misstatements and misquotations show a weak cause. But when, in addition, the Book of Genesis is brought into a medical discussion, when the curse pronounced upon Eve is invoked as an argument, and when a grave and a vexed obstetric question is settled by an appeal to Moses and the Prophets, the cause must be a hopeless one.

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DR. ISAAC S. ESHLEMAN then read the history of the following

CASE OF VERSION IN A CONTRACTED PELVIS.

Since the beginning of the discussion on Dr. Wm. Goodell's paper I was called to assist Dr. J. J. Maguigan in the delivery of a woman whose pelvis was greatly narrowed in its conjugate diameter. Meeting with difficulties in the case that embarrassed very seriously if they did not wholly preclude the use of the forceps, I felt justified in resorting to version, notwithstanding the very decided objections I have entertained concerning this method of delivery. I will describe the case as treated by a succession of expedients, embracing one that I believe is novel to the profession, in the hope that a scintillation from the humblest source may add to the flood of light now pouring in upon obstetrical surgery. The mother was a well-developed, healthy woman, at full term with her second child. The pelvis was kidney-shaped, but larger on the right than on the left side, with a conjugate diameter of about three inches. The child weighed fully nine pounds. The head lay transversely, with the vertex on the right of the promontory of the sacrum, but looking toward the left. The forceps were applied laterally, but slid from their hold. Examination made with the instruments applied demonstrated that the blades were arrested by the right iliac bone, while the head was large and somewhat extended, so that they simply grasped the back part of the head vertically. The waters having been evacuated for twelve hours, hard labor pains had jammed the head and shoulders in this position. No foetal life could be detected, yet as sufficient cause could not be assigned for the death of the child, craniotomy was not entertained. It is an alternative to which I have never been driven in my practice. The mother had been delivered, when a primipara, with the forceps, by Dr. Magnigan, of a living child; but the Doctor represents the head as having passed the superior strait without aid, the child being only half the size of the present one, and probably as presenting vertex right to the wider side of the pelvis. After due deliberation we decided to lay aside our usual expedients, and try podalic version, the method of delivery so ably advocated and successfully practised by Dr. Goodell. A mixture of ether and chloroform was administered. In consequence of the early rupture of the membranes, and the child being (as afterwards proved) dead, a foot was with great difficulty brought down to the orifice of the vagina, while the body scarcely changed its position; nor could it be moved until a fillet was applied to the foot, at which Dr. Magnigan made very strong traction,

while with one hand in the vagina and the other on the abdomen I exerted a very considerable amount of power upon the head and body, giving the uterus time to dilate in its lateral walls, when version was finally accomplished. Then followed the greatest resistance to the least progress. Labor pains had well nigh ceased, although ergot was freely used. The hips were at length delivered with great difficulty in the hands of Dr. Magnigan. He also succeeded in extricating one arm, both being up. When he became exhausted, in turn I obtained the other arm. We were now well satisfied that we had been laboring with a dead child from the beginning. Turning had brought the vertex to the right, where we wished to engage it.

Applying a towel about the body of the child, I now proceeded to use proper movements to obtain "the leverage." With Dr. Magnigan's quota of pressure upon the vault, I brought the child first down, then forward, and finally back against the perineum, under a traction force of about 100 lbs., with the effect only of severing the cervical vertebrae. This rather surprised me; my first experience in this line—evidently too heroic for the neck of a *dead* child. Pretty well exhausted, and disgusted with what I had done, I resolved next to operate on a theory according to which I practise in head-first labors with the forceps. I therefore asked Dr. M. to elevate well the body, pressing the neck against the pubic bone, while I moulded the head to one side, working the edge of the cranial base laterally under the promontory of the sacrum by external supra-pubic manipulation and pressure. This done, persistent backward and downward pressure, from above, readily extricated the head with less than half the traction power before used in a backward direction, now simply directed in a line with the body.

Having practised on this theory in narrow conjugate diameters in head-first cases with the forceps successfully, I was induced to try the same, by reversing the rule, in this head-last case without the forceps. It is, I am inclined to believe, upon this principle of canting the base that Dr. Goodell succeeds so well, although he does not represent it as the *modus operandi* as here explained; nor do I know that any other accoucheur ever so taught or practised. According to this theory, the relative measurements of the child's bi-temporal and the mother's conjugate are ignored. The child's greater bi-temporal and parietal diameters pass geometrically the lesser conjugate without shortening of the cranial base by compression. In a well-formed pelvis, when the forceps are applied and traction made, the handles will show that the head, in either the left or right occipito-anterior position, borders very closely upon the vertex



symphysis position. In these instances, the head being flexed, dips the cranial base almost perpendicularly into the pelvic cavity in the line of its *long diameter*. But in the case of an ordinary-sized head, when the conjugate is shortened, or as more frequently occurs in practice with a relatively large, broad head having increased temporal and parietal diameters, when the pelvis is normal, one parietal protuberance is found to lodge on the pubic bridge, while the chin departs and the other parietal protuberance sinks below, is moulded and passes under the promontory of the sacrum, dragging after and dipping or canting the *short diameter* of the cranial base of the same side laterally. If left to nature, and the disproportions of the head and the pelvis are not too great, the head will, under good labor pains, be gradually moulded into a rhomboid, forming a wedge of one side or edge of the cranial base, which dips down, following slowly the cephalic mass, and passes under the promontory of the sacrum far enough to allow the other side to pass the pubic bone. Should the efforts of nature prove insufficient in these cases, the forceps, when applied, may be adapted according to this indication in the fronto-mastoid diameter, in a measure diagonally, with one blade pressing *under* the sacral side of the cranial base, while the other presses *over* the pubic side of the cranial base, compressing the head into a rhomboid and causing the base of the skull to dip laterally in sufficient measure while it passes under the promontory of the sacrum. Now in head-last deliveries with narrow conjugates, where with this lateral dip the cranial base precedes the cephalic mass, possibly this movement of the head may be more readily executed than in head-first cases. If so, version early resorted to in very narrow conjugates may, without much risk to the mother, be the means of saving the life of the child where the forceps would fail.

Operating on this theory with the forceps, applying one blade behind the sacral, and the other in front of the pubic ear, making the pressure diagonally with the cranial base in the fronto-mastoid diameter, I have always been able to deliver with the forceps, head first, until I encountered the present case; and I am by no means sure that I should not have been successful in this case, by reversing the forceps and adjusting the head, had I not been desirous of trying the head-last method of Dr. Goodell's paper. The mal-position of the child's head, lying as it did entirely on the right side of the pelvis, with the chin departed, might have been corrected by manual effort, just as the head may be restored in a shoulder or arm presentation, the books to the contrary notwithstanding. This I was compelled to do in this case after I had obtained the foot

before the child could be turned. The head was restored in the course of the revolution it made as it swept across the pelvis from right to left. This, as done, did not necessitate the effort required to obtain and bring down the feet as in turning; neither did it incur the risk of forcing the long diameter of the child through the short diameter of the uterus. It comprised none of the hazards of turning. At this stage the forceps might have been applied, and delivery effected in either the L.O.A. or the L.O.P. position, or possibly in the R.O.A. position. With the occiput left, the broader parietal part of the head would have been brought unfavorably in relation with the more contracted side of the pelvis; and to turn the head occiput right might have tried the neck. Yet, with a living child and a three-inch conjugate, one of these expedients would be preferred by me to either turning or craniotomy. The case of turning and delivery here reported was followed by metritis, but after remaining in a critical condition for some time the woman made a good recovery.

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STATED MEETING, JULY 1ST., 1875. DR. A. H. SMITH, PRESIDENT, IN THE CHAIR.

#### ABORTED OVA.

DR. SAVERY presented three specimens of aborted ova.

The first was the result of a pregnancy of three months' duration, characterized by the various symptoms, as sick stomach, etc.

The abortion was preceded by considerable hemorrhage requiring the use of a tampon.

When the tampon was removed the mass was found lying in the vagina.

The sack was full of liquid, but the most careful examination failed to discover the fœtus.

The second ovum was about  $2\frac{1}{2}$  months of development, and the third of a very early age, probably about six weeks after conception; but in neither was there a trace of a fœtus.

DR. GOODELL asked what were the pathological changes taking place, which produced a state of things like that shown by Dr. Savery's specimen.

DR. INGHAM replied that there had always been in these cases doubts whether they were aborted ova or not.

There have been three cases before this Society. One of these was referred to the microscopical committee, and they reported the presence of villi of the chorion. He thought that in these cases there was an early death of the fœtus with subsequent development of the chorion, and the fœtus had been dissolved by the amniotic fluid.

## QUARTERLY REPORT ON OBSTETRICS AND DISEASES OF WOMEN AND CHILDREN.

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THE TENDENCY TO HABITUAL DEATH OF THE FŒTUS IN UTERO  
AS AN INDICATION FOR THE INDUCTION OF PREMATURE LABOR,  
WITH REMARKS ON THE PATHOLOGY OF THE PLACENTA AND  
FUNIS.

IN the *Archiv für Gynäkologie*, Vol. VIII., No. 2, DR. GERHARD LEOPOLD, of Leipzig, gives a very thorough *résumé* of the nature, causes and treatment of habitual intrauterine mortality, and sums up the conclusions arrived at as follows :

1. The tendency to habitual abortion is due to various causes, viz. :

(a) *Syphilis of the parents.* (The fœtus may either be born in the early months—hydrops sanguinolentus of Martin—in a macerated condition, presenting no signs of the disease, or towards or at term, alive or dead, generally but short-lived, and showing distinctly the traces of specific disease in its organs, or the placenta or funis; in cases where the father alone was syphilitic, the subsequent marriage of the mother with a healthy man will occasionally prove a cure for the habit.)

(b) *Anæmia or blood-anomalies of the mother.*

(c) *Chronic diseases and irritation of the uterus* (especially chronic metritis and local irritation—too frequent coition during pregnancy).

(d) *Individual general irritability* (excessive delicacy of connection between external general exciting causes and the motor nerves of the uterus).

(e) *Hereditary disposition.*

(f) *Malformations or diseases of the placenta or umbilical cord*, particularly in their vessels, which do not depend on syphilis, and the origin of which is still obscure (torsion or constriction of the umbilical vessels, placentitis, etc.).

2. The rational treatment of habitual abortion can consist only in the removal of these chief causes.

3. Of these, the first four are amenable to treatment, which consists :

In (a), in antisyphilitic remedies.

In (b), in building up the system.

In (c) and (d), in a suitable local tonic and restorative treatment of the sexual organs.

The causes (e) and (f) are scarcely to be reached by therapeutic measures.

4. In accordance with these facts the logical conclusion would be, that the induction of premature labor, is to be discarded in the first four cases, but to be sanctioned in the last two. This is not the case, however, and the first four causes are to be separated in accordance with their importance. For,

5. In consequence of parental syphilis the fœtus is always infected in cases of habitual intra-uterine death. Whether the death has usually occurred at an earlier (8th month) or later period (10th month), *premature labor*, even though induced about two weeks before the usual time of death, *will evidently exert no influence on the infection of the fœtus, which may be born alive, of course, but only to lead a miserable and short-lived existence.*

Consequently, in this respect, the opinions of Martin, Döbner, Scanzoni and Spiegelberg deserve unqualified support, the latter of whom has but lately expressed himself most decidedly to the effect, *that the habitual death of the fœtus in utero in consequence of parental syphilis is not to be considered an indication for the induction of premature labor.*

*With the other causes, however, as several cases teach us, the fœtus is not diseased.*

6. *With these other causes, therefore, the induction of premature labor is to be recommended the more, the nearer the habitual disease of the fœtus in utero approaches the regular end of gestation.*

The *most favorable time* is shown, by a series of cases, to be about one to two weeks before the period when the fœtus was in the habit of dying, particularly the thirty-seventh week of utero gestation. The *very latest* period is after the beginning of the usual symptoms of the habitual intra-uterine death, and the *most unfavorable*, when these symptoms have already existed some time, even though the fœtal pulse still indicate the life of the child.

7. Therefore, the regular intra-uterine disease of the child in the last month of pregnancy is to be considered a decided indication for premature labor, dating from the thirty-seventh week, *because an almost mature living child with fair chances of continuing to live must be of greater value to us than a child which is one to two weeks older, to be sure, but which each additional week renders more liable to intra-uterine death.*

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REMARKS ON THE PATHOLOGY AND PHYSIOLOGY OF THE CIRCULATORY APPARATUS IN PREGNANT AND PUERPERAL WOMEN.  
By DR. HEINRICH FRITSCH, of Halle. (*Archiv. für Gyn.*, viii., 3.)

I. *The dangers of mitral disease.* The author refers to the entire omission or slight mention made of the perils of organic cardiac lesions in connection with gestation in ancient and modern text-books, even the most recent of which, such as Nægele and Leishman, entirely pass the subject over, while others, such as Hohl, Cazeaux and Hecker, refer to them casually, the last named reporting cases, without, however, offering a physiological explanation of the phenomena witnessed by him. Spiegelberg (*Arch. für Gyn.*, II.) was the first to investigate this complication of gestation, and to search for reasons for the increase of danger from disease of the mitral valve during parturition. Fritsch reviews his conclusions as follows: 1. The amount of blood in the body is normally increased during pregnancy. 2. In consequence of this increase of blood the heart becomes stronger, undergoes excentric hypertrophy (Gerhard doubts this enlargement, as also Fritsch, who thinks that the heart and vessels merely dilate slightly, and accommodate themselves without the former becoming hypertrophied). 3. In abnormal conditions of the heart, this system of compensation and accommodation to the increased volume of blood during pregnancy does not suffice, and consequently the heart's action is more or less hurried or labored, the general *malaise* increased, and the local cardiac disease aggravated thereby. 4. During physiological labor the blood-pressure, both arterial and venous, rises while a uterine contraction is present; thus various viscera, brain, heart, lungs, kidneys, etc., are temporarily congested; a sudden rush of blood of this kind into a hypertrophic diseased heart, caused by the uterine contraction, and the forced expiration accompanying it, paralyzes the heart and causes instant death. 5. After the evacuation of the uterus the abdominal veins, which had been more or less obstructed during pregnancy by the pressure of the enlarged uterus in physiological cases, become filled with venous blood, and the cardiac function is for a time more or less deranged; in normal cases this temporary derangement occasionally manifests itself by an anæmic appearance and a small anæmic pulse, but it is soon compensated for. In mitral disease, however, the facts are different, and may briefly be enumerated as follows: The hypertrophic heart does not receive a sufficient amount of blood (owing to the abdominal venous engorgement), the heart consequently contracts irregularly, too little blood enters the



circulation, and its decarbonization is imperfectly performed; vitality suffers in consequence, the imperfectly nourished cardiac muscle loses vigor, its suction-power diminishes; the evacuation of the uterus also produces cerebral anæmia and dyspnoea, the puerpera becomes pale, cyanotic, the pulse no longer corresponds with the cardiac contractions, it is weak, small, arhythmic; death ensues suddenly, from "paralysis of the heart," an unscientific term, which we are compelled to retain because there is a species of death which cannot be described as well otherwise.

According to Fritsch's observations, predominating mitral *obstruction* is the most dangerous. Of aortic disease he has no records, and therefore enters into no theoretical speculations.

II. *Puerperal retardation of the pulse.* A gradual retardation of the pulse is often very marked in healthy puerperal women, beginning generally from twelve to sixteen hours after delivery, and lasting an indefinite time—indefinite, because the women, being perfectly healthy, soon withdraw from medical observation. The only scientific explanation for this fact, thus far, has been offered by Blot, who says that the arterial pressure is increased in consequence of the increase of the volume of blood; that therefore the total blood-pressure is too great, and the frequency of the pulse is diminished, and bases this explanation on the doctrine of Marey: "The frequency of the pulse changes in an inverse ratio with the pressure in the arterial system." Although this rule may generally be correct, Fritsch denies its applicability to the present case. After reviewing various theories as to the cause of this phenomenon, such as the influence of irritation of the pneumogastric dependent on irritation of the uterine nerves, the loss of peculiar substances which are supposed to circulate in the blood during pregnancy and to stimulate the growth of distant portions of the body (*Goltz, Arch. für Phys.*, 1874), and showing that they are as yet mere vague hypotheses, F. gives the following simple and apparently sufficient explanation: "If muscular action becomes minimal—as is always the case during the puerperal state—the amount of oxygen required by the body will also sink. Consequently, the quota of carbonic acid is smaller than during standing or walking, for instance. The respiratory centre is less frequently irritated by the disharmony between want of oxygen and excess of carbonic acid. Respiration must diminish in frequency, with it the pulse, just as both become slower during sleep, especially during the hibernation of animals." Analogous instances are not uncommon, for the pulse of any person will vary from ten to twenty beats in the recumbent position

and while standing or walking, and a similar retardation is often observed during convalescence from febrile diseases.

III. *The physiological bruit de souffle with the first heart-sound in puerperal women, with remarks on the origin of the first sound and of accidental murmurs.*

Having frequently noticed a murmur with the first heart-sound in puerperal women, which he at first mistook for the result of organic disease, Fritsch made a large series of observations, and found that *in a large number of puerperal women, the first heart-sound is changed to a soft blowing murmur, a so-called bruit de souffle.* This phenomenon was found both in anæmic and plethoric women, as early as two hours, or as late as the second or third day after delivery, and was most distinct at the base or apex of the heart. Occasionally the first sound disappeared entirely, and only one heart-sound was audible. On assuming the sitting posture the murmurs often disappeared entirely. *In all of these cases the second heart-sound was and remained diastolic,* probably because the two semi-lunar orifices do not close simultaneously. Fritsch believes that it is not the vibrations of the walls of the vessels, but the velocity of the fluid in them which produces the sound. Therefore the first heart-sound is a fluid-sound, and its change to a murmur in puerperal cases is caused by the relatively small amount of blood flowing to the heart after the diminution of intra-abdominal pressure. To the same source must also be referred other accidental anæmic and chlorotic cardiac murmurs in lying-in women.

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THE TIME OF COMMENCEMENT OF LABOR. By PROF. LUDWIG KLEINWÄCHTER, of Prague (*Zeitschr. für Geburtsh. u. Frauenkrankh., Berlin, i., 2*).

THE only papers in which the influence of the time of day on the commencement of labor is discussed, which K. was able to find, are by Berlinski and Veit. With the object of throwing some light on the unsolved question of the immediate exciting cause of labor, K. examined the records of 3,408 cases, which were reported with the necessary accuracy, and arrived at the following conclusions:

1. The majority of labors begin between 4 p.m. and 6 a.m.; the pains begin most frequently between 10 and 12 p.m.; most rarely twelve hours sooner.

2. In primiparæ the influence of the time of day on the commencement of labor is about the same as in all women in labor.

3. In multiparæ the favorite time for the commencement of

labor is between 4 and 12 P.M.; these are the only hours during which the number of commencing labors rises above the average.

4. Between the ages of twenty-one and twenty-five years, the acme of female procreative life, the time of day exercises the most influence on the beginning of labor. From 7 A.M. to 3 P.M. begin the smallest number of labors; the most unusual time is from 10 A.M. to 12 M.; the most frequent twelve hours later.

5. If the woman is younger, the influence of the time of day decreases; besides, the time of the largest average advances by three hours.

6. With advancing years the same decrease is also noticeable, the more, the older the woman is. The time of the smallest average also advances by three hours.

7. In male and female births the influence of the time of day seems to be nearly equal, if anything less marked in female labors.

8. In labors at term, the influence of the time of day is about the same as in all women in labor.

9. In labors before term the influence of the time of day on the commencement of the labor is least marked. Irregularities in the curve are here met with, which bear no analogy to those occurring in multiparæ and advanced years.

The only deduction which K. is able to draw from these investigations, is that women who have not yet reached, or who have already passed the bloom of life, also multiparæ and women not yet arrived at term, all these respond more actively to the exciting causes of labor, whatever they may be; in them the beginning of labor is divided more equally throughout the twenty-four hours, than in primiparæ of twenty-one to twenty-five years, who are delivered at term. Further, that the exciting causes of labor, which naturally are likely to be more active and frequent during the twelve hours of daylight, are not followed by a reaction on the part of the uterus until several hours later, that is, generally during the following night.

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THE BOUNDARY LINE BETWEEN THE BODY AND CERVIX OF THE UTERUS IS VISIBLE ON THE ABDOMEN OF THE PARTURIENT WOMAN, says DR. LUDWIG BANDL, of Vienna (*Trans. German Med. Assoc.*, 1875, *Arch. f. Gyn.*, viii., 3), whenever the uterus during the expulsion of its contents meets with abnormal obstacles. The arrangement of the muscular fibres of the body and cervix, as demonstrated by Luschka, readily explains this statement. B. first pointed out this pheno-

menon in his communications on rupture of the uterus, and has since distinctly seen the transverse furrow about midway between the umbilicus and symphysis in four cases in which vigorous uterine contractions had wedged the presenting part so firmly into the narrow brim of the pelvis as almost to incarcerate the lower portion of the cervix. The muscular fibres of the cervix then become paralyzed and incapable of con- and retraction, and the cervix remains long and flabby, or even expands. B. injected the uterus of one of his cases, which died of puerperal fever, with tallow, and the same transverse furrow became visible between the distended cervix and the body of the organ; a longitudinal section of the frozen specimen demonstrated the inward projection of the uterine tissue at the junction of body and cervix. The value of this observation as an indication of the progress of the labor, and an aid in choosing the time and species of operative interference, is evident. Important also is the influence of this condition of the uterus on subsequent labors, for it is probable that hyper-distention and stretching of the fibres of the cervix is followed by sub-involution of the uterus, and consequent greater or lesser functional disability of the organ during labor. In perfectly normal labors this furrow is never seen, because the presenting part fills out the whole cervical canal equally, and every pain presses it deeper into the pelvis.<sup>1</sup>

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THE RAPIDITY OF OSMOSIS BETWEEN THE MOTHER AND THE FÆTUS has been studied by DR. BENECKE, of Berlin (*Trans. German Med. Assoc., Arch. f. Gyn., viii., 3*), by giving salicylic acid to twenty-five mothers during labor, in doses of 1.5–2 grammes (25–35 grains), drawing the urine of mothers and children immediately after delivery, and testing it with a very dilute solution of the chloride of iron, with which salicylic acid gives a violet color. His results were as follows:

1. *The osmosis between mother and fetus is very active.* The salicylic acid was always given after the pains had commenced. In two cases in which the children were born ten and fifteen minutes later no salicyl was found in its urine. In two other cases, where the children were born forty and eighty-five minutes afterwards no salicyl was found in the urine drawn immedi-

<sup>1</sup> We distinctly remember noticing this transverse furrow in several cases of labor rendered tedious by excessive obliquity of the pelvis and consequent anteversion of the pregnant uterus; the oblique position in which the presenting part, particularly the unyielding head, enters the pelvic brim, for a time materially obstructs its descent into the pelvis, and simulates the condition existing in labor complicated by slight contraction of the conjugate diameter.

—ED.

ately after birth, but appeared in that removed several hours later. In all the other cases salicyl was found in the urine of the child immediately after delivery. The shortest time from its administration till delivery was two hours. The drug has thus passed into the foetal system after forty minutes, and appears in its urine within two hours.

2. *The osmosis of the child itself is much more active immediately after birth than in the uterus.* This is proved by the much stronger reaction for salicylic acid in the urine removed several hours after delivery, than in that drawn immediately after birth.

3. The urine of the mother shows no traces of salicyl as soon as twenty-four to thirty-six hours; in the urine of the child it appears up to the third or fourth day.

In four of the twenty-five cases in which the liquor amnii was obtained pure and unmixed, it was found to contain no salicylic acid even when the drug had been given from six to fifteen days before delivery; yet it always appeared in the child's urine. This result contradicts Gusserow's opinion, that in the later months the urine is the chief source of the amniotic fluid, and that of Schatz, who believes that towards the end of pregnancy the foetus often swallows liquor amnii, and excretes it again through the kidneys and skin. The practical value of these experiments of Benecke is apparent in their relation to the transmission of nutrient material, of the germs of disease, and of drugs from the mother to the child.

OVULATION AND MENSTRUATION.—Prof. BISCHOFF, of Munich, the veteran searcher into the mysteries of generation, in a long article in the *Wiener Med. Wochenschrift*, Nos. 21-24, 1875, gives his views on the new theory which is rapidly gaining ground, that ovulation is not necessarily coincident with menstruation, and that the ovum is discharged *before* the menstrual period to which it belongs, not *subsequent* to it, as has been the hitherto generally accepted opinion. The great importance of the subject and the general interest it excites will justify us in briefly reproducing the principal points of Prof. B.'s article. He persists in his old view, based on undeniable experimental evidence, that the mammals join in the general law of organic nature, according to which the germinal substances, ova and spermatozoa, ripen at regular typical intervals in both sexes, subsequently become detached from the parental system and follow their separate paths without reference to each other, but with the intention, to be sure, of meeting; and also, that the



time of so-called œstuation in them is also the time of ovulation or maturation and discharge of the ova.

The commencing formation of a decidua in the uterus is as necessary and indispensable to conception and pregnancy as ovulation. But the hemorrhage may be absent, and doubtless often is absent, especially if conception has taken place with that very ovulatory period, as, for instance, when coitus was performed shortly before the beginning of menstruation. Enlarged Graafian follicles in new-born infants, and cicatrices and corpora lutea in the ovaries of girls up to twelve or fourteen years of age, are decidedly the exception in our climate. With the exception of twins and triplets (and not always with them, for two and three ova have been found to have escaped from one Graafian follicle), no one has ever observed more than one ruptured Graafian follicle, or several corpora lutea in the same stages of development during the same ovulatory period. Infantile ovulation is thus not countenanced by Bischoff.

Although cases of early conception, or conception without preceding menstrual hemorrhage, or of extruterine pregnancy, prove the independence of ovulation and conception from the changes and hemorrhage in the uterus, in those cases they prove it only as a rare exception, not as a rule, and by no means refute the law that ovulation, formation of a decidua and uterine hemorrhage are necessary to the normal development of the whole process of procreation.

The occurrence of regular uterine hemorrhage independently of the ovaries (when they are degenerated or have been removed) is possible, and explicable on the ground of a habit acquired after many years of regular typical congestion and hemorrhage coincident with ovulation.

The order in which the stages of the process of generation follow each other, are: 1. Ovulation in the ovaries; 2. formation of decidua and hemorrhage in the uterus; and, 3. action of the sexual impulse to bring the two generative substances together.

It is an indispensable law for the human race, as well as for animal creation, that menstruation depends on the maturation and discharge of an ovum from the ovary, and that impregnation and conception directly depend on this process. The ripening and discharge of an ovum no more depends on sexual intercourse, than does menstruation.

How long a matured and discharged ovum is fecundable has not yet been ascertained; from analogy in animals, B. believes until the middle of the inter-menstrual period, but it is impregnable only in the Fallopian tube, not in the uterus. Coition at the end of a menstrual period may be fruitful, be-

cause the spermatozoa may remain alive in the genital canal for eight and nine days, and may therefore meet the ovum discharged at the new menstrual epoch. B. and Leuckardt have decided, by statistical research, that there is a period between two menstrual epochs which is unfavorable to impregnation.

The modern theory advanced by Power, Gusserow, Sigismund, Löwenhardt, Schroeder, and apparently supported by the anatomical investigations of Kundrat and Williams (who differ, however, as to the degree of intra-uterine change during menstruation), to the effect that conception never occurs immediately after, but always through a coitus *before* an ovulatory period, because the impregnated ovum will only then find a soil favorable for its development, before that soil is destroyed by the expected hemorrhage, is certainly very plausible, and would explain the well-known fertility of the Jews, notwithstanding the Levitical law, as also the cases of impregnation without preceding menstruation. Still B. thinks that this theory is not proved, and that Kundrat's investigations do not show conclusively that the menstrual decidua at the time of the hemorrhage really is in process of destruction, that the hemorrhage is caused by this process, and that the latter has reached such a degree when the hemorrhage ceases as to render the fixation of the ovum in it impossible or improbable. As for Williams' idea, that the uterine mucous membrane is totally destroyed and regenerated at each menstrual period, B. says that it can readily be applied to the old theory on ovulation and conception, as follows: During the destruction of the uterine mucous membrane and the hemorrhage, the follicle matures in the ovary, the ovum is discharged, passes slowly through the tube, and by the time coition and impregnation take place, the uterine mucous membrane has become regenerated and fully adapted to the reception of the fecundated ovum. If impregnation does not occur, of course the ovum is lost, and the mucous membrane goes on to its repeated destruction at the next menstrual period. B. still adheres to the old doctrine that, during the growth and maturation of the follicle, the uterine mucous membrane swells and develops, then the follicle bursts, the ovum is discharged, hemorrhage from the uterus occurs (and B. does not see why there should necessarily be a destruction or retrograde metamorphosis of the menstrual decidua to allow the hemorrhage), the ovum slowly traverses the tube where it is met by the spermatozoa (for which meeting there is plenty of time), and then passes on to become fixed in the unimpregnated menstrual or uterine decidua. It were strange if man should make the sole exception to the universal law existing in the animal kingdom, that coition and fecundation always relate to an ovum already detached from

the ovary, or in process of immediate discharge. B. thinks that many additional investigations and observations are necessary to justify us in departing from the doctrine of intimate connection between the various factors of generation : ovulation, formation of menstrual decidua, hemorrhage, coition and conception, in the order in which they are named. Our investigations should lead us to ascertain, if possible, 1. When, during so-called menstruation, the ovum is discharged from the ovary ; 2, how long a period it requires to pass through the Fallopian tube ; 3, how long it remains capable of impregnation. The extreme difficulty of answering these questions is obvious, and is an almost insurmountable obstacle to the solution of this problem.

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THE DISCHARGE OF OVA AND ITS RELATION TO THE TIME OF MENSTRUATION. By DR. JOHN WILLIAMS (*Proc. Royal Soc., No. 162, 1875*).

It is a recognized fact in physiology that ova are discharged in connection with the menstrual function, but it is uncertain at what time in the course of the month the separation takes place. It is generally understood to occur towards the end of the discharge or immediately after its cessation. Reichert, in Berlin, has examined 23 cases, in which the genital organs showed signs of menstruation. In one case a Graafian follicle had ruptured ; in 4, the follicle had matured before hemorrhage began, one of which follicles was on the point of rupture, and in 18 cases the follicle had actually ruptured and hemorrhage had taken place into the decidua menstrualis. Reichert concludes from these specimens, that the rupture of the Graafian follicle takes place at an early stage of the menstrual flow. Williams, however, believes, from observations made in 16 cases, that the rupture occurs, as a rule, before the appearance of the monthly flow with which it is connected ; for in 12 out of the 16 cases rupture of a follicle or hemorrhage into its cavity had occurred before the return of the catamenia ; in 1 it was doubtful whether rupture of a follicle or the appearance of the discharge would have taken place first ; in 2 a menstrual period had passed without maturation of a follicle, and in 1 a periodical discharge was imminent, though the ovaries contained no matured Graafian follicle. It is not improbable that the follicles which were found in the three last cases, and which were enlarged to the size of a pea, would have become mature by the next return of the flow. A careful consideration of the cases recorded by Cruikshank, Jones, Pater-

son. Lee, Girdwood, Negrier, Coste, and others, shows them to favor the view advanced by Dr. Williams, a view which derives support from the custom imposed by the Levitical law, and observed to this day by the stricter sect of the Hebrew community—a law requiring entire abstinence from sexual intercourse for ten days after the cessation of the menstrual flow.

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A CASE OF MEMBRANOUS DYSMENORRHOEA CURED BY ELECTRICITY.

By DR. ALEXANDER SOLOVIEFF, of Kazan. (*Arch. f. Gyn.* viii., 3.)

THE patient, 22 years of age, married, had been ill for five years, and when seen presented the following conditions: Hyperæsthesia and relaxation of the vagina, the folds of which had disappeared; the uterus anteфлекed, indurated; the fundus tender; *regular monthly expulsion of a menstrual decidua*; the cervical canal narrowed by the laxity of its mucous membrane; the genital organs easily irritated; the sensation of weight in the perineum and thighs; considerable leucorrhœa; locomotion impossible on account of the aggravation of these symptoms. The treatment consisted in local faradization, by means of one electrode over one or the other ovary, and the other in the uterine cavity during ten minutes, and for the next five minutes one electrode over the symphysis and the other in the vagina. A weak current was first used, and gradually increased until the finger in the vagina holding the internal electrode perceived a distinct contraction of the uterus and vagina; the occurrence of menorrhagia was thus avoided. The constant current was used, besides, over the sympathetic and the spinal column, 20 cells being employed, 12 for the spine and 8 for the sympathetic. After four months' uninterrupted treatment the condition of the patient, who had for years previously been subjected in vain to all the known remedies for her complaint, was the following: The hyperæsthesia and relaxation of the vagina have disappeared and the rugæ are distinct, because the canal has regained its normal tonicity; the uterus is of firm consistence, the fundus sensitive only to strong pressure, the anteфlexion unchanged; *the menstrual decidua has not appeared since the beginning of the treatment*; the sound readily enters the uterus up to the point of flexion; the abnormal sensations are so inconsiderable as to permit the patient to walk about without inconvenience; leucorrhœa almost gone. About a year later she became pregnant, and was delivered at term of a healthy child, which was followed by another within eighteen months. The menstrual decidua has

not reappeared, and menstruation has always been painless. S. pronounces this to be the first case of cure of membranous dysmenorrhœa which he has been able to discover. In two other cases in which S. employed electricity the treatment could not be carried on sufficiently long, but the hyperæsthesia of the genital organs was removed and the decidua much reduced in thickness.

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THE LOSS OF FUNCTION OF THE FEMALE URETHRA, AND THE CONSTRUCTION OF A NEW ONE OVER THE SYMPHYSIS. DR. RUTENBERG, of Strasburg (*Wiener Med. Woch.*, 37, 1875), after enumerating the various operations practised by Simon, Hegar, Baker Brown, Emmet and others, to close the urethra in desperate cases of urethro-vaginal fistula with greater or lesser loss of substance, and the devices employed to enable the patients to retain the urine in incurable incontinence from paralysis of the sphincter, irreparable injuries and loss of muscular fibres, and referring to the amount of distention which the female urethra will endure, and the ready restoration of its power after years of inactivity, says that there are still cases in which none of these measures will be effectual, and in which the patients appear doomed to go to their graves without relief. For such cases, R., acting on the observation that patients with fistulæ near the vesical neck are able to retain their urine for several hours while lying down, and have no retention whatever while standing, and patients with fistulæ near the mouths of the ureters present exactly the reverse condition, proposes to make an abdomino-vesical fistula immediately above the symphysis, and then to close the urethra if it still exist, or the original fistula. After this has been done, a sufficiently capacious cavity will have been found for the urine, which can either be voided every few hours by bending the body forward, or by means of a catheter, best an elastic catheter acting as a syphon. The danger attending the formation of such an abdomino-vesical fistula is very slight, for the peritoneum can easily be avoided by keeping close to the symphysis. The injurious consequences following stagnation of urine at the bottom of the bladder can be avoided by occasionally washing out the bladder with warm water. The constant wearing over the fistula of a pad, attached like a truss, will give the patient almost complete control of the artificial opening, and will be much more efficacious and less likely to be displaced than the vulvar or vaginal pads hitherto used to close the non-retaining normal or artificial urethra.

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ON THE DEVELOPMENT OF THE GRAAFIAN FOLLICLES IN THE OVARY OF THE NEW-BORN INFANT. By DR. DE SINETY (*Soc. de Biol. Annales de Gynécologie*, Sept., 1875).

UNTIL recently the ovary was thought to remain in a state of perfect repose until puberty, and Coste says that "during the first part of the existence of the female, that is, from birth until the time when she is capable of reproduction, the ova which are found in the ovary of the fœtus at birth live a latent existence, remain stationary, and do not really begin to develop until towards the age of puberty." Since the progress of histology has, however, enabled us to observe more correctly, the Graafian follicles in the infant have frequently been seen to undergo considerable development, and the ovaries of young immature girls to present cicatrices resulting from the disappearance of follicles. Waldeyer, in his treatise on the ovary, distinctly makes this statement; and the details of the process (which Waldeyer calls atresia of the follicle) have been studied by Slavjanski, of St. Petersburg, in two papers (*Virchow's Archiv*, 51, p. 470, and *Arch. de Physiol.*, 1874, p. 213). De Sinety has examined a large number of ovaries of infants of different ages, principally new-born (two of which were killed by cephalotripsy, and consequently perfectly fresh), and found that very frequently at the moment of birth there were Graafian follicles readily visible to the naked eye. Those most developed are always seen in the neighborhood of the hilus. At this period the vascular layer, which later becomes the medullary portion, is situated almost entirely outside of the ovary, and forms a species of pedicle supporting the ovary, like the stem of a mushroom supports its head. In this pedicle are found large vessels which barely enter the ovary proper, which latter contains, scattered about, ova in various stages of development. He quite frequently saw a certain number of Graafian follicles during the few days immediately following birth, acquire considerable dimensions, and represent a series of veritable cysts. In all these cysts ovules were invariably found, which left no doubt as to the origin of these productions. Below these cysts several cicatrices, more or less retracted, were observed, which proved that these hypertrophic follicles were capable of absorption. Is this growth on the part of the ovaries at the time of birth, connected in any way with the phenomena occurring at the same time in the mammary gland? De S. is inclined to admit such a relation, although the number of facts at his disposal does not authorize him in declaring it to be of constant occurrence. In the same manner as the congestion accompanying the production of a

lactiform fluid in the mammary gland of infants occasionally exceeds physiological limits, and gives rise to mastitis and mammary abscess, so this development of the Graafian follicles in certain cases assumes exaggerated proportions, and forms the cystic ovaries which are so often met with in very young children.

These enlarged follicles are no longer found a few years after birth, which fact de S. compares with the circumstance that the testicles of male infants at birth possess cells analogous to those in which subsequently the spermatozoa are developed, and that these cells disappear some time after birth, not to reappear until puberty. The new-born infants of both sexes also present a mammary secretion, which de S. has ascertained to be true milk.

Phenomena corresponding to these facts are the inflammations of the mammary gland in both sexes at the period of puberty, between which and the transformations occurring in the genital organs at that time there appears to be a distinct connection. The development of the Graafian follicles in infants is in accordance with the form and constitution of the ovary at different ages. The cicatrices resulting from the atresia of these follicles cause the connective tissue and the vessels to enter deeper and deeper into the ovary, and to push the layer containing the ovules towards the periphery. In this layer the Graafian follicles most distant from the surface, and consequently nearest the vascular tissues, are those which are the most developed. It would seem as though they receive more nourishment, and therefore live a more active life, both during infancy and the period of generative activity.

An examination of the ovaries from their first appearance during intra-uterine life to the moment of birth, shows that at that age there is no such thing as a division of the ovary into two portions, a cortical and a medullary. In the new-born infant, what subsequently becomes the cortical layer constitutes the whole ovary, and ova are found throughout its whole tissue. The future medullary substance is but rudimentary, and is represented by what may be termed the pedicle of the ovary

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ON THE SEPARATION OF THE UMBILICAL CORD. By DR. TSCHAMER, of Graz (*Jahrb. f. Kinderheilk.*, ix., 2).

THE only detailed description of the macroscopic changes taking place in the funis during its desiccation and separation has been furnished by Billard, who came to the conclusion that the "demarcation of the funis was the result of a constrict-

tion exerted by the dried gelatine at the navel on the umbilical vessels, and that the actual separation was produced by the traction acting from the external and internal surface of the abdomen on this constricted, dried and flexible portion of the cord, and that further suppuration was a merely accidental occurrence."

The process of separation of the other dead, desiccated and mummified tissues from their connection with the living substance does not, however, accord with Billard's description, and T. undertook to investigate the subject, by carefully measuring the circumference of the cord, the length of the stump and cutaneous navel, and the weight and length of 100 children immediately after birth, and daily examining the stump of each child before and after its bath. The results obtained were the following: After the ligation and division of the cord, any spiral turns which may chance to be present in the foetal portion disappear in consequence of the cessation of umbilical circulation, the cord loses its fulness, becomes flaccid, the lumina of its vessels appear retracted, but in reality are merely covered by the protruding gelatine. The process of desiccation commences after from twelve to thirty-six hours; it is more rapid in thin, lean cords, more gradual in those endowed with a large amount of gelatine; the color of the cord changes to a dark green; the cord becomes flat from the pressure of the abdominal bandage; desiccation, as a rule, progresses from the free end towards the umbilicus, and in the majority of cases is concluded on the third day. The umbilical ring is surrounded by radiating furrows, caused by the traction from the external amniotic envelope of the cord, and disappearing after the separation of the latter. Occasionally, if the cutaneous navel is more developed, the navel is depressed, inverted. Since the desiccation of the umbilical cord is a normal physiological process in the living child, it is of importance, in a medico-legal point of view, as a test whether the child was born alive or not; with a still-born child, or one which died soon after birth, the cord does not dry up, it putrefies. Only one case is on record in which this normal process of desiccation was not concluded on the fourth day; it is reported by Dr. Underwood, who explains the fact that only the portion beyond the ligature dropped off, the remainder merely shrinking somewhat, by the supposition that the latter was nourished by an anomalous vessel. After three weeks the cord was still living, and one and a half inches long.

Slightly previous to, or at the moment of complete desiccation, at the point of junction of the cord and the cutis appears generally in the whole circumference a swelling and redness--

a line of demarcation—which differs in extent, but is often over one line in breadth. This line of demarcation may escape detection by being concealed by the scab exuded from the base of the cord, or by the base itself, but it was present in all the 100 cases. In thick cords this inflammatory areola is more marked; in 3 cases, shoots of skin, 2–3 lines long, stretched up on the cord, and the inflammatory redness was deeper at these points, the shoots being destroyed by suppuration. When the enclosing amniotic sheath has once separated from the cutis, the more or less shrivelled connective tissue soon follows, and the cord remains attached only by its vessels, of which the vein is the most persistent, all of which, however, generally separate on the same or the next day. This constant areola of inflammation renders the separation of the cord by reactive inflammation a settled fact.

In the majority of cases T. found distinct pus-corpuscles at the line of demarcation, and explains their absence in some cases by the presence of a closely-fitting scab, by which the granulations are protected, and the formation of pus is prevented, the surface becoming covered with epithelium under the scab (Lister and Volkmann).

During the process of desiccation of the cord, its cells undergo fatty degeneration.

The average time of separation of the cord is four to five days. The thinner the cord, and the shorter the cutis-navel, the sooner does the separation occur, and *vice versâ*. The degree of development of the child in so far influences the separation of the cord, as in well developed infants a thick cord will separate more rapidly than a cord of equal size in a weaker child. After the separation the cicatrization completes the whole process. The time for its completion varies according to the size of the cord and the wound left after its separation, and the length of the cutis-navel. In thin cords, cicatrization is completed on the first or second day after separation; in thick cords a slight cavity is formed, in the centre of which is the stump of the vessels, with which the cutaneous margin gradually unites by granulation. At this stage, if the vessels were ranged in a row, as is the case in flat cords, the umbilical fissure assumes a longitudinal shape, whereas it is triangular, if the vessels were placed in that order, as they are in round cords. Finally, this fissure also unites and becomes covered with epidermis, leaving first a pale red, generally linear scar, rarely triangular (5 in these 100 cases), which gradually becomes firm. During cicatrization and subsequently, the retraction of the umbilical vessels forms the umbilical depression; the navel loses its circular shape, and assumes that

of two semi-circles, a large upper and a smaller lower, in consequence of the predominating retraction of the umbilical arteries. Once T. saw these two folds not directly one above the other, but diagonally, probably through stronger retraction of one umbilical artery.

#### ANALYSIS OF THE MAMMARY SECRETION (GALACTOSTASIS) OF A NEW-BORN INFANT.

DR. THEODOR VON GENSER (*Jahrb. f. Kinderheilk.*, ix., 2) collected about one drachm of the secretion accumulated in the breasts of a female infant two weeks of age, both of whose breasts were enlarged to the size of an English walnut, but not in the least inflamed, and subjected it to a thorough physical and chemical analysis: Reaction very alkaline, more so than is usual in woman's milk; under the microscope, fat-globules in some quantity and of variable size, besides quite a considerable number of colostrum-corpuscles, and a few scattered granular particles. The chemical analysis showed the following constituents in 1,000 parts:<sup>1</sup>

	Genser.	Gubler.	Mother's Milk (Robin).
Caseine . . . . .	5.57	26.40	Caseine . . . . . 29.000
Albumen . . . . .	4.90		Lacto-pepsine. 1.000
Milk-sugar . . . . .	9.56	62.20	Albumen, traces.
Butter . . . . .	14.56	14.00	Butter . . . . . 25.000
Salt . . . . .	8.26	3.40	Sugar of milk. 37.000
			Salts . . . . . 5.283
Total of solid parts.	42.95		Water . . . . . 902.717
Water . . . . .	957.05	894.00	
	1,000.00	1,000.00	1,000.000

Specific gravity, 1.01986.

THE HEREDITARY TRANSMISSION OF SYPHILIS. (*Die Vererbung der Syphilis.*) By DR. M. KASSOWITZ, Attending Physician to the I. Public Hospital for Children, in Vienna. Vienna, 1876. Wm. Braumüller. Pp. 137.

As the result of his investigations and observations, and after a review of the opinions of other specialists, K. sums up the various chapters of his book, as follows:

1. The disease known to us as hereditary syphilis can arise logically only in two ways: either that the transmitting agent,

<sup>1</sup> For comparison we have added another analysis of the infantile lacteal secretion, and one of human milk, both taken from Flint's Physiology, 1875.—ED. and REP.



containing the syphilitic virus (spermatozoon or ovum), carries in itself from the very first the conditions for the development of the disease; or that the foetus, although healthy from the time of conception, becomes infected through the nutritive substances which draw the virus from the maternal circulation. The facultative existence of both these heterogeneous processes with the same final result is exceedingly improbable. The former theory of the origin of congenital syphilis, the hereditary transmission *sui generis*, is decidedly the most probable.

2. The possibility and great frequency of the birth of syphilitic children of non-syphilitic mothers is conclusively proved by numerous cases.

3. A woman impregnated by a syphilitic man brings forth syphilitic children, but herself remains free from the disease, unless infected from without.

4. The syphilitic virus does not cross the septum between the foetal and maternal circulatory systems in the direction from the foetus to the mother.

5. A child, both of whose parents were not syphilitic at the time of conception, does not become syphilitic, even though its mother at any period of her pregnancy acquires constitutional syphilis. The latter may exert a deleterious influence on the continuation of gestation, but is never transmitted to the foetus.

6. The syphilitic virus therefore does not cross the septum between the foetal and maternal circulatory systems either in the direction from the mother to the foetus.

7. The power of transmission is wholly independent of the existence of tertiary syphilitic affections in the person transmitting the disease to his descendants; that is, it may still exist, or have been destroyed by mercurial treatment, or have disappeared voluntarily.

8. The transmission of the disease during a course of syphilis, the virulence of which has been weakened neither by thorough mercurial treatment nor by a long lapse of time, is a necessary result, and the immunity of the foetus, if it occurs at all, must be classed among the rarest exceptions.

9. The various degrees of intensity of inheritance of syphilis manifest themselves in three directions:

(a.) By the interruption of normal pregnancy by miscarriage or premature delivery.

(b.) By the disturbance of the vitality of the ovum.

(c.) In viable children by the time of the appearance of the visible symptoms.

As regards (a), the cause of the death of the foetus and the premature delivery is to be found principally and in most syphilitic premature labors in disease of the foetus itself.

As regards (*b*), of 350 children out of 119 syphilitic marriages observed by K.:

Exactly one-third were born dead.

Of those born alive an additional 24 per cent. died of the hereditary dyscrasia during the first half-year.

Thus, including the children born dead and those dying during the first six months, the sad result appears that of all children with inherited syphilis, 58 per cent., or nearly three-fifths, succumb to this hereditary taint, and only two-fifths escape death as the immediate consequence of their disease. Further.

During the first three years of an undisturbed, untreated parental syphilis the birth of a viable child is a rare exception.

As regards (*c*), the eruption of an exanthema as late as the third month after birth may be considered a sign of the waning transmissibility of the syphilitic infection.

10. Finally, the transmission of syphilis from the parents to the child through the spermatozoon or the ovum always produces the strikingly characteristic and unmistakable symptoms of hereditary syphilis, and can produce no others.

## REVIEWS AND NOTICES OF BOOKS.

CYCLOPÆDIA OF THE PRACTICE OF MEDICINE. Edited by DR. H. VON ZIEMSEN. VOL. X. DISEASES OF THE FEMALE SEXUAL ORGANS. By PROF. CARL SCHROEDER, Erlangen, Bavaria. Translated by Drs. E. W. SCHAUFFLER, L. WHEELER, W. L. RICHARDSON, E. B. BRONSON, J. H. EMERSON and PAUL F. MUNDÉ. A. H. BUCK, M.D., New York, Editor of American Edition. Wm. Wood & Co., New York. Pp. 570. 1875.

WE must confess that our first feeling after reading this book was one of disappointment. The title of the work, the exhaustive character of the previous volumes of the series, and, above all, the distinguished reputation of the author, had greatly raised our expectations. On reflection, however, we feel that we ought not to be disappointed. We ought not to expect in a volume of 550 pages a full and complete exposition of a subject about which several such volumes might be written.

Neither ought we to expect to be perfectly satisfied with the

views of any foreign writer, on a subject which is confessedly best understood and practised at home.

Prof. Schroeder has made good use of his space, and has given us much that we needed to complete or fill out our knowledge of the subject. Certainly the pathological portion of the book is unexcelled. No book that we know of contains such an excellent description of the pathological anatomy of uterine diseases, and thus it comes as a complement to our own more practical, but less scientific works. This is what might be naturally expected from the acknowledged pre-eminence of the Germans as pathologists.

As a text-book for practitioners in this country, this work is very far inferior to our own and the English works. Evidently the German methods of treating the ordinary uterine diseases, if we accept this as a fair exposition of their status, are very much behind those employed in this country.

In one very important class of diseases, that of uterine displacements, the treatment advocated by Schroeder can certainly bear no comparison to that taught us by Hodge, Sims and Thomas. The operative procedures are not always those which experience has shown our surgeons to be the best; and their descriptions are so vague that to a beginner they would be worse than useless.

The book begins with a very sensible chapter on gynecological examination, in which the advantages of bimanual palpation are very strongly set forth. A preference is given for the old glass tubular speculum, which is so largely used in Germany for ordinary examination. Simon's instruments are advised for operations, Sims' speculum being evidently but partially appreciated. The reason why this instrument is not more generally used abroad, we think is the failure to secure the proper position of the patient, the side-position being generally used instead of the semi-prone, or true Sims' position. Another reason undoubtedly is the necessity of an assistant; though this necessity is entirely done away with by the very excellent modification proposed by Dr. Thomas.

Our author is evidently unacquainted with that invaluable aid to diagnosis, Sims' probe, against which, when properly used, viz., introduced through a Sims' speculum, very few of the objections can be urged which are generally made against the uterine sound. The author's opinion as to the frequency with which the sound should be used, is best gathered from the following: "Some gynecologists employ it almost without exception in every case; others, however, use it comparatively seldom, only when they expect to derive some especial, not otherwise obtainable information. I must confess that I belong to the latter class."

The use of rapid dilatation, by means of dilating instruments in place of tents, is decried.

The article on Uterine Malformation is preceded by a short account of the embryonic development of this organ, which helps very much to explain the anomalies of development. This plan is also very fully carried out, when speaking of vaginal malformation, and is a very good feature.

The views advanced on the subject of hypertrophy of the cervix are good, and this chapter is certainly very instructive.

The author's opinions on chronic metritis, to which name he holds, not having a new one to propose, are best told in his own words :

"My conviction is that we cannot dispense with the clinical picture of chronic metritis, for we should otherwise be obliged to separate closely-connected pathological conditions, having the same symptoms, and requiring the same treatment. Neither do I consider the term 'Chronic Metritis' to be very improper, because it is probably nothing more than a war of words, whether that condition be called a hyperplasia of the connective tissue of a hyperæmic uterus, or the product of an exceedingly chronic inflammation.

"Indeed, I should be loth to dispense with the name 'inflammation' for this very condition, partly because the treatment needs to be decidedly antiphlogistic, and partly because in the early stages we always have the clinical symptoms of inflammation—hyperæmia, tumefaction, and pain."

"We therefore include under the term 'chronic metritis' these cases also—placing them at the head of the list, because they are the most numerous—which originally arise independently of inflammation, such as defective puerperal involution, because inflammatory symptoms—hyperæmia, swelling, and pain—occur in their course, and also because the treatment of those etiologically separate cases is decidedly antiphlogistic." The main reason, then, for holding to the name is that the treatment should be antiphlogistic. The etiology he holds to be of different natures, though the clinical appearances and treatment are the same.

The clinical picture of the disease is strikingly wanting in all the clearness of description and vividness of representation, which is so valuable a feature of some of our English writings.

In fact, this fault is very prominent throughout the book, the results of labor in the post-mortem rooms and laboratory being much more prominent than those derived from observation at the examination table and the operating room.

In the treatment of chronic metritis, local depletion by scarification occupies the foremost place, the scarification to be frequently repeated. Cold is less frequently applicable as an antiphlogistic. A systematic and rational course of treatment at a watering place is very strongly recommended, and a long list of springs, noted for curing what the doctors can't cure, is given, but is of more interest to German than to American practitioners. The douche is spoken of with faint praise, and the use of iodine is referred to as being recommended by others.

This, with the use of rest and directions as to keeping the bowels open, and some other hygienic rules, comprise about all the medical treatment of this very obstinate affection. Not a word about the necessity of relieving displacements, if any exist, or of treating endometritis. Neither is the state of the secretions once mentioned in the whole chapter, but the reader is referred to the chapter on endometritis, etc., for information on these points.

The diagnosis of corporeal metritis, first premising that the author holds that corporeal endometritis uncomplicated by cervical inflammation is very rare, is stated as follows: "If upon examination with the speculum we find the cervix but slightly diseased, and at the same time a copious watery discharge from the os, then the whole lining membrane of the uterus is diseased. Whether the parts principally affected can be distinguished by the sensitiveness to the touch of the sound, seems doubtful."

But how, then, are we to diagnose those cases where the cervix is very decidedly implicated?

Intra-uterine injections are favorably considered in the treatment of this affection, as being the surest means we have of producing a uniform effect upon the uterine mucous membrane.

Laceration of the cervix uteri, so well described by Emmet, is nowhere mentioned, but ectropion of the os, the "inversion of the canal of the cervix uteri" of Tyler Smith, is still evidently confounded with it.

We heartily recommend the careful perusal of the chapter on Ulceration of the Cervix to that large class of practitioners who find "ulceration of the womb" in every case of uterine disease. The chapter on Follicular Hypertrophy of the Cervix is very interesting. We do not remember to have seen a case similar to the one described, in any recent English literature.

We had scarcely expected any very great addition to our stock of information on the treatment of uterine displacements, but we were scarcely prepared for the assertions here met with, assertions which are daily contradicted in our own practice.



The treatment of this important class of uterine disorders has certainly not made any very great advance in Germany, notwithstanding the earnest efforts of a few able writers. It was our privilege to see a Hodge pessary introduced upside down and wrong end foremost, for the relief of a retroversion, with the result of finally discarding the instrument as worthless; and this not by an ignorant practitioner, but in one of the most celebrated clinics in Germany.

But before going any further, we would like to know on what anatomical principles German women are made. Certainly, if the figure of the normal position of the uterus, on page 159, is after nature, nature has played some queer pranks on the other side of the water. We can only account for it by supposing that the drawings were taken from the body of a condemned criminal, in which the act of hanging had caused a dislocation of the whole vertebral column upwards, and a prolapse of the pelvic contents. A line drawn from the symphysis to the coccyx cuts off the lower margin of the uterus, nearly the whole of the vagina, the whole of the urethra, and about three inches of the rectum. The perineum looks as if it could never be ruptured, and the occurrence of a vesicovaginal fistula is an apparent impossibility. Surely Professor Schroeder cannot in sober earnest ask any one to accept this as the normal relation of the parts; and yet all the illustrations in the book are drawn on the same model, some being even more ridiculous.

The use of vaginal pessaries meets with no favor in the eyes of our author. Neither is he willing to admit the principle of leverage as applied to them. He holds that a Hodge acts by stretching (from side to side?) the posterior cul-de-sac, and thus drawing the cervix backward. In reference to anteversion pessaries, he says: "Since the idea of elevating the body of the flexed uterus by lever-power working within the vagina is an entirely mistaken one, we must condemn the form of pessary proposed by Graily Hewitt. It is incorrect in principle, and experience shows that it is not well borne." The same will therefore apply to those used by Thomas, they acting on a similar plan. All we can say is, come and see.

He advocates the use of the intra-uterine stems to correct the flexion and cotton tampons to correct the version. Surely a very good arrangement for filling the doctor's pocket if not the patient's vagina. But of the use of the stem he says, "The cases, then, which are adapted to the intra-uterine method of treatment are the pure flexions, uncomplicated by inflammatory processes." "But we must keep the fact in mind that this displacement, as such, does not admit of a permanent cure."

The article on prolapsus is quite full, but contains little new. The description of the operations for its relief is very meagre. Inversion is quickly and quite satisfactorily disposed of. With the article on fibroids the interest of the book really commences, and we heartily recommend this and the succeeding chapters on cancer and sarcoma to those interested in the subject. It is by far the best description of the pathology of these affections which we have seen, and, after the hopeless muddle into which some authors have got the subject, is very refreshing reading. We are surprised at the light estimation put upon the ergot treatment of fibroids. Certainly the results thus far reported made a better showing than is here given.

The theory of normal menstruation is based on the researches of Kundrat and Engelmann, and is a very clear exposition of the subject from their standpoint. The articles on abnormal menstruation are by far too short and unsatisfactory. The diseases of the tubes and ovaries are next considered. The pathology of ovarian cysts is that advanced by Waldeyer. He holds that they are "all formed according to one original plan," viz.: "The dropsy of the Graafian follicles, and the cyst tumor or cystoma." "While the latter is to be regarded as a glandular new formation (adenoma) with a secondary cystic formation, which arises from the follicle of the ovary, the dropsy of the Graafian follicles represents a so-called retention cyst." According to Waldeyer, "Cystomata are really epithelial tumors, that is, they are formed from the superficial or glandular epithelium, and this formation proceeds from the actual parenchyma of the ovary, or, in other words, from its glandular structure."

These views differ essentially from those of many other writers on ovarian pathology, but are certainly very satisfactory, and have been worked out with great pains and care. The chapter on ovariectomy is not as good as some others. It lacks the authoritative dicta of one who speaks from extended experience.

In regard to drainage he holds that "the possibility of being able to remove an intra-peritoneal exudation which has accumulated after the operation, is of greatest importance," and so, "if it is thought desirable, at the time of the operation, to make provision in the safest manner possible for washing out the abdominal cavity, and for drainage on any exudation which may accumulate, the plan recommended by Munsich, of introducing short silken threads, or that devised by Nussbaum, of using a drainage tube, may be considered the best." These are to be passed from the abdominal wound through the cul-de-sac of Douglas, and out of the vagina.

As to the much vexed question of peri-versus parametritis,

he says. "I am of the opinion that parametritis is a connective-tissue phlegmon which is due to an infection with septic material. Hence, that it is common in the puerperal state, but at other times is tolerably rare, and that perimetritis, or pelveo-peritonitis, as he calls it, is a partial peritonitis which may be, and frequently is, induced by the most diverse causes." Not much light is thrown on the differential diagnosis of the two affections, though "if on puncturing an elastic pelvic exudation a stream of thin serum is obtained, the exudation invariably pertains to perimetritis." Schroeder's views on retro-uterine hæmatocele are already sufficiently well known. The volume concludes with an account of the diseases of the vagina and vulva. The arrangement of the book is clear and the style good. The translation is generally good, and the get-up of the book all that could have been desired.

We are led to wonder, by the great excellence of parts of the book, why Dr. von Ziemssen did not pursue the same plan as that followed in the other volumes, and employ a corps of different writers. We might thus have had a work which would have surpassed anything which has yet been written on the subject, and have been an honor to German industry and science. We must add that American literature is very well represented in the lists of literature at the beginning of each chapter, but it does not seem to have made as much impression as it might have done.

M. D. M.

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A SYSTEM OF MIDWIFERY, INCLUDING DISEASES OF PREGNANCY AND THE PUERPERAL STATE. By WILLIAM LEISHMAN, M.D., Regius Professor of Midwifery in the University of Glasgow, etc., etc. Second American from the Second and Revised English Edition, with additions by JOHN S. PARRY, M.D., Obstetrician to the Philadelphia Hospital, etc. Phila.: Henry C. Lea. 1875. Pp. 766.

THIS new edition decidedly confirms the opinion which we expressed of the first edition of the work in the May, 1874, number of this JOURNAL, that this is "the best modern work on the subject in the English language." We are pleased to record also, that, besides being thoroughly revised, the new edition shows an evident increase of familiarity with foreign current literature, and a growing appreciation of the new researches in the physiology of parturition, the almost total lack of which we took occasion to notice two years ago. The chapters relating to embryology and the physiology of parturition, and those on puerperal fever, have been entirely remodelled and largely improved. In the latter disease, Dr. L. has taken a firm stand,

and expresses himself strongly in favor of the generally accepted septicæmic theory, and opposed to the specific poison theory of puerperal fever. The excellent practical notes contributed by Dr. Parry refer principally to the use of the forceps, lactation and the puerperal diseases, and are intended to increase the usefulness of the work in this country. An entirely new chapter on diphtheria of puerperal wounds has been added (Dr. P. having had unusual experience in this form of puerperal fever), and also a number of illustrations of the principal obstetrical instruments in use in America. We have no hesitation in saying that the work in its present shape is a great improvement on its predecessor, and in recommending it as the one obstetrical text-book which we should advise every English-speaking practitioner and student to buy.

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EXTRA-UTERINE PREGNANCY : ITS CAUSES, SPECIES, PATHOLOGICAL ANATOMY, CLINICAL HISTORY, DIAGNOSIS, PROGNOSIS, AND TREATMENT. By JOHN S. PARRY, M.D., Obstetrician to the Philadelphia Hospital, etc. Philadelphia: Henry C. Lea. 1876. Pp. 276.

THE title of this monograph indicates its scope and purpose. It is, so far as we know, the most complete work on the subject, and is based on the analysis of 500 cases, collected by the author with the view of throwing some light on the natural history of the accident, and deciding on the proper methods of treatment. The book is one of exceeding interest, and the copious references on almost every page show the extent of the literary researches which the writer must have made in preparing it. Each chapter treats of the part of the subject to which it is devoted in the most thorough manner possible, without being in the least tedious. We regret that the nature of the subject and the multiplicity of the points proposed and conclusions reached, positively preclude a detailed discussion of its individual merits; we have derived much pleasure from a, as yet, scarcely sufficiently careful review of its pages, and congratulate the author on the successful accomplishment of a doubtless often wearisome task.

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A TREATISE ON HUMAN PHYSIOLOGY. By JOHN C. DALTON, M.D., Prof. of Physiology and Hygiene in the College of Physicians and Surgeons, New York, etc., etc. Sixth edition, revised and enlarged, with 316 illustrations. Philadelphia: Henry C. Lea. 1875. Pp. 825.

THIS old and valued friend comes to us again in a still recognizable but much improved and enlarged form. By the sub-

stitution of smaller type, the accommodation of at least 50 per cent. more matter has become possible without unduly swelling the volume. Although every chapter has been revised in accordance with the latest researches in physiology, those treating of physiological chemistry, the nervous system, and reproduction and generation, have been especially improved and added to. Indeed, the last-named chapters are not surpassed, perhaps not equalled, in any physiological work we have seen. As we know well from personal experience, Dalton is essentially the text-book for the student; from no other can he learn physiology so pleasantly and thoroughly. Its world-wide reputation needs no comment from our pen.

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A TEXT-BOOK OF HUMAN PHYSIOLOGY. By AUSTIN FLINT, Jr., M.D., Prof. of Physiology and Physiological Anatomy in the Bellevue Hospital Medical College, New York, etc., etc. Illustrated by 3 lithographic plates and 313 woodcuts. New York: D. Appleton & Co., 549 and 551 Broadway. 1876. Pp. 978.

ALTHOUGH this work is "designed for the use of practitioners and students" both, it seems to us that it differs from Dalton in being more specially adapted to the requirements of the practitioner. The comparatively small type—considerably smaller than that of Dalton—enables the author to bring into its 978 large pages much more matter than is found in the other work mentioned, or than the average student requires. The chapters on generation cover at least 100 pages of the book, and treat of the subject in a somewhat different manner, with generally different illustrations from those used by Dalton. One section, which we do not recollect seeing, as such, in any scientific work, is that on coition, physiologically considered (action of the male, action of the female, course of the spermatozoa through the female generative passages, mechanism of fecundation). The illustrations are good and almost, without exception, different from those contained in Dalton. If we were asked, which of these two works, Dalton's or Flint's, we should prefer as a standard work on physiology, we confess we should almost find ourselves in a quandary, and be obliged to refer the questioner either to his own individual preference or to the opinion which we expressed at the beginning of this review.

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CYCLOPEDIA OF THE PRACTICE OF MEDICINE. Edited by DR. H. VON ZIEMESSEN. Vol. V. DISEASES OF THE RESPIRATORY ORGANS. By PROFS. JUERGENSEN, HERTZ, RUEHLE and RIND-



FLEISCH. Translated by Drs. G. F. YEO, A. BRAYTON BALL, FRANCIS DELAFIELD, FRANK P. FOSTER, EDWARD FRAENKEL, JOHN C. JAY, and EDWARD W. SCHAUFFLER. ALBERT H. BUCK, M.D., New York, Editor of American Edition. New York: William Wood & Co. 1875. Pp. 712.

THIS is the sixth volume of the American edition, Volume X. having been published out of the regular order. As regards the excellence of the subject-matter and the typographical features, this volume fully equals the previous numbers of the series.

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PRACTICAL TREATISE ON FRACTURES AND DISLOCATIONS. By FRANK HASTINGS HAMILTON, A.M., M.D., LL.D., Surgeon to Bellevue Hospital, etc. Fifth edition, revised and improved. With 344 woodcuts. Philadelphia: Henry C. Lea. 1875. Pp. 831.

THERE is no better work on the subject in existence than that of Dr. Hamilton. It should be in the possession of every general practitioner and surgeon.

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PHTHISIS: THE MORBID ANATOMY, ETIOLOGY, SYMPTOMATIC EVENTS AND COMPLICATIONS, FATALITY AND PROGNOSIS, TREATMENT AND PHYSICAL DIAGNOSIS. In a series of clinical studies. By AUSTIN FLINT, M.D., Prof. in Bellevue Hosp. Med. Coll., etc., etc. Phila.: Henry C. Lea. 1875. Pp. 446.

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THE CHOLERA EPIDEMIC OF 1873 IN THE UNITED STATES. Washington: Government Printing-Office, 1875. Pp. 1,025, containing:

1. Introduction of Epidemic Cholera through the Agency of the Mercantile Marine. By John M. Woodworth, M.D., U.S.A. Pp. 28.
2. History of the Cholera Epidemic of 1873 in the U. S. By Ely McClellan, M.D., U.S.A. Pp. 513.
3. The Travels of Asiatic Cholera (Asia, Europe, and North America). By Jno. C. Peters, M.D., and Ely McClellan, M.D. Pp. 192.
4. The Bibliography of Cholera. By Jno. S. Billings, M.D., U.S.A. Pp. 320.

THIS second notice of a work, the principal portion of which is from the pen of Dr. McClellan, is a proper correction of the previous inadvertently inaccurate notice in the November number.

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A SERIES OF CLINICAL LECTURES. Edited by E. C. SEGUIN, M.D. Volume I. New York: G. P. Putnam's Sons. 1875.

- No. 1. On Disease of the Hip-joint. By Lewis A. Sayre, M.D. Pp. 24.
- No. 2. Acute Rheumatism in Infancy and Childhood. By A. Jacobi, M.D. Pp. 38.
- No. 3. Pneumo-Thorax. By Austin Flint, Sr., M.D. Pp. 19.
- No. 4. Rest in the Treatment of Nervous Disease. By S. Weir Mitchell, M.D. Pp. 19.
- No. 5. On the Treatment of Sciatica. By W. H. Thompson, M.D. Pp. 32.
- No. 6. Otitis. By C. R. Agnew, M.D. Pp. 22.
- No. 7. Capillary Bronchitis of Adults. By Calvin Ellis, M.D. Pp. 36.
- No. 8. The Inflammatory Origin of Phthisis. By Jas. H. Hutchinson, M.D. Pp. 21.
- No. 9. Peritonitis. By Alfred L. Loomis, M.D. Pp. 31.
- No. 10. Gleet and its Relations to Urethral Stricture. By Fessenden N. Otis, M.D. Pp. 22.
- No. 11. On the Diagnosis of Diseases accompanied with Red or Apparent Paraplegia, without marked Muscular Degeneration. By H. C. Wood, Jr., M.D. Pp. 27.
- No. 12. On the Nature of the Gouty Vice; Its Manifestations and Treatment. By W. H. Draper, M.D.

THESE twelve numbers comprise the volume for 1875. The evident popularity of the series, so far, is not surprising, in view of the scientific prominence of the gentlemen who have contributed to Vol. I.; it is also attested by the announcement of the publishers that subscriptions for Vol. II., at \$3.50 each, will be received, the first numbers of Vol. I. having been sold separately, as they were issued each month, until the growing favor of the enterprise induced this sale by subscription. The typography of the Lectures is excellent, and their general appearance very creditable to the publishers.

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COMMUNICATIONS HAVE BEEN RECEIVED from DRS. NATHAN ALLEN, Lowell, Mass., on "The Normal Standard of Woman for Propagation, physiologically discussed from four heads: Effects of Pregnancy, Mechanism of Labor, Lactation, and Character of Offspring;" WM. GOODELL, Philadelphia, on "The Mechanism of Natural and of Artificial Labor in Narrow Pelvis;" L. G. W. LIMPert, New York, on "Electro-therapeutical Observations in Gynecological Practice;" M. J. MOSES, New York, on "The Mechanism of Perineal Laceration;" A. JACOBI, New York, on "Masturbation and Hysteria in Young Children" (conclusion); T. CURTIS SMITH, Middleport, Ohio, on "Pelveo-Peritonitis."

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